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## THE DEMISE OF THE DEMARCATION PROBLEM \*

### 1. INTRODUCTION

We live in a society which sets great store by science. Scientific 'experts' play a privileged role in many of our institutions, ranging from the courts of law to the corridors of power. At a more fundamental level, most of us strive to shape our beliefs about the natural world in the 'scientific' image. If scientists say that continents move or that the universe is billions of years old, we generally believe them, however counter-intuitive and implausible their claims might appear to be. Equally, we tend to acquiesce in what scientists tell us not to believe. If, for instance, scientists say that Velikovsky was a crank, that the biblical creation story is hokum, that UFOs do not exist, or that acupuncture is ineffective, then we generally make the scientist's contempt for these things our own, reserving for them those social sanctions and disapprobations which are the just deserts of quacks, charlatans and con-men. In sum, much of our intellectual life, and increasingly large portions of our social and political life, rest on the assumption that we (or, if not we ourselves, then someone whom we trust in these matters) can tell the difference between science and its counterfeit.

For a variety of historical and logical reasons, some going back more than two millennia, that 'someone' to whom we turn to find out the difference usually happens to be the philosopher. Indeed, it would not be going too far to say that, for a very long time, philosophers have been regarded as the gatekeepers to the scientific estate. They are the ones who are supposed to be able to tell the difference between real science and pseudo-science. In the familiar academic scheme of things, it is specifically the theorists of knowledge and the philosophers of science who are charged with arbitrating and legitimating the claims of any sect to 'scientific' status. It is small wonder, under the circumstances, that the question of the nature of science has loomed so large in Western philosophy. From Plato to Popper, philosophers have sought to identify those epistemic features which mark off science from other sorts of belief and activity.

Nonetheless, it seems pretty clear that philosophy has largely failed to deliver the relevant goods. Whatever the specific strengths and deficiencies of

the numerous well-known efforts at demarcation (several of which will be discussed below), it is probably fair to say that there is no demarcation line between science and non-science, or between science and pseudo-science, which would win assent from a majority of philosophers. Nor is there one which *should* win acceptance from philosophers or anyone else; but more of that below.

What lessons are we to draw from the recurrent failure of philosophy to detect the epistemic traits which mark science off from other systems of belief? That failure might conceivably be due simply to our impoverished philosophical imagination; it is conceivable, after all, that science really is *sui generis*, and that we philosophers have just not yet hit on its characteristic features. Alternatively, it may just be that there are no epistemic features which all and only the disciplines we accept as 'scientific' share in common. My aim in this paper is to make a brief excursion into the history of the science/non-science demarcation in order to see what light it might shed on the contemporary viability of the quest for a demarcation device.

## 2. THE OLD DEMARCATIONIST TRADITION

As far back as the time of Parmenides, Western philosophers thought it important to distinguish knowledge (*episteme*) from mere opinion (*doxa*), reality from appearance, truth from error. By the time of Aristotle, these epistemic concerns came to be focussed on the question of the nature of *scientific* knowledge. In his highly influential *Posterior Analytics*, Aristotle described at length what was involved in having scientific knowledge of something. To be scientific, he said, one must deal with causes, one must use logical demonstrations, and one must identify the universals which 'inhere' in the particulars of sense. But above all, to have science one must have *apodictic certainty*. It is this last feature which, for Aristotle, most clearly distinguished the scientific way of knowing. What separates the sciences from other kinds of beliefs is the infallibility of their foundations and, thanks to that infallibility, the incorrigibility of their constituent theories. The first principles of nature are directly intuited from sense; everything else worthy of the name of science follows demonstrably from these first principles. What characterizes the whole enterprise is a degree of certainty which distinguishes it most crucially from mere opinion.

But Aristotle sometimes offered a second demarcation criterion, orthogonal to this one between science and opinion. Specifically, he distinguished between know-how (the sort of knowledge which the craftsman and the

engineer possess) and what we might call 'know-why' or demonstrative understanding (which the scientist alone possesses). A shipbuilder, for instance, knows how to form pieces of wood together so as to make a seaworthy vessel; but he does not have, and has no need for, a syllogistic, causal demonstration based on the primary principles or first causes of things. Thus, he needs to know that wood, when properly sealed, floats; but he need not be able to show by virtue of what principles and causes wood has this property of buoyancy. By contrast, the scientist is concerned with what Aristotle calls the "reasoned fact"; until he can show why a thing behaves as it does by tracing its causes back to first principles, he has no scientific knowledge of the thing.

Coming out of Aristotle's work, then, is a pair of demarcation criteria. Science is distinguished from opinion and superstition by the certainty of its principles; it is marked off from the crafts by its comprehension of first causes. This set of contrasts comes to dominate discussions of the nature of science throughout the later Middle Ages and the Renaissance, and thus to provide a crucial backdrop to the re-examination of these issues in the seventeenth century.

It is instructive to see how this approach worked in practice. One of the most revealing examples is provided by pre-modern astronomy. By the time of Ptolemy, mathematical astronomers had largely abandoned the (Aristotelian) tradition of seeking to derive an account of planetary motion from the causes or essences of the planetary material. As Duhem and others have shown in great detail,<sup>1</sup> many astronomers sought simply to correlate planetary motions, independently of any causal assumptions about the essence or first principles of the heavens. Straightaway, this turned them from scientists into craftsmen.<sup>2</sup> To make matters worse, astronomers used a technique of *post hoc* testing of their theories. Rather than deriving their models from directly-intuited first principles, they offered hypothetical constructions of planetary motions and positions and then compared the predictions drawn from their models with the observed positions of the heavenly bodies. This mode of theory testing is, of course, highly fallible and non-demonstrative; and it was known at the time to be so. The central point for our purposes is that, by abandoning a demonstrative method based on necessary first principles, the astronomers were indulging in mere opinion rather than knowledge, putting themselves well beyond the scientific pale. Through virtually the whole of the Middle Ages, and indeed up until the beginning of the seventeenth century, the predominant view of mathematical astronomy was that, for the reasons indicated, it did not qualify as a 'science'.

(It is worth noting in passing that much of the furor caused by the astronomical work of Copernicus and Kepler was a result of the fact that they were claiming to make astronomy 'scientific' again.)

More generally, the seventeenth century brought a very deep shift in demarcationist sensibilities. To make a long and fascinating story unconscionably brief, we can say that most seventeenth century thinkers accepted Aristotle's first demarcation criterion (viz., between infallible science and fallible opinion), but rejected his second (between know-how and understanding). For instance, if we look to the work of Galileo, Huygens or Newton, we see a refusal to prefer know-why to know-how; indeed, all three were prepared to regard as entirely scientific, systems of belief which laid no claim to an understanding grounded in primary causes or essences. Thus Galileo claimed to know little or nothing about the underlying causes responsible for the free fall of bodies, and in his own science of kinematics he steadfastly refused to speculate about such matters. But Galileo believed that he could still sustain his claim to be developing a 'science of motion' because the results he reached were, so he claimed, infallible and demonstrative. Similarly, Newton in *Principia* was not indifferent to causal explanation, and freely admitted that he would like to know the causes of gravitational phenomena; but he was emphatic that, even without a knowledge of the causes of gravity, one can engage in a sophisticated and *scientific* account of the gravitational behavior of the heavenly bodies. As with Galileo, Newton regarded his non-causal account as 'scientific' because of the (avowed) certainty of its conclusions. As Newton told his readers over and again, he did not engage in hypotheses and speculations: he purported to be deriving his theories directly from the phenomena. Here again, the infallibility of results, rather than their derivability from first causes, comes to be the single touchstone of scientific status.

Despite the divergence of approach among thinkers of the seventeenth and eighteenth centuries, there is widespread agreement that scientific knowledge is apodictically certain. And this consensus cuts across most of the usual epistemological divides of the period. For instance, Bacon, Locke, Leibniz, Descartes, Newton and Kant are in accord about this way of characterizing science.<sup>3</sup> They may disagree about how precisely to certify the certainty of knowledge, but none quarrels with the claim that science and infallible knowledge are co-terminous.

As I have shown elsewhere,<sup>4</sup> this influential account finally and decisively came unraveled in the nineteenth century with the emergence and eventual triumph of a *fallibilistic* perspective in epistemology. Once one accepts, as

most thinkers had by the mid-nineteenth century, that science offers no apodictic certainty, that all scientific theories are corrigible and may be subject to serious emendation, then it is no longer viable to attempt to distinguish science from non-science by assimilating that distinction to the difference between knowledge and opinion. Indeed, the unambiguous implication of fallibilism is that there is no difference between knowledge and opinion: within a fallibilist framework, scientific belief turns out to be just a species of the genus opinion. Several nineteenth century philosophers of science tried to take some of the sting out of this *volte-face* by suggesting that scientific opinions were more probable or more reliable than non-scientific ones; but even they conceded that it was no longer possible to make infallibility the hallmark of scientific knowledge.

With certainty no longer available as the demarcation tool, nineteenth century philosophers and scientists quickly forged other tools to do the job. Thinkers as diverse as Comte, Bain, Jevons, Helmholtz and Mach (to name only a few) began to insist that what really marks science off from everything else is its *methodology*. There was, they maintained, something called 'the scientific method'; even if that method was not fool-proof (the acceptance of fallibilism demanded that concession), it was at least a better technique for testing empirical claims than any other. And if it did make mistakes, it was sufficiently self-corrective that it would soon discover them and put them right. As one writer remarked a few years later: "if science lead us astray, more science will set us straight".<sup>5</sup> One need hardly add that the nineteenth century did not invent the idea of a logic of scientific inquiry; that dates back at least to Aristotle. But the new insistence in this period is on a fallible method which, for all its fallibility, is nonetheless superior to its non-scientific rivals.

This effort to mark science off from other things required one to show two things. First, that the various activities regarded as science utilized essentially the same repertoire of methods (hence the importance in the period of the so-called thesis of the 'unity of method'); secondly, the epistemic credentials of this method had to be established. At first blush, this program of identifying science with a certain technique of inquiry is not a silly one; indeed, it still persists in some respectable circles even in our time. But the nineteenth century could not begin to deliver on the two requirements just mentioned because there was no agreement about what the scientific method was. Some took it to be the canons of inductive reasoning sketched out by Herschel and Mill. Others insisted that the basic methodological principle of science was that its theories must be restricted to observable

entities (the nineteenth century requirement of '*vera causa*').<sup>6</sup> Still others, like Whewell and Peirce, rejected the search for *verae causae* altogether and argued that the only decisive methodological test of a theory involved its ability successfully to make surprising predictions.<sup>7</sup> Absent agreement on what 'the scientific method' amounted to, demarcationists were scarcely in a position to argue persuasively that what individuated science was its method.

This approach was further embarrassed by a notorious set of ambiguities surrounding several of its key components. Specifically, many of the methodological rules proposed were much too ambiguous for one to tell when they were being followed and when breached. Thus, such common methodological rules as "avoid *ad hoc* hypotheses", "postulate simple theories", "feign no hypotheses", and "eschew theoretical entities" involved complex conceptions which neither scientists nor philosophers of the period were willing to explicate. To exacerbate matters still further, what most philosophers of science of the period offered up as an account of 'the scientific method' bore little resemblance to the methods actually used by working scientists, a point made with devastating clarity by Pierre Duhem in 1908.<sup>8</sup>

As one can see, the situation by the late nineteenth century was more than a little ironic. At precisely that juncture when science was beginning to have a decisive impact on the lives and institutions of Western man, at precisely that time when 'scientism' (i.e., the belief that science and science alone has the answers to all our answerable questions) was gaining ground, in exactly that quarter century when scientists were doing battle in earnest with all manner of 'pseudo-scientists' (e.g., homeopathic physicians, spiritualists, phrenologists, biblical geologists), scientists and philosophers found themselves empty-handed. Except at the rhetorical level, there was no longer any consensus about what separated science from anything else.

Surprisingly (or, if one is cynically inclined, quite expectedly), the absence of a plausible demarcation criterion did not stop *fin de siècle* scientists and philosophers from haranguing against what they regarded as pseudo-scientific nonsense (any more than their present-day counterparts are hampered by a similar lack of consensus); but it did make their protestations less compelling than their confident denunciations of 'quackery' might otherwise suggest. It is true, of course, that there was still much talk about 'the scientific method'; and doubtless many hoped that the methods of science could play the demarcationist role formerly assigned to certainty. But, leaving aside the fact that agreement was lacking about precisely what the scientific method was, there was no very good reason as yet to prefer any one of the proposed 'scientific

methods' to any purportedly 'non-scientific' ones, since no one had managed to show either that any of the candidate 'scientific methods' qualified them as 'knowledge' (in the traditional sense of the term) or, even more minimally, that those methods were epistemically superior to their rivals.

### 3. A METAPHILOSOPHICAL INTERLUDE

Before we move to consider and to assess some familiar demarcationist proposals from our own epoch, we need to engage briefly in certain metaphilosophical preliminaries. Specifically, we should ask three central questions: (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'?

(1) Early in the history of thought it was inevitable that characterizations of 'science' and 'knowledge' would be largely stipulative and *a priori*. After all, until as late as the seventeenth century, there were few developed examples of empirical sciences which one could point to or whose properties one could study; under such circumstances, where one is working largely *ab initio*, one can be uncompromisingly legislative about how a term like 'science' or 'knowledge' will be used. But as the sciences developed and prospered, philosophers began to see the task of formulating a demarcation criterion as no longer a purely stipulative undertaking. Any proposed dividing line between science and non-science would have to be (at least in part) explicative and thus sensitive to existing patterns of usage. Accordingly, if one were today to offer a definition of 'science' which classified (say) the major theories of physics and chemistry as non-scientific, one would thereby have failed to reconstruct some paradigmatic cases of the use of the term. Where Plato or Aristotle need not have worried if some or even most of the intellectual activities of their time failed to satisfy their respective definitions of 'science', it is inconceivable that we would find a demarcation criterion satisfactory which relegated to unscientific status a large number of the activities we consider scientific or which admitted as sciences activities which seem to us decidedly unscientific. In other words, the quest for a latter-day demarcation criterion involves an attempt to render explicit those shared but largely implicit sorting mechanisms whereby most of us can agree about paradigmatic cases of the scientific and the non-scientific. (And it seems to me that there is a large measure of agreement at this paradigmatic level, even

allowing for the existence of plenty of controversial problem cases.) A failure to do justice to these implicit sortings would be a grave drawback for any demarcation criterion.

But we expect more than this of a *philosophically* significant demarcation criterion between science and non-science. Minimally, we expect a demarcation criterion to identify the *epistemic* or *methodological* features which mark off scientific beliefs from unscientific ones. We want to know what, if anything, is special about the knowledge claims and the modes of inquiry of the sciences. Because there are doubtless many respects in which science differs from non-science (e.g., scientists may make larger salaries, or know more mathematics than non-scientists), we must insist that any philosophically interesting demarcative device must distinguish scientific and non-scientific matters in a way which exhibits a surer epistemic warrant or evidential ground for science than for non-science. If it should happen that there is no such warrant, then the demarcation between science and non-science would turn out to be of little or no philosophic significance.

Minimally, then, a philosophical demarcation criterion must be an adequate explication of our ordinary ways of partitioning science from non-science and it must exhibit epistemically significant differences between science and non-science. Additionally, as we have noted before, the criterion must have sufficient precision that we can tell whether various activities and beliefs whose status we are investigating do or do not satisfy it; otherwise it is no better than no criterion at all.

(2) What will the formal structure of a demarcation criterion have to look like if it is to accomplish the tasks for which it is designed? Ideally, it would specify a set of individually necessary and jointly sufficient conditions for deciding whether an activity or set of statements is scientific or unscientific. As is well known, it has not proved easy to produce a set of necessary and sufficient conditions for science. Would something less ambitious do the job? It seems unlikely. Suppose, for instance, that someone offers us a characterization which purports to be a necessary (but not sufficient) condition for scientific status. Such a condition, if acceptable, would allow us to identify certain activities as decidedly unscientific, but it would not help 'fix our beliefs', because it would not specify which systems actually were scientific. We would have to say things like: "Well, physics *might* be a science (assuming it fulfills the stated necessary conditions), but then again it *might* not, since necessary but not sufficient conditions for the application of a term do not warrant application of the term." If, like Popper, we want to be able to answer the question, "when should a theory be ranked



as scientific?'',<sup>9</sup> then merely necessary conditions will never permit us to answer it.

For different reasons, merely sufficient conditions are equally inadequate. If we are only told: "satisfy these conditions and you will be scientific", we are left with no machinery for determining that a certain activity or statement is *unscientific*. The fact that (say) astrology failed to satisfy a set of *merely sufficient* conditions for scientific status would leave it in a kind of epistemic, twilight zone — possibly scientific, possibly not. Here again, we cannot construct the relevant partitioning. Hence, if (in the spirit of Popper) we "wish to distinguish between science and pseudo-science",<sup>10</sup> sufficient conditions are inadequate. The importance of these seemingly abstract matters can be brought home by considering some real-life examples. Recent legislation in several American states mandates the teaching of 'creation science' alongside evolutionary theory in high school science classes. Opponents of this legislation have argued that evolutionary theory is authentic science, while creation science is not science at all. Such a judgment, and we are apt to make parallel ones all the time, would *not* be warranted by any demarcation criterion which gave only necessary *or* only sufficient conditions for scientific status. Without conditions which are both necessary and sufficient, we are never in a position to say "*this* is scientific: but *that* is unscientific". A demarcation criterion which fails to provide both sorts of conditions simply will not perform the tasks expected of it.

(3) Closely related to this point is a broader question of the purposes behind the formulation of a demarcation criterion. No one can look at the history of debates between scientists and 'pseudo-scientists' without realizing that demarcation criteria are typically used as *machines de guerre* in a polemical battle between rival camps. Indeed, many of those most closely associated with the demarcation issue have evidently had hidden (and sometimes not so hidden) agendas of various sorts. It is well known, for instance, that Aristotle was concerned to embarrass the practitioners of Hippocratic medicine; and it is notorious that the logical positivists wanted to repudiate metaphysics and that Popper was out to 'get' Marx and Freud. In every case, they used a demarcation criterion of their own devising as the discrediting device.

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 'un-sound', '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 labelling 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. Although the cleaver that makes the cut may be largely epistemic in character, it has consequences which are decidedly non-epistemic. Precisely because a demarcation criterion will serve as a rationale for taking a number of *practical* actions which may well have far-reaching moral, social and economic consequences, it would be wise to insist that the arguments in favor of any demarcation criterion we intend to take seriously should be especially compelling.

With these preliminaries out of the way, we can turn to an examination of the recent history of demarcation.

#### 4. THE NEW DEMARCATIONIST TRADITION

As we have seen, there was ample reason by 1900 to conclude that neither certainty nor generation according to a privileged set of methodological rules was adequate to denominate science. It should thus come as no surprise that philosophers of the 1920's and 1930's added a couple of new wrinkles to the problem. As is well known, several prominent members of the *Wiener Kreis* took a syntactic or logical approach to the matter. If, the logical positivists apparently reasoned, epistemology and methodology are incapable of distinguishing the scientific from the non-scientific, then perhaps the theory of meaning will do the job. A statement, they suggested, was scientific just in case it had a determinate meaning; and meaningful statements were those which could be exhaustively verified. As Popper once observed, the positivists thought that "verifiability, meaningfulness, and scientific character all coincide."<sup>11</sup>

Despite its many reformulations during the late 1920's and 1930's verificationism enjoyed mixed fortunes as a theory of meaning.<sup>12</sup> But as a would-be demarcation between the scientific and the non-scientific, it was a disaster. Not only are many statements in the sciences not open to exhaustive verification (e.g., all universal laws), but the vast majority of non-scientific and pseudo-scientific systems of belief have verifiable constituents. Consider, for instance, the thesis that the Earth is flat. To subscribe to such a belief in the twentieth century would be the height of folly. Yet such a statement is

verifiable in the sense that we can specify a class of possible observations which would verify it. Indeed, every belief which has ever been rejected as a part of science because it was 'falsified' is (at least partially) verifiable. Because verifiable, it is thus (according to the 'mature positivists' criterion) both meaningful and scientific.

A second familiar approach from the same period is Karl Popper's 'falsificationist' criterion, which fares no better. Apart from the fact that it leaves ambiguous the scientific status of virtually every singular existential statement, however well supported (e.g., the claim that there are atoms, that there is a planet closer to the sun than the Earth, that there is a missing link), it has the untoward consequence of countenancing as 'scientific' every crank claim which makes ascertainably false assertions. Thus flat Earthers, biblical creationists, proponents of laetrile or orgone boxes, Uri Geller devotees, Bermuda Triangulators, circle squarers, Lysenkoists, charioteers of the gods, *perpetuum mobile* builders, Big Foot searchers, Loch Nessians, faith healers, polywater dabblers, Rosicrucians, the-world-is-about-to-enders, primal screamers, water diviners, magicians, and astrologers all turn out to be scientific on Popper's criterion — just so long as they are prepared to indicate some observation, however improbable, which (if it came to pass) would cause them to change their minds.

One might respond to such criticisms by saying that scientific status is a matter of degree rather than kind. Sciences such as physics and chemistry have a high degree of testability, it might be said, while the systems we regard as pseudo-scientific are far less open to empirical scrutiny. Acute technical difficulties confront this suggestion, for the only articulated theory of degrees of testability (Popper's) makes it impossible to compare the degrees of testability of two distinct theories *except when one entails the other*. Since (one hopes!) no 'scientific' theory entails any 'pseudo-scientific' one, the relevant comparisons cannot be made. But even if this problem could be overcome, and if it were possible for us to conclude (say) that the general theory of relativity was more testable (and thus by definition more scientific) than astrology, it would not follow that astrology was any less worthy of belief than relativity — for testability is a semantic rather than an epistemic notion, which entails nothing whatever about belief-worthiness.

It is worth pausing for a moment to ponder the importance of this difference. I said before that the shift from the older to the newer demarcationist orientation could be described as a move from epistemic to syntactic and semantic strategies. In fact, the shift is even more significant than that way of describing the transition suggests. The central concern of the older tradition

had been to identify those ideas or theories which were worthy of belief. To judge a statement to be scientific was to make a *retrospective* judgment about how that statement had stood up to empirical scrutiny. With the positivists and Popper, however, this retrospective element drops out altogether. Scientific status, on their analysis, is not a matter of evidential support or belief-worthiness, for all sorts of ill-founded claims are testable and thus scientific on the new view.

The failure of the newer demarcationist tradition to insist on the necessity of retrospective evidential assessments for determining scientific status goes some considerable way to undermining the practical utility of the demarcationist enterprise, precisely because most of the 'cranky' beliefs about which one might incline to be dismissive turn out to be 'scientific' according to falsificationist or (partial) verificationist criteria. The older demarcationist tradition, concerned with actual epistemic warrant rather than potential epistemic scrutability, would never have countenanced such an undemanding sense of the 'scientific'. More to the point, the new tradition has had to pay a hefty price for its scaled-down expectations. 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. But more often than not, the views in question can be tested, have been tested, and have failed those tests. But such failures cannot impugn their (new) scientific status: quite the reverse, *by virtue of failing the epistemic tests to which they are subjected, these views guarantee that they satisfy the relevant semantic criteria for scientific status!* The new demarcationism thus reveals itself as a largely toothless wonder, which serves neither to explicate the paradigmatic usages of 'scientific' (and its cognates) nor to perform the critical stable-cleaning chores for which it was originally intended.

For these, and a host of other reasons familiar in the philosophical literature, neither verificationism nor falsificationism offers much promise of drawing a useful distinction between the scientific and the non-scientific.

Are there other plausible candidates for explicating the distinction? Several seem to be waiting in the wings. One might suggest, for instance, that scientific claims are well tested, whereas non-scientific ones are not. Alternatively (an approach taken by Thagard),<sup>13</sup> one might maintain that scientific knowledge is unique in exhibiting progress or growth. Some have suggested that scientific theories alone make surprising predictions which turn out to be true. One might even go in the pragmatic direction and maintain

that science is the sole repository of useful and reliable knowledge. Or, finally, one might propose that science is the only form of intellectual system-building which proceeds cumulatively, with later views embracing earlier ones, or at least retaining those earlier views as limiting cases.<sup>14</sup>

It can readily be shown that none of these suggestions can be a necessary and sufficient condition for something to count as 'science', at least not as that term is customarily used. And in most cases, these are not even plausible as necessary conditions. Let me sketch out some of the reasons why these proposals are so unpromising. Take the requirement of well-testedness. Unfortunately, we have no viable over-arching account of the circumstances under which a claim may be regarded as well tested. But even if we did, is it plausible to suggest that all the assertions in science texts (let alone science journals) have been well tested and that none of the assertions in such conventionally non-scientific fields as literary theory, carpentry or football strategy are well tested? When a scientist presents a conjecture which has not yet been tested and is such that we are not yet sure what would count as a robust test of it, has that scientist ceased doing science when he discusses his conjecture? On the other side of the divide, is anyone prepared to say that we have no convincing evidence for such 'non-scientific' claims as that "Bacon did not write the plays attributed to Shakespeare", that "a mitre joint is stronger than a flush joint", or that "off-side kicks are not usually fumbled"? Indeed, are we not entitled to say that all these claims are much better supported by the evidence than many of the 'scientific' assumptions of (say) cosmology or psychology?

The reason for this divergence is simple to see. Many, perhaps most, parts of science are highly speculative compared with many non-scientific disciplines. There seems good reason, given from the historical record, to suppose that most scientific theories are false; under the circumstances, how plausible can be the claim that science is the repository of all and only reliable or well-confirmed theories?

Similarly, cognitive progress is not unique to the 'sciences'. Many disciplines (e.g., literary criticism, military strategy, and perhaps even philosophy) can claim to know more about their respective domains than they did 50 or 100 years ago. By contrast, we can point to several 'sciences' which, during certain periods of their history, exhibited little or no progress.<sup>15</sup> Continuous, or even sporadic, cognitive growth seems neither a necessary nor a sufficient condition for the activities we regard as scientific. Finally, consider the requirement of cumulative theory transitions as a demarcation criterion. As several authors<sup>16</sup> have shown, this will not do even as a necessary condition

for marking off scientific knowledge, since many scientific theories — even those in the so-called ‘mature sciences’ — do not contain their predecessors, not even as limiting cases.

I will not pretend to be able to prove that there is no conceivable philosophical reconstruction of our intuitive distinction between the scientific and the non-scientific. I do believe, though, that we are warranted in saying that none of the criteria which have been offered thus far promises to explicate the distinction.

But we can go further than this, for we have learned enough about what passes for science in our culture to be able to say quite confidently that it is not all cut from the same epistemic cloth. Some scientific theories are well tested; some are not. Some branches of science are presently showing high rates of growth; others are not. Some scientific theories have made a host of successful predictions of surprising phenomena; some have made few if any such predictions. Some scientific hypotheses are *ad hoc*; others are not. Some have achieved a ‘consilience of inductions’; others have not. (Similar remarks could be made about several non-scientific theories and disciplines.) *The evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion.* Where, even after detailed analysis, there appear to be no epistemic invariants, one is well advised not to take their existence for granted. But to say as much is in effect to say that the problem of demarcation — the very problem which Popper labelled ‘the central problem of epistemology’ — is spurious, for that problem *presupposes* the existence of just such invariants.

In asserting that the problem of demarcation between science and non-science is a pseudo-problem (at least as far as philosophy is concerned), I am manifestly not denying that there are crucial epistemic and methodological questions to be raised about knowledge claims, whether we classify them as scientific or not. Nor, to belabor the obvious, am I saying that we are never entitled to argue that a certain piece of science is epistemically warranted and that a certain piece of pseudo-science is not. It remains as important as it ever was to ask questions like: When is a claim well confirmed? When can we regard a theory as well tested? What characterizes cognitive progress? But once we have answers to such questions (and we are still a long way from that happy state!), there will be little left to inquire into which is epistemically significant.

One final point needs to be stressed. In arguing that it remains important to retain a distinction between reliable and unreliable knowledge, I am not

trying to resurrect the science/non-science demarcation under a new guise.<sup>17</sup> However we eventually settle the question of reliable knowledge, the class of statements falling under that rubric will include much that is not commonly regarded as 'scientific' and it will exclude much that is generally considered 'scientific'. This, too, follows from the epistemic heterogeneity of the sciences.

### 5. CONCLUSION

Through certain vagaries of history, some of which I have alluded to here, we have managed to conflate two quite distinct questions: What makes a belief well founded (or heuristically fertile)? And what makes a belief scientific? The first set of questions is philosophically interesting and possibly even tractable; the second 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 'pseudo-science' and 'unscientific' from our vocabulary; they 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.<sup>18</sup> Insofar as our concern is to protect ourselves and our fellows from the cardinal sin of believing what we wish were so rather than what there is substantial evidence for (and surely that is what most forms of 'quackery' come down to), then our focus should be squarely on the empirical and conceptual credentials for claims about the world. The 'scientific' status of those claims is altogether irrelevant.

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### NOTES

\* I am grateful to NSF and NEH for support of this research. I have profited enormously from the comments of Adolf Grünbaum, Ken Alpern and Andrew Lugg on an earlier version of this paper.

<sup>1</sup> See especially his *To Save The Phenomena* (Chicago: University of Chicago Press, 1969).

<sup>2</sup> This shifting in orientation is often credited to the emerging emphasis on the continuity of the crafts and the sciences and to Baconian-like efforts to make science 'useful'. But such an analysis surely confuses agnosticism about first causes – which is what really lay behind the instrumentalism of medieval and Renaissance astronomy – with a utilitarian desire to be practical.

<sup>3</sup> For much of the supporting evidence for this claim, see the early chapters of Laudan, *Science and Hypothesis* (Dordrecht: D. Reidel, 1981).

<sup>4</sup> See especially Chapter 8 of *Science and Hypothesis*.

<sup>5</sup> E. V. Davis, writing in 1914.

<sup>6</sup> See the discussions of this concept by Kavaloski, Hodge, and R. Laudan.

<sup>7</sup> For an account of the history of the concept of surprising predictions, see Laudan, *Science and Hypothesis*, Chapters 8 and 10.

<sup>8</sup> See Duhem's classic *Aim and Structure of Physical Theory* (New York: Atheneum, 1962).

<sup>9</sup> Karl Popper, *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963), p. 33.

<sup>10</sup> *Ibid.*

<sup>11</sup> *Ibid.*, p. 40.

<sup>12</sup> For a very brief historical account, see C. G. Hempel's classic, 'Problems and Changes in the Empiricist Criterion of Meaning,' *Revue Internationale de Philosophie* 11 (1950), 41–63.

<sup>13</sup> See, for instance, Paul Thagard, 'Resemblance, Correlation and Pseudo-Science,' in M. Hanen *et al.*, *Science, Pseudo-Science and Society* (Waterloo, Ont.: W. Laurier University Press, 1980), pp. 17–28.

<sup>14</sup> For proponents of this cumulative view, see Popper, *Conjectures and Refutations*; Hilary Putnam, *Meaning and the Moral Sciences* (London: Routledge and Kegan Paul, 1978); Władysław Krajewski, *Correspondence Principle and Growth of Science* (Dordrecht, Boston: D. Reidel, 1977); Heinz Post, 'Correspondence, Invariance and Heuristics,' *Studies in History and Philosophy of Science* 2 (1971), 213–55; and L. Szumilewicz, 'Incommensurability and the Rationality of Science,' *Brit. Jour. Phil. Sci.* 28 (1977), 348ff.

<sup>15</sup> Likely tentative candidates: acoustics from 1750 to 1780; human anatomy from 1900 to 1920; kinematic astronomy from 1200 to 1500; rational mechanics from 1910 to 1940.

<sup>16</sup> See, among others: T. S. Kuhn, *Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962); A. Grünbaum, 'Can a Theory Answer More Questions than One of Its Rivals?', *Brit. Journ. Phil. Sci.* 27 (1976), 1–23; L. Laudan, 'Two Dogmas of Methodology,' *Philosophy of Science* 43 (1976), 467–72; L. Laudan, 'A Confutation of Convergent Realism,' *Philosophy of Science* 48 (1981), 19–49.

<sup>17</sup> In an excellent study ['Theories of Demarcation Between Science and Metaphysics,' in *Problems in the Philosophy of Science* (Amsterdam: North-Holland, 1968), 40ff)], William Bartley has similarly argued that the (Popperian) demarcation problem is not a central problem of the philosophy of science. Bartley's chief reason for devaluing the importance of a demarcation criterion is his conviction that it is less important whether a system is empirical or testable than whether a system is 'criticizable'. Since he thinks many non-empirical systems are nonetheless open to criticism, he argues that the demarcation between science and non-science is less important than the distinction between the revisable and the non-revisable. I applaud Bartley's insistence that the empirical/non-empirical (or, what is for a Popperian the same thing, the scientific/non-scientific) distinction is not central; but I am not convinced, as Bartley is, that we should assign pride of place to the revisable/non-revisable dichotomy. Being willing to change one's mind is a commendable trait, but it is not clear to me that such revisability addresses the central *epistemic* question of the well-foundedness of our beliefs.



<sup>18</sup> I cannot resist this swipe at the efforts of the so-called Edinburgh school to recast the sociology of knowledge in what they imagine to be the 'scientific image'. For a typical example of the failure of that group to realize the fuzziness of the notion of the 'scientific', see David Bloor's *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976), and my criticism of it, "The Pseudo-Science of Science?" *Phil. Soc. Sci.* 11 (1981), 173–198.