

Addressing Three Popular Philosophic Myths about Karl Popper's Demarcation Criteria

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Introduction

Here are three philosophic myths:

- (1) Karl Popper's demarcation criterion of falsifiability is his sole criterion of demarcation.
- (2) The criterion sets out the boundaries of the natural sciences from non-science (or pseudo-science).
- (3) His criterion explicitly applies solely to singular theories that are universal in scope.

A number of objections against (1)–(3) can be grouped together as *objections from ill-fit*. In brief, Popper's demarcation criteria is (so his many critics claim) too broad or narrow in scope, thereby failing to include some (or all) paradigmatic 'scientific' theories or exclude some (or all) paradigmatic 'pseudo-scientific' theories. These objections are both widely known and remain influential in reconstructing the dialectical history of twentieth-century philosophy of science. For example, at least one collected book has been recently published on the deficiencies of Popper's purported demarcation criteria (Boudry and Pigliucci 2013), rehashing a number of the well-known objections against (1)–(3)—most of which take the form of objections from ill-fit—and asserting that these objections lead both to the downfall of the Popperian programme and helped facilitate the eventual rejection of the overall demarcationist project.

However, these philosophers of science continue to misrepresent both Popper's problem of demarcation and proposed demarcation criteria. That is to say, it is not disputed that the objections to (1)–(3) are effective against (1)–(3); however, all three myths are *false*. Consequently, many philosophers of science have been shadowboxing against a philosophical ghost.

1 A brief summary of Popper's positions

These objections from ill-fit are spurious for at least three reasons: (i) they misidentify the scope of the boundaries that are drawn, (ii) what is to be demarcated,

and (iii) what purpose demarcation criteria serve.

First, when Popper first set out his demarcation criteria, he explicitly was *not* interested in setting out the limits of what is science from non-science (or pseudo-science); rather, Popper attempted to set out a demarcation of what is *empirically significant* and what is not. In short, Popper, like A.J. Ayer and Rudolf Carnap (Ayer 1936, pp. 97-99, Carnap 1956), attempted to introduce a new set of categories in philosophical taxonomy to join the likes of the analytic/synthetic, a priori/a posteriori and necessary/contingent distinctions.

And yet, many philosophers of science continue to insist that Popper's criteria—specifically, his criterion of falsifiability—separate what is 'scientific' from what is 'non-scientific' or 'pseudo-scientific'. The first error (that is, claiming Popper's stated criterion demarcates the 'scientific' from 'non-scientific') fails to grasp the appropriate territorial boundaries; the second error (that is, claiming Popper's criterion demarcates the 'scientific' from 'pseudo-scientific') is doubly wrong, and conflates territorial demarcation criteria (e.g. the analytic/synthetic distinction) with normative criteria (e.g. what is preferable/dispreferable).

Second, Popper's criterion of falsifiability is *not* limited to singular theories. Specifically, Popper explicitly claims that his proposed criterion of falsifiability *cannot* under any circumstances apply to singular theories; it applies only to *theoretical systems*.

And yet, many philosophers of science continue to (falsely) insist that Popper's criterion of falsifiability applies to singular theories that are universal in scope. Any examination of Popper's early and later texts reveals that Popper explicitly claims the criterion of falsifiability does not; furthermore, Popper explicitly and repeatedly affirms a version of the Duhem thesis that *requires* the criterion of falsifiability apply *only* to theoretical systems.

Third, Popper (like Ayer and Carnap) proposed *two* separate necessary and sufficient conditions for demarcation¹. An article could very well be dedicated to listing the numerous instances of English-speaking philosophers of science failing to recognise that there exist two criteria of demarcation. Another article could be

¹In fact, Ayer's 1936/46 criteria and Carnap's 1956 criteria (Carnap 1956) are either trivial or reduce to Popper's 1934/5 criteria (See: Popper (1959, pp. 65-66), Ayer (1946)).

dedicated solely to addressing the fact that Popper's stated criteria are limiting cases of Carnap's (and Carnap's a limiting case of Ayer's); however, an examination of Popper 2000, pp. 65-66 is sufficient to reveal that there exist two demarcation criteria, not one: the first—Popper's falsifiability criterion—is a restricted version of Carnap's criterion of 'disconfirmability', and applies solely to *theoretical systems*; the second is a restricted version of Carnap's criterion of 'confirmability' criterion (itself a restricted version of Ayer's proposed criterion of 'predictability' (Ayer 1946)), which determines whether or not any member of a theoretical system is to be considered empirically predictive.

An inquisitive philosopher may ask: if so many philosophers of science have been misreading Popper for so long, how did these three myths become so widespread and entrenched within the discipline?

There are a number of potential explanations. Popper, as well as his student W.W. Bartley, seemed to believe there was a grand conspiracy against him and his work (see: Bartley 1990). Perhaps he was too curmudgeonly, rude, or went against the philosophic grain. This explanation, though parsimonious, relies on making a form of inference Popper himself excluded (Popper 2013, p. 306): positing a grand conspiracy.

Here is, I believe, a far more reasonable explanation that is also parsimonious, available in two textual 'Rosetta Stones': the objection from ill-fit looks *prima facie* promising upon a surface reading of Popper's 1959 book, *The Logic of Scientific Discovery* and a skim of his 1962 collection of informal lectures, *Conjectures and Refutations*. Both books are, in comparison to his more technical writings, quite popular introductions to Popper in the English-speaking world. However, both books practically invite the reader to engage in a rudimentary mistake in exegesis.

In *The Logic of Scientific Discovery*, Popper introduces a number of key terms which have an implied meaning in English not present in German. This was enough to lead to a gross misunderstanding amongst English-speaking philosophers of science. Due to a quirk of the German language that does not exist in English (i.e. the respective scopes of 'science' vs '*Wissenschaft*') Popper chose to translate a key technical term in the German edition, '*empirischen wissenschaft*' as 'empirical science', rather than, for example, 'knowledge gained by experience'.

As textual evidence, I provide a number of key passages that plausibly lead to

the entrenchment of this myth. I then compare them to the original text of Popper's book, *Logik der Forschung* (1934/5). Even a limited grasp of German reveals the extent of the problems in the 1959 translation that, as I posit, lead to the perpetuation of this myth. This is but a polite way of saying that Popper, who was mainly responsible for the English translation, was a far better philosopher than translator, even of his earlier work.

Other mistakes that have been promulgated amongst English-speaking philosophers of science may be due to the slapdash nature of Popper's comments in a public talk published in edited form in *Conjectures and Refutations* (1963). This article is not, however, representative of Popper's views either before or after its publication. That is, by 1963, Popper was a poor public communicator of Popper's views from 1934/5.

The best likely explanation for this is twofold: (1) the relative popularity of the volume of essays amongst philosophers of science in comparison to Popper's far more technical work in *Logik der Forschung*, and (2) Popper's overly informal speaking style in these essays. Thus *Conjectures and Refutations* is an abysmal choice if one wishes to understand the problem of demarcation.

And yet, the first chapter of *The Logic of Scientific Discovery* and the essay in *Conjectures and Refutations* on demarcation are the go-to sources for beginning to understand Popper's thoughts on demarcation.

Subsequently, many of Popper's critics mistakenly took the aim of his stated demarcation criterion to delineate the boundaries between the natural sciences and other forms of empirical inquiry; other philosophers concluded the criterion was normative rather than territorial; other inattentive critics took the aim of the criterion to be limited to individual theories.

That is not to say that there are no good objections to Popper's demarcation criteria or, for that matter, objections to the overarching demarcationist programme. There *are* some plausible objections available; however, grasping why these objections are worthy of taking seriously require understanding what purpose Popper's demarcation criteria serve and what the criteria actually are.

My purpose here is only to shed light on these popular misconceptions, and in doing so, facilitate better understanding of the history of the discipline of philosophy of science. In sum, it does philosophers of science no good—both on a ped-

agogical and academic level—to engage in a false reconstruction of the history of their discipline; it does historians of twentieth-century philosophy of science a further disservice in reconstructing the positions available at the time to misrepresent Popper’s position. It is natural that the complexities and nuances of philosophical positions will be papered over while engaged in public speaking, the classroom and in writing, but as philosophers of science and historians of philosophy, we owe past philosophers a fair hearing, even if we ultimately reject their ideas.

2 A misunderstanding of scope

Though brief, what follows is a fairly accurate depiction of what the philosophical community takes to be the two possible purposes of Popper’s proposed demarcation criterion: ‘Popper... wanted to distinguish scientific theories or hypotheses from nonscientific and pseudoscientific ones’ (Pigliucci 2013, p. 10). See also Preston 1994, p. 320). We can call the two problems a *normative* (i.e. ‘science’ versus ‘pseudo-science’) and a *territorial* problem of demarcation (i.e. ‘science’ versus ‘non-science’).

Territorial demarcation problems are taxonomic: distinctions are made without judgment over their respective values. One formulation of a territorial problem requires elucidating ‘the distinction between science and nonscience in general’ (Mahner 2013, p. 31). This formulation is repeatedly attributed to Popper in the philosophical literature (e.g., see Boudry 2013, p. 81 and Nickles 2013, p. 101). For one example, the *Stanford Encyclopedia of Philosophy* article on Popper states that falsifiability is a ‘criterion for demarcating science from non-science’ (Thornton 2016). This understanding of the territorial problem addressed by Popper is, as I show, fundamentally mistaken.

On the other hand, the normative problem attempts to distinguish ‘bona fide science from pseudoscience’ (Boudry 2013, p. 79). ‘Science’ in this sense is intended as an honorific bestowed on certain theories, practices or communities and involves a judgment of their respective values. This formulation of the normative problem is attributed to Popper as well (See also Laudan (1983, p. 118) and Boudry (2013, p. 80)). As I show, this depiction of Popper’s aims in setting out his version

of the demarcation problem is also inaccurate.

Contra Boudry (2013, p. 82), Mahner (2013, p. 114), *et al.*, Popper's interests at the time of the publication of *Logik der Forschung* are not set towards demarcating the natural sciences from non-science. In fact, he is explicit about this in the 1959 introduction to the English translation: the natural sciences are 'common-sense knowledge writ large... Its very problems are enlargements of the problems of common-sense knowledge' (Popper 1959, pp. xxvi, xix).

Popper's stated purpose of his proposed falsifiability criterion is to 'provide a suitable distinguishing mark of the *empirical, non-metaphysical, character of a theoretical system*' (Popper 1959, 11, cf. 14, emphasis added); the problem of demarcation is to find 'a criterion... [that] would enable us to distinguish between the empirical sciences on the one hand, and ... "metaphysical" systems on the other' (Popper 1959, p. 11). Thus any proposed solution to this territorial problem elucidates a theory of the absolute limits of empirical inquiry, not the limits of the natural sciences.

3 Lost in translation

An attentive philosopher may interject: *What is meant by the 'empirical sciences', then? Surely Popper is speaking about the limits of the sciences, not the limits of empirical inquiry!* Here is one major problem with Popper's translation of *Logik der Forschung* that has led to the perpetuation of this myth. Popper explicitly defines 'empirical science' ('*empirischen Wissenschaft*' in the 1934/5 German edition) as follows: "The system called "empirical science"... must satisfy the criterion of demarcation, ... i.e. it must not be metaphysical, but must represent a world of possible experience' (Popper 1959, pp. 16-17; see also Popper 1934/5, pp. 11-12). Popper's use in *Logik* of the phrase '*empirischen Wissenschaft*' tracks the German use of this technical term. For one example, I quote from Weibl and Herdina's English-German technical philosophical dictionary:

'empirical science' is defined as '*empirische Wissenschaft* ... empirical knowledge, knowledge by experience, empirical generalisation' (Weibl and Herdina 1997, p. 120).

In contrast to his technical term ‘empirical science’ when referring to ‘empirical knowledge’, Popper uses the technical term, ‘natural science’ (*‘naturwissenschaft’*), when referring to the sciences (Popper 1959, pp. 18, 48, 58). Thus ‘empirical science’ extends to cover knowledge possibly gained from experience *in toto*, not limited to knowledge gained from the natural sciences.

Note also that Popper introduces a definition of this technical term several pages after he sets out his formulation of the demarcation problem. This failure to signpost this technical term has, I believe, helped this myth spread. But how did such a failure to signpost a key technical term enter the English translation?

The translators of the English translation—Popper, Julius and Ian Freed—set out to be faithful to the text. From the introduction to the 1959 translation, we have the following: ‘The original text of 1934 has been left unchanged for the purpose of the translation’ (Popper 1959, p. xiv).

This aim in translation is impossible: a change to a text is necessary in translation. Examining the original edition and translation side-by-side, there is a strict adherence to sentence-by-sentence translation in the first chapter. What has not changed, however, is the order of sentences. Furthermore, whenever possible a ‘literal’ one-to-one English equivalent of a German term is given.

This choice in translation sets the reader of the English translation up for exegetical confusion in the very passage in which Popper first set out the demarcation criterion. The German edition says the following:

Ein empirisch-wissenschaftliches System muß an der Erahrung scheitern können (Popper 1934/5, p. 13).

Popper translates ‘empirisch-wissenschaftliches System’ as ‘an empirical scientific system’ (Popper 1959, p. 18), in accordance to its technical meaning in German. However, this can be understood to the English eye as either ‘an *empirical*-“scientific” system’, i.e. as a system of empirical knowledge, *à la* a form of *Wissenschaft*, or ‘an empirical *scientific*-system’, i.e. as the system of theories limited to the natural sciences.

Due to this choice in translation and how early the criterion is set out in *Logic*, this is one likely source of this confusion over Popper’s demarcation criterion. Over time, I conjecture, little attention to the original source material lead to the

perpetuation of the myth that Popper's criterion is directed towards setting out the limits of the natural sciences. Rather than define his technical term on the first page in an added footnote or aside, the definiens is introduced after Popper had used the term eighteen times (Popper 1959, pp. 3-4, 9, 11-12, 14-16).

Furthermore, this problem is compounded: in these sixteen pages before the definiens, Popper frequently uses the terms 'scientific' and 'natural sciences' in ways that repeatedly invite equivocation if the reader is not aware that they are each themselves technical terms in German philosophical discourse, and reflected as such in the original German edition. Popper's use tracks the use of their corresponding German technical terms, in which the closest analogue in English to the word *Wissenschaft* is the catchall term 'science'. The only differentiation made by Popper to each term is the choice to translate these key terms by amending 'empirical' ('*empirischen*') or 'natural' ('*natur*') to 'science' ('*Wissenschaft*'); their technical meaning is—naturally—not signposted by Popper, for the 'original text ... has been left unchanged'. This adherence to this rule of translation is to the English version's detriment, up to and including the choice of the title of the English translation, which were it to adhere more closely to the original German implication, would be more suitably titled, 'The Logic of [Empirical] Investigation [or Inquiry]'.

These problems are nonexistent in the German edition (Popper 1934/5). In fact, after an exhaustive search of the available philosophic literature, I could only place variations of this formulation of the objection from ill-fit after 1959, and could find no instances of this objection from ill-fit in any German texts from 1934/5 to 1959. If this explanation accounts for the available evidence, then so much for Popper's acumen at translation: he picked the closest English term available to the German term '*Wissenschaft*', then failed to revise the translation to reflect the implicature of the term 'science' in English.

4 A misunderstanding of subject

The problems within English-oriented philosophy of science in understanding Popper's demarcation criteria do not end here. Furthermore, Popper's falsifiability criterion is routinely falsely purported to be the following: a singular theory

is scientific if and only if it is falsifiable (Thornton 2016 and Nickles 2013, p. 101). That is, a universal statement counts as belonging to the natural sciences if and only if it is prohibitive: it rules out the possibility of some state of affairs. If the statement is not prohibitive, it is either pseudoscientific or nonscientific. This is Popper's (supposed) famous falsifiability criterion. This formulation is the one I was told as a student, heard in casual conversation at conferences, and read in numerous articles and books. The number of examples perpetuating this formulation of the criterion of falsification in the available literature is overwhelming.

We have already seen that one part of this common restatement of Popper's falsifiability criterion is mistaken: falsifiability is not restricted to demarcating the natural sciences from other empirical concerns. However, an examination of *The Logic of Scientific Discovery* reveals that Popper's criterion of falsifiability bears little resemblance to the commonly-held conception of the criterion falsifiability. Popper first sets it out as follows:

it must be possible for an *empirical scientific system* to be refuted by experience (Popper 1959, p. 18).

That is to say, a theoretical system—a large set of sentences, including auxiliary hypotheses—must rule out the possibility of some systems of statements 'of a lesser level of universality' (Popper 1959, 25, emphasis added). This formulation is explicitly *not* about singular universal statements, but of *systems of statements*. Further textual evidence demonstrates Popper's repeated insistence that his criterion of falsifiability is directed towards demarcating *systems of statements*, rather than demarcating singular theories that are universal in scope. Popper repeatedly says variations of the following in his oeuvre, for example:

... we can indeed falsify only systems of theories (Popper 1983, p. 187)².

Another example:

... it is important to remember that [the criterion of demarcation] applies to theoretical systems rather than to statements picked out

²See also Popper (1934/5, pp. 12-13), Popper (1959, p. 18) and Popper (1963, pp. 56, 66) for other examples.

from the context of a theoretical system (Popper 1983, p. 178. See also Popper 1963, pp. 186, 256).

Given that Popper repeatedly clarified that only theoretical systems could be falsified, and only by 'lower level' theoretical systems, the prevalence of the mistaken formulation as falsifiability applying to a singular theory is concerning. How could this happen?

One likely answer to this misunderstanding is as follows: Popper first introduced his falsifiability criterion in German, and with a high degree of clarity; he turned his attention elsewhere in his later writings, specifically to the *normative* problem of demarcation, and rarely differentiated between the two problems; consequently, his later popular writing in English reflecting on his past positions are at times incredibly opaque. It is only clear from context whether he is referring to whether certain *decisions or actions of an epistemic community* are 'pseudo-scientific' (i.e., dispreferable) or 'scientific' (i.e. preferable) (i.e. the normative problem of demarcation) or whether certain *systems of statements* should be categorised as empirically significant or not.

This opaqueness is most noticeable in his collected volume of collected essays, *Conjectures and Refutations* (1963). Consider the following passage:

... statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable observations' (Popper 1962, p. 39).

Were a philosopher to read *Conjectures and Refutations* without checking *The Logic of Scientific Discovery*, they would conclude two things: Popper's falsifiability criterion applied to both singular theories and theoretical systems, and Popper's criterion demarcates science from non-science or pseudo-science. And yet, in his earlier writings, Popper explicitly repudiated that falsifiability was concerned with demarcating science from non-science or pseudo-science and that singular sentences were falsifiable. In this public lecture, Popper's exegesis is almost as deficient as the exegesis of his critics.

5 The erroneous objection from ill-fit

With these two common misconceptions laid out, how does the objection from ill-fit hold up? First, it helps to understand the substance of the objection, as well as its prevalence in the literature. Carl Hempel notes territorial criteria are bound to be both too restrictive and too permissive (Hempel 1950; Hempel 1951). While Hempel's focus is directed at Ayer and Carnap's criteria, this general method has been appropriated to apply to Popper's criterion of falsifiability.

One example arguing that falsifiability is a criterion that is too permissive comes from Larry Laudan in his (in)famous article, *The demise of the demarcation problem*: it—so Laudan thinks—‘has the untoward consequence of countenancing as “scientific” every crank claim which makes ascertainably false assertions’ (Laudan 1983, p. 121). Other examples that parrot Laudan can be found in Lakatos (1981, p. 117), Mahner (2013, p. 30) and Boudry (2013, p. 87).

Philip Kitcher (1982, p. 44), on the other hand, asserts the falsifiability criterion is too restrictive: ‘one can appeal to naive falsificationism to show that any science is not a science’: Kitcher rightly notes a scientific theory requires auxiliary hypotheses in order to make any predictions, thus no theory qualifies as ‘scientific’ if it is divorced from a theoretical system. (See also Thornton (2016) and Putnam (1974) for other examples in the literature.)

If no proposed criteria can set out the boundaries between the natural sciences and non-science, philosophers then abandon this territorial problem and redouble efforts to solve normative problems of demarcation Boudry and Pigliucci 2013.

Note, however, that Laudan confuses normative and territorial criteria: since the territorial criterion set out by Popper delineates the absolute limits to empirical inquiry; it is not normative (Popper 1959, pp. 14, 16). It does not matter if there exists ascertainably false or other epistemically objectionable empirically significant systems of statements that are falsifiable. This is to be expected for any solution to a territorial criterion proposing the limits to the domain of some form of discourse. In this case, empirical systems of statements that are known to be false are empirical because that they are *known to be false through empirical inquiry*. Naturally, *many* pseudo-scientific claims will be falsifiable: these claims are false,

are known to be false by many scientists, and yet pseudo-scientists disregard any potential defeating evidence from a 'low-level' theoretical system. In fact, these very individuals are often dubbed 'pseudo-scientists' or 'pseudo-scientific' because they accept only (tenuously) confirming evidence for their claims while refusing to engage with apparent defeaters in good faith (i.e. their actions when faced with criticism are undesirable according to Popper's *normative* demarcation criteria).

Two analogies illustrate the absurdity of Laudan's reasoning: it would be as fruitful to object to falsifiability as a territorial criteria on these stated grounds as it would to object to criteria for synthetic statements on the grounds that under some stated criterion of synthetic statements, a synthetic statement can be ascertainably false and still be synthetic; furthermore, it would be as absurd to believe that a group of people that doggedly refuse to treat certain analytic sentences as analytic come what may, and no matter the defeating evidence or arguments provided, somehow undermines any proposed criteria of analyticity not predicated on communal acceptance; that is, the objection from ill-fit as framed previously by Laudan is not a cogent objection.

Of course, since Laudan has confused territorial and normative demarcation criteria, and the two are often simply referred to as 'the problem of demarcation', his objection has often been accepted without further investigation. Laudan's reply is, however, more of a thought-terminating cliché than an appropriate rejoinder to Popper's criterion of falsifiability.

Furthermore, note the objection from Kitcher that scientific theories on their own lack any predictive capacity is predicated on two of the previously mentioned mistakes in exegesis: first, the purpose the falsifiability criterion is not about whether a theory is *scientific* or *non-scientific*; falsifiability is a criterion for *empirical significance*. Second, singular theories are *not* what is demarcated by the criterion of falsifiability; rather, *theoretical systems* are (Popper 1959, p. 25). Instead, Popper's limiting case of Carnap's criterion of confirmability (acceptance of 'low-level' theoretical systems) determines whether a statement within a theoretical system should be considered empirically predictive.

6 Conclusion

In summary, the objections from ill-fit confuse the problem as normative when it is territorial; as concerned with a singular theory and not empirical theoretical systems; and as setting out the differences between science and non-science when it is about setting out the absolute limits to empirical inquiry. Dismissing Popper's territorial problem based on the objections from ill-fit and characterising Popper's stated territorial problem and criterion as such are both predicated on mistakes in exegesis. These confusions are most likely due to Popper's refusal to immediately explain how he used the technical term 'empirical science' in the 1959 English translation of *Logik der Forschung*, and exacerbated by Popper's most popular writings providing a simplistic, obfuscatory gloss of his more technical work.

Should any philosophic moral be drawn from this extended exegetical examination? There are at least two:

First, philosophers of science and historians of philosophy of science should stop perpetuating these previously presented objections as if they target Popper's territorial demarcation criteria. To perpetuate these myths as if they accurately reflected the position of one of the most widely known twentieth-century philosophers of science, as well as claiming the objections from ill-fit put to rest the position, is to engage in bad history of twentieth-century philosophy.

Second, and more generally, philosophers of science and philosophy students should be extremely careful when accepting that purportedly definitive objections to a philosophical programme are accurate. If an objection against a target can be raised with a moment's thought, more reflection is due on whether the crosshairs are properly aligned. This may be doubly concerning in regards to history of twentieth-century philosophy of science, however: if Popper, one of the most popular philosophers of science, was so easily misunderstood, it is worth examining whether other twentieth-century philosophers of science were also routinely misread.

Lastly, there may be other reasons to reject Popper's territorial demarcation criteria or, for that matter, the demarcationist programme in general; however, these objections are not worth teaching to philosophy students, nor are they worth

taking seriously by philosophers: objections from ill-fit are much ado about nothing, potshots aimed at an intellectual ghost.

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