

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

Nathan Oseroff

Introduction

Here are three philosophic myths:

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

These three myths are each expressed in, for instance, the collection of—and commentary on—key philosophical texts, *Philosophy of Science: The Central Issues*: 'According to Popper, a theory is scientific if and only it is falsifiable' (Curd, Cover, and Pincock 2013, p. 1307).

A number of objections against (1)–(3) can be grouped together under the category of *objections from ill-fit*. In brief, Popper's demarcation criteria is (so his many critics claim) too broad or too narrow in scope, thereby failing to include some (or all) paradigmatic 'scientific' theories or exclude some (or all) paradigmatic 'pseudo-scientific' theories. The objections for example, include that it is difficult or impossible to have certainty that a scientific theory has been falsified (and therefore demarcation criteria are too narrow); that paradigmatic 'pseudo-scientific' theories are classified as 'scientific' (and therefore demarcation criteria

are too broad); that only a theoretical system can be falsifiable (and therefore demarcation criteria are too narrow); that paradigmatic 'metaphysical' theories play an integral role in the natural sciences (and therefore demarcation criteria are too broad); and so on. One example of an objection from ill-fit is as follows:

Popper's simple idea does not work. ... [F]alsifiability is both too weak and too strong. It is too weak because it would allow as scientific any number of claims that are testable in principle but that are not, by any stretch of the imagination, scientific. It is too strong because it would rule out as unscientific many of the best theories in the history of science. (Curd, Cover, and Pincock 2013, p. 68)

These objections are both widely accepted and remain influential in reconstructing the history of twentieth-century philosophy of science. To list another example, at least one collected book has been recently published on, in part, deficiencies with Popper's purported demarcation criterion (Boudry and Pigliucci 2013), rehashing a number of objections from ill-fit against (1)–(3). As many of these philosophers of science see it, these objections directly lead to the downfall of the Popperian programme.

However, 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 objections from ill-fit are effective against (1)–(3), for hardly anyone can dispute their effectiveness both as a matter of an analysis of the direction taken in history of philosophy of science away from the Popperian programme and taken on the apparent deductive validity of many of these arguments. Nevertheless, *all three myths are demonstrably false*.

These objections from ill-fit are spurious for at least three reasons: (a) they misidentify the scope of the boundaries that are drawn (the natural sciences versus the empirical and non-empirical), (b) what domains the criteria apply to (sets of sentences or individual theories), and (c) the number of criteria (there are two, not one). Clarifying these three points is enough to make much of the popular criticism of these demarcation criteria expressed in objections from ill-fit demonstrably moot. In reality:

- (1*) Popper's demarcation problem is to determine if there are necessary and sufficient conditions for drawing borders between what is *empirical* and what is *non-empirical* (encompassing the metaphysical, analytic, normative, mathematical and logic domains).
- (2*) The criterion of falsifiability explicitly only applies to *large sets of statements*.
- (3*) Popper set forward a *second*—almost entirely neglected—criterion of demarcation that classifies individual statements as either empirical or non-empirical.

Consequently, many philosophers of science have dismissed a philosophical programme based on a mischaracterisation; they have been shadowboxing against a philosophical ghost.

In what follows I set out why these three myths are wrong (§§1-3), how many objections to Popper's demarcation criteria are predicated on accepting these myths and where these objections fall flat (§4) and provide a plausible account of the origin of these myths (§5) that may provide useful advice for writing philosophy, as well as reading the work of philosophers.

1 The first myth: demarcation sets out the boundaries of the sciences from non-science or pseudo-science

The first myth, that *the criterion of falsifiability sets out the boundaries of the natural sciences from non-science (or pseudo-science)*, is widespread. Highly capable philosophers of science will say as such. Take Massimo Pigliucci, for example: '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'). Other examples are legion (see both above and below).

Territorial demarcation problems are taxonomic: distinctions are made without judgment over their respective values. One formulation of a territorial problem attributed to Popper requires elucidating 'the distinction between science and

nonscience in general' (Mahner 2013, p. 31). This formulation is not unique; it is repeatedly attributed to Popper in the philosophical literature (cf. Boudry (2013, p. 81) and Nickles (2013, p. 101)). For one prominent example, the *Stanford Encyclopedia of Philosophy* article on Popper states that the criterion of falsifiability is a 'criterion for demarcating science from non-science' (Thornton 2016). Other popular examples on internet encyclopaedias include Wikipedia ('Popper uses falsification as a criterion of demarcation to draw a sharp line between those theories that are scientific and those that are unscientific' ((n.a.) n.d.)) and the *Internet Encyclopedia of Philosophy* ('Much of Popper's early work in the philosophy of science focuses on what he calls the problem of demarcation, or the problem of distinguishing scientific (or empirical) theories from non-scientific ones' (Shea 2018)).

On the other hand, the normative problem, as traditionally construed, 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 (cf. Laudan (1983, p. 118) and Boudry (2013, p. 80)). The 'conventional wisdom' surrounding Popper's purported motivations is captured in, for example, (Kneale 1974, p. 206): 'His [Popper's] primary aim was to distinguish between the universal propositions of genuine science, whether true or false, and those of pseudoscience', and is almost as popular as the canard that Popper's demarcation criterion separates scientific from non-scientific theories.

Even the authoritative biography of Popper, *Karl Popper - The Formative Years, 1902-1945*, includes such difficult to parse passages as 'Contrary to his [Popper's] autobiographical accounts, he had not yet settled on testability, or falsifiability, as the demarcation between science and pseudoscience, or science and metaphysics' (Hacohen 2002, p. 96) and 'Falsifiability became a marker of scientific statements' (Hacohen 2002, p. 198).

These depictions, however, are inaccurate. The first major error (that is, claiming Popper's stated criterion demarcates the 'scientific' from 'non-scientific') fails to grasp the appropriate territorial boundaries; the second major error (that is, claiming Popper's criterion demarcates the 'scientific' from 'pseudo-scientific') conflates territorial demarcation criteria (e.g. the analytic/synthetic distinction)

with normative criteria (i.e. what sorts of beliefs or behaviours are preferable/dispreferable). But what boundaries, then, do demarcation criteria *demarcate*?

1.1 The truth: falsifiability is about the limits of empirical matters for an epistemic community

Contra Boudry (2013, p. 82), Mahner (2013, p. 114), *et al.*, Popper's interests in setting out his demarcation criterion of falsifiability at the time of the publication of *Logik der Forschung* through his later works are not set towards demarcating the natural sciences from non-science or pseudo-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).

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 sorts of sentences should be treated as *empirically significant* (or 'empirical' or 'empirical science') and what sorts of sentences should be treated as *empirically non-significant* (or 'non-empirical', 'not empirical' or 'not part of empirical science').

Popper's stated purpose of his proposed falsifiability criterion is, rather, 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.

It is of note that Popper repeatedly says variations of what is to demarcated in his oeuvre, for example, in the English translation of his first book, *The Two Fundamental Problems of the Theory of Knowledge*:

What distinguishes the *empirical sciences* from the *non-empirical sciences*...? A criterion that marks out some statements or systems of

statements as empirical, and others as non-empirical... (Popper 2009, p. 383)¹

So much for the first myth: Popper's criterion does not set out the boundaries of the natural sciences from non-science (or pseudo-science). In fact, Popper's criterion sets out the boundaries of what is empirical from what is not.

2 The second myth: individual, isolated theories are not falsifiable

Let us now turn to the second myth. Popper's falsifiability criterion is routinely said 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 empirical 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. It is worth briefly covering how widespread this second myth is, even within 'Popperian' circles:

Reading Popper's *The Logic of Scientific Discovery* would be sufficient for many people to arrive at the conclusion that falsifiability applies only to individual theories²

¹A far more subtle myth, however, acknowledges what Popper had to say about the demarcation but fails to grasp that the limits of what is 'empirical' and 'non-empirical' are indexed to the epistemic limits and standards of acceptance in a particular epistemic community. Objections from ill-fit that make this mistake will be addressed below.

²To list a few examples in order, Popper says '... there is the investigation of the logic form of the theory, with the object of determining whether it has the character of an empirical or scientific theory ...' (Popper 1959, p. 9); '... there is the testing of the theory ...' (Popper 1959, p. 9); '... other statements ...are deduced from

Perhaps one of the biggest defenders of ‘orthodox’ Popperianism, David Miller, may have (most likely accidentally) helped perpetuate this confusion, saying, ‘This natural, but essential, qualification is Popper’s criterion of demarcation between science and nonscience: a hypothesis may be admitted to the realm of scientific knowledge only if it is falsifiable by experience’ (Miller 1994, p. 7) and ‘A theory, Popper’s criterion of demarcation says, must be falsifiable in principle if it is to belong to empirical science’ (Miller 2006, p. 5)³.

The popular scientist and avowed Popperian, David Deutsch, says, ‘Testability is now generally accepted as the defining characteristic of the scientific method. Popper called it the “criterion of demarcation” between science and non-science’ (Deutsch 2011, p. 14) while the historian of philosophy and biographer of Popper, Haim Hacohen, says, ‘Falsifiability would demarcate testable statements as scientific, nontestable statements as metaphysical’ (Hacohen 2002, p. 208).

Critics of Popper also routinely claim the falsifiability criterion applies only to theories, but are too numerous to mention here. Remember, for example, Curd, Cover, and Pincock (2013, 1307, emphasis added): ‘According to Popper, a *theory* is scientific if and only if it is falsifiable’.

As we now know, the falsifiability criterion is not demarcating what is scientific from what is non-scientific or pseudo-scientific (at least in the sense understood in common English parlance). We can put aside the first myth and reformulate the popular conception of Popper’s demarcation criterion as follows: *a universal statement counts as being empirically significant if and only if it is prohibitive: it rules out the possibility of some state of empirical affairs.*

the theory’ (Popper 1959, p. 10); ‘...if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced ...’ (Popper 1959, p. 10); ‘... no conclusive disproof of a theory can ever be produced’ (Popper 1959, p. 28); (cf. Popper 1959, 28, 29, 49, 55ff.). Similar examples can be found in, e.g., Popper (1962), Popper (1963), Popper (1974), and Popper (1983), each following the format of referring to a *theory* that is falsified by accepting an empirical statement that contradicts the theory.

³In later sections, Miller, thankfully, clarifies that when he speaks of ‘science’ and ‘nonscience’, he is speaking of the ‘empirical sciences’; however, he frequently does not clarify that the criterion of falsifiability does not apply to individual, isolated theories.

2.1 The Truth: Falsifiability applies only to theoretical systems

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. In fact, Popper is explicit that his falsifiability criterion applies only to a large set of statements. Popper first sets out his criterion 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 T —a large set of sentences, including auxiliary hypotheses, basic statements and a number of scientific and non-scientific (but empirical) theories—must rule out the possibility of some 'experience'.

But how can 'experience' be expressed in a more exact way? According to Popper, this can only be done by some system of statements E 'of a lesser level of universality' (Popper 1959, p. 25) when compared to T , which are dubbed 'basic statements'. That is, E is already classified by an epistemic community as uncontroversially 'empirical', and is the sort of expression about what is, in principle, possibly intersubjectively observable, *viz.* expressions specifying the existence or nonexistence of entities at certain limited spatiotemporal regions, the occurrence or nonoccurrence of specific events at space-time regions, and so on.

As W.H. Newton-Smith summarises,

Such statements, which he calls *basic statements*, are characterized not epistemologically but in terms of their form and their role. The form of a basic statement is that of a singular existential statement where this means that they are existential assertions about some definite spatio-temporal region. To use Popper's favourite example: 'There is a raven in spatio-temporal region k '. In addition, any conjunction of such statements constitutes a basic statements. ... As I have stressed, the role of basic statements is not to provide any epistemological bed-rock... (Newton-Smith 1981, p. 49)

Naturally, this formulation does not entail that a theoretical system, were it to contradict another basic statement (itself part of a theoretical system 'of a lesser

level of universality'), is therefore *false* in virtue of such a contradiction, nor does the criterion of falsifiability say anything about whether such a refutation must be made actual; Popper's criterion is merely modal: that it is *possible* that at least one *E* contradict *T*. Furthermore, and more importantly for this myth, this formulation is explicitly *not* about singular universal statements, but of *systems of statements*⁴.

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. From, for example, *Realism and the Aim of Science*:

... we can indeed falsify only systems of theories (Popper 1983, p. 187)⁵.

Another illustrative passage from *Conjectures and Refutations* is as follows:

Hence I suggested that testability or refutability or falsifiability should be accepted as a criterion of the scientific character of theoretical systems...' (Popper 1963, pp. 174–5) (See also Popper (1963, pp. 186, 256))⁶.

A further example from *Realism and the Aim of Science* is even more explicit:

... it is important to remember that [the criterion of demarcation] applies to theoretical systems rather than to statements picked out from the context of a theoretical system (Popper 1983, p. 178).

In *Replies to My Critics*, Popper further directly addresses this myth:

⁴In fact, it would seem the primary distinction to be drawn between Popper and, for example, some interpretations of Quine, is that Popper denied only a radical version of Quinean holism and instead explicitly adopted a version of holism that is notably close to Duhem's own version (Ariew 1984).

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

⁶However, Popper's ability to invite confusion is exemplified in the rest of the statement: '... that is to say, as a criterion of demarcation between *empirical sciences on the one hand and pure mathematics, logic, metaphysics, and pseudo-science on the other*', as addressed previously, viz. Popper's lack of clarity concerning the expression 'pseudo-science'.

[the problem of demarcation] can be considerably improved if one speaks of *theoretical systems or systems of statements*, as I did ... even if we can apply it to systems of statements, it may be difficult if not impossible to say which particular statement, or which subsystem of a system of statements, has been exposed to a particular experimental test. Thus we may describe a system as scientific or empirically testable, while being most uncertain about its constituent parts. ... *if we falsify it, we falsify the whole system.* (Popper 1974, p. 982)

And from *Conjectures and Refutations*, Popper belabors this point:

... the problem of drawing a line of demarcation between those statements and systems of statements which could be properly described as belonging to empirical science, and others which might, perhaps, be described as ‘pseudo-scientific’ or (in certain contexts) as ‘meta-physical’, or which belonged, perhaps, to pure logic or to pure mathematics. ... I proposed ... that the refutability or falsifiability of a theoretical system should be taken as the criterion of its demarcation ... a system is to be considered as scientific only if it makes assertions which may clash with observations’ (Popper 1962, pp. 255–56)

In sum, the textual evidence for falsifiability applying to sets of statements—rather than individual statements—is overwhelming. While it is difficult—if not impossible—to rationally reconstruct Popper’s entire work over his decades-long span in a consistent manner, this continued insistence and clarification that any use of the word ‘theory’ in relation to his falsifiability criterion be understood as an elliptical expression for ‘theoretical system’, however, is one case in which Popper was consistent across his career. We can now put to rest the second myth.

3 The third myth: falsifiability is sole criterion of demarcation

Now it is time to address the last, most entrenched and widely-accepted myth: *there is one sole criterion of demarcation and it is the criterion of falsifiability.*

It is difficult to find many philosophers of science that explicitly formulate this myth, for it is a myth perpetuated only by way of omission: when Curd, Cover, and Pincock (2013, 1307, emphasis added) say, 'According to Popper, a theory is scientific *if and only* it is falsifiable', the explicit use of material equivalence entails that no other demarcation criteria are necessary. The widespread invocation of this myth, though, is demonstrated by noting that Popper and his students do not address it after he first introduced it in *Logik*. It is, at least upon a careful reading, thought to be a criterion of demarcation that is either a sister-criterion to his criterion of falsifiability of theoretical systems that applies to member statements of the system or reducible to it.

3.1 The truth: there is a second, neglected demarcation criterion

However, Popper does in fact set out a *second* criterion of demarcation, which we can call, after A.J. Ayer, the criterion of 'predicability'. Following Popper's reasoning, we see that he is fully in accord with many of his critics on the deficiencies of relying solely on a demarcation criterion for theoretical systems:

As a first attempt one might perhaps try calling a theory 'empirical' whenever singular statements can be deduced from it. This attempt fails, however, because in order to deduce singular statements from a theory, we always need other singular statements—the initial conditions that tell us what to substitute for the variables in a theory.

Popper, like Duhem, is absolutely correct: a statement that contains variables cannot entail anything (Ariew 1984). Since statements with variables can be 'empirical', it is not merely deduction from a statement that makes it 'empirical'; the statement must be conjoined with other statements in order to be 'empirical'. Therefore this first attempt is too strong, permitting too few plausibly empirical statements to be classified as 'empirical'.

As a second attempt, one might try calling a theory 'empirical' if singular statements are derivable with the help of other singular statements serving as initial conditions. But this will not do either; for

even a non-empirical theory, for example a tautological one, would allow us to derive some singular statements from other singular statements'

Again, Popper, like many philosophers of science that address the 'tacking paradox', note that the conjunction of an analytic sentence ('Bachelors are bachelors') and a synthetic sentence ('Here is a black raven') one can derive a singular statement from it ('Here is a raven'). Thus this second attempt is too weak, allowing us to classify a number of paradigmatically metaphysical or analytic sentences as 'empirical'.

Popper continues on to set out further attempts, such as that a conjunction of a statement and initial conditions 'we should be able to deduce *more* than we could deduce from those initial conditions alone' (Popper 1959, p. 64), and notes their deficiencies (it would be too weak, permitting synthetic metaphysical statements like 'Every occurrence has a cause' (Popper 1959, p. 64)) before arriving at his final proposal for classifying individual statements as 'empirical':

... the theory should allow us to deduce, roughly speaking, more *empirical* singular statements than we can deduce from the initial conditions alone. (Popper 1959, p. 65)

This passage is, I admit, confusing, for it employs Popper's idiosyncratic terminology and leads to equivocation on whether Popper is referring to individual statements within a theoretical system or theoretical systems. However, when it is rephrased in more exact speech, it becomes abundantly clear: a statement S is to be classified as 'empirical' if S , when conjoined with a set of other statements T , $S \cup T \models E$, where E is a 'basic statement' (that is, a statement that is uncontroversially considered 'empirical' by an epistemic community) and $T \not\models E$ and $S \not\models \perp$ (Popper 1959, p. 64).

Why has this second criterion of demarcation been neglected? It certainly does Popper no good that in the very paragraph that Popper sets out this neglected second criterion of demarcation he then invites confusion for the reader, saying, 'Seeing that it would not be very easy to say in detail how a complicated

theoretical system helps in the deduction of singular or basic statements, I propose the following definition. A theory is to be called “empirical” or “falsifiable” if ...the class of its potential falsifiers is not empty’ (Popper 1959, pp. 65–66)

Are we to understand Popper in this passage as setting out a formal definition for falsifiability? Perhaps.

Perhaps Popper believed this to be the case, since his proposed second criterion *does* entail that a statement S , when conjoined with T , will itself be ‘falsifiable’ in the sense that $S \cup T$ is falsifiable if $S \cup T \models E$ (one need only accept the possibility of a basic statement $\neg E$). However, though falsifiability is derivable from this criterion, this second criterion is not *reducible* to it, for it operates not on the level of theoretical systems but identifies which members of a theoretical system entail observation statements.

Perhaps Popper recognised that this second criterion for statements, rather than theoretical systems, entailed that the statement was ‘falsifiable’, in that if the rest of the theoretical system T remained fixed, the statement S is *indeed* falsifiable. Thus (perhaps, as Popper presumably concluded) falsifiability was both necessary and sufficient both for member statements of a theoretical system and the theoretical system as a whole. Unfortunately, Popper does not explicate his reasoning anywhere in his published writings for this decision.

Interestingly, though, this criterion for individual sentences is itself surprisingly similar to A.J. Ayer’s own criterion of predictability. As Hempel summarises Ayer’s criterion,

...a sentence S has empirical import if from S in conjunction with suitable subsidiary hypotheses it is possible to derive [observation statements] which are not derivable from the subsidiary hypotheses alone (Hempel 1965, p. 106)

Hempel could very well have been summarising Popper’s criterion for individual sentences. Examining Ayer’s criterion for ‘direct verifiability’ reveals striking similarities in its language with Popper’s: in Ayer’s words,

...a statement is directly verifiable if it is either itself [an observation statement] or is such that in conjunction with one or more [auxiliary

statements] it entails at least one [observation statement] which is not deducible from these other premises alone (Ayer 1946, p. 13)

Given the similarities between Popper, Carnap and Ayer's criteria, it's worth interpreting this second criterion as not reducible to his first for sake of conceptual clarity alone, and as fully in line with other attempts at setting out demarcation criteria.

In short, Popper (like Ayer and Carnap) proposed *two* separate necessary and sufficient conditions for demarcation⁷: one for theoretical systems (as mentioned previously), another for members of a theoretical system.

Popper's falsifiability criterion is a restricted version of Carnap's criterion of 'disconfirmability', and applies solely to *theoretical systems* (Lutz 2013); 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.

This (admittedly) surprising result undermines the assumption within the philosophic literature that Popper's criterion of falsifiability is both a necessary and sufficient condition for empirical significance; rather, it is necessary, but not sufficient, for there exists a second criterion of demarcation. So much for the third myth.

Correcting these three myths

When faced with these three philosophic myths, what remains? We have something like the following:

- (1') Popper's territorial demarcation criteria are designed, roughly, to set out the boundaries of what an epistemic community can know through experience from what that epistemic community cannot know through experience.

⁷In 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), Church (1949)).

- (2') Popper's criterion of falsification explicitly applies to theoretical systems that are not reducible to any possible finite set of sentences we classify as 'empirical' (i.e. 'basic statements').
- (3') Popper's demarcation criterion of falsifiability is one of two demarcation criteria, the other being a criterion of predictability, which explicitly applies to member statements of theoretical systems.

To paraphrase Curd, Cover, and Pincock (2013), the Popperian slogan is better understood as: 'According to Popper, a theoretical system T is classified as 'empirical' or says something about possible experience if and only if T is falsifiable; a statement S is "empirical" or says something about possible experience if S , when conjoined with T , entails E , which is classified as a 'basic statement', and E is not derivable from T alone'.

4 The many erroneous objections from ill-fit

We now have our conceptual house in order. While these three myths have been put to rest, we can now turn to a part-historical, part-philosophical question: how do many of the most popular and well-entrenched objections from ill-fit to Popper's solution to the demarcation problem hold up upon re-examination?

First, it helps to understand the substance of these objections, as well as their prevalence in the literature. These sorts of objections are a form of 'Goldilocks objections': as with the story of Goldilocks and the three bears, Goldilocks enters a house to find three bowls of porridge: the first bowl is too hot, the second too cold, the third just right; she tries out their beds: the first is too hard, the second too soft, the third just right.

Similarly, objections from ill-fit claim to have discovered that the conditions for inclusion or exclusion from a set are either too weak or too strong, that is, adopting these conditions leads to including what should be excluded or excluding what should be included. Carl Hempel notes territorial criteria of demarcation 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 gen-

eral method of objecting from ill-fit has been appropriated to apply to Popper's solution to the demarcation criterion.

In what follows, I set out a number of objections from ill-fit, note how they rely on accepting one, two or all of the three philosophic myths and then correct these mistakes by noting which of the three myths have been uncritically accepted in the construction of these objections⁸.

4.1 Falsifiability is too weak: it lets in false theories

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*, Laudan claims that Popper's demarcation criterion 'has the untoward consequence of countenancing as "scientific" every crank claim which makes ascertainably false assertions' (Laudan 1983, p. 121)⁹.

A slightly different version of this objection from ill-fit is from David Papineau:

Popper's answer to this "problem of demarcation" is that proper sciences, unlike "pseudo-sciences" such as astrology or phrenology, are distinguished by their falsifiability. ... However ... [t]he real difference between the atomic theory of matter, say, and astrology, is that the atomic theory is firmly established by a large amount of evidence, whereas astrology is mere speculation (Papineau 1995).

Papineau raises a similar objection from ill-fit elsewhere:

... [Popper's] account of science is fundamentally flawed. ... The whole point of science is to provide a trustworthy guide to the future (Papineau 2000).

⁸Note: this section may be tedious, pedantic, repetitive or boring; however, it exists, in part, to catalogue the extent of these three myths within the English-speaking philosophical community during the past few decades. If one wishes to skip this section and continue on to §5, little to nothing of importance will be lost other than an understanding of how so many philosophers have accepted these three myths.

⁹Other examples that parrot Laudan can be found in Lakatos (1981, p. 117), Mahner (2013, p. 30) and Boudry (2013, p. 87).

Laudan's objection is as follows: *falsifiability is too weak, for it lets in obviously false statements into the sciences!* We can think of Papineau's objection as the corollary to Laudan's: *falsifiability is too weak, for the sciences should include only 'trustworthy' or 'firmly established' theories!*

An analogy illustrates the absurdity of Laudan's (and Papineau's) reasoning: it would be as fruitful to object to falsifiability of theoretical systems as a territorial criterion (or, for that matter, any demarcation 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. Nevertheless, a synthetic statement can be false and still be categorised as synthetic. Similarly, falsifiable systems can be false (and in fact, for any *true* falsifiable system, there will be an exceedingly large number of corresponding *false* falsifiable systems) and still be categorised as 'empirical'.

Where did Laudan go wrong? Laudan has confused normative and territorial criteria, falling victim to the first myth: since the territorial criterion set out by Popper delineates the absolute limits to empirical inquiry, that is, what can be learned from experience; 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. In fact, it is guaranteed that, so long as at least one basic statement is accepted as 'true', there will be an infinite number of falsifiable theoretical systems that are categorised as 'false'. This feature 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, Laudan's objection rings hollow: empirical systems of statements that are treated as false are empirical because that they are, as best we can know, *known to be false through empirical inquiry*. In fact, Popper recognises this, saying '... theories which have been shown to be false... can nevertheless retain the character of empirical, scientific hypotheses' (Popper 1983, p. xix). 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.

Laudan can be forgiven for confusing territorial and normative demarcation criteria, however, since the territorial and normative problems are often simply

referred to as *'the problem of demarcation'* and Popper (along with Carnap) treats the normative and territorial problems as interrelated. And (as addressed later) such a confusion is practically invited when reading Popper.

While Laudan may not deserve retrospective blame for his mistake, however, individuals that have become aware of the problems with Laudan's objection from ill-fit may deserve ascriptions of blame now that they have discovered the deficiencies with Laudan's reply: it is more of a thought-terminating cliché than an appropriate rejoinder to Popper's criterion of falsifiability.

In sum, Laudan's version of the the objection from ill-fit is not a cogent objection.

4.2 Falsifiability is too strong: individual theories cannot be falsified

One of the most widespread objections from ill-fit can be seen in an objection raised by Philip Kitcher (1982, p. 44), who asserts the falsifiability criterion is too restrictive. In his words, 'one can appeal to naive falsificationism to show that any science is not a science'; Kitcher rightly notes a scientific theory requires auxiliary hypotheses, background knowledge and initial conditions in order to make any predictions, thus no theory qualifies as 'scientific' if it is divorced from a theoretical system¹⁰. Another example of this objection from ill-fit is as follows:

... notable theories of science are typically unfalsifiable by observation statements, because they only make empirical predictions in association with certain auxiliary theories. Should any such prediction turn out to be false, logic does not compel us to regard the principal theory as untrue, since the error may lie in one or more of the auxiliaries.' (Howson and Urbach 2006, p. 104)

In sum, Kitcher's objection is: *falsifiability is too strong, for it applies to individual theories, and individual theories are not falsifiable!*

¹⁰See also Thornton (2016) and Putnam (1974) for other examples in the literature.

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, Kitcher has fallen victim to the first myth, as addressed in §1, that Popper's demarcation criterion sets out the boundaries of the natural sciences from non-science. In reality, the purpose of the falsifiability criterion is not about whether a theory is *scientific* or *non-scientific*; falsifiability is a criterion for *empirical significance* (or, perhaps, to use Popper's turn of phrase, 'empirical science').

Second, Kitcher has accepted the second myth, that falsifiability applies solely to singular theories. However, as addressed in §2, falsifiability applies to *theoretical systems*: singular theories are *not* what is demarcated by the criterion of falsifiability; rather, *theoretical systems* are (Popper 1959, p. 25)¹¹.

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.

Kitcher's mistake is to take Popper at his word (at least in his most widespread pronouncements): it is easy to conclude that when Popper uses the word 'theory' in one context that he is referring solely to a universal statement; however, Popper will often clarify in a later passage in his books and articles that his use of the word is idiosyncratic; he refers (most of the time) to a large theoretical system of theories, auxiliary statements, basic statements and analytic and metaphysical statements.

Thus Kitcher, along with other philosophers that object to the criterion of falsifiability on the basis that it applies to individual theories, rather than to theoretical systems as a whole, will be thankful to discover that Popper is fully in agreement with his critics, and anticipated these objections in the formulation of his falsifiability criterion. So much for the objection that since falsifiability applies to individual theories the falsifiability criterion is too strong.

¹¹This objection from ill-fit has rightly been (quietly) corrected previously by Miller (1994) and Worrall (1995), for example.

4.3 Falsifiability is too strong: falsifications must be certain / falsifications are conjectural

Another objection from ill-fit that often appears in the literature can be put as follows:

... as an attempt to understand the practice of science, Popper's ideas bear little fruit. First of all, the claim that scientific theories are falsifiable by "possible, or conceivable, observation" raises a difficulty, because an observation can only falsify a theory (in other words conclusively demonstrate its falsity) if it is itself conclusively certain. Yet as Popper himself appreciated, no observation falls into this category; they are all fallible.' (Howson and Urbach 2006, p. 103)

We were face an objection from ill-fit that focuses on a popular version of the Duhem thesis. We can frame this holist objection as follows: *the criterion of falsifiability is too strong, for it is not possible to ever conclusively falsify a theory!*

A similar holist objection is present in Hübner (1978):

But every falsification, too, we can reply, has some premises, such as axioms of certain observational theories. Now if these premises are conjectural ... the falsification is conjectural too. This conjecture may be purely arbitrary, and consequently the falsification would be practically meaningless (Hübner 1978, p. 280).

This similar objection can be framed as: *the criterion of falsifiability is too strong, for any purported falsification is necessarily conjectural!*

One last holist objection comes from Feyerabend:

A principle of falsification that removes theories because they do not fit the facts would have to remove the whole of science (or it would have to admit that large parts of science are irrefutable)' (Feyerabend 1975, p. 303)

We can summarise this objection as: *all theories contradict accepted empirical statements, therefore all theories that are classified as empirical are either false or non-empirical!*

Now these objections present by Howson, Hübner and Feyerabend are, in part, correct, but *only in the most trivial sense*, since we are interested in what sorts of statements and theoretical systems should be classified as *empirical*, not whether we can determine whether certain empirical statements are known to be true or false.

As is the case of Laudan's objection, it would be as helpful to object to falsifiability as a demarcation criterion on the grounds that one cannot know with a high degree of confidence that certain falsifiable statements are false as it would be to object to a criterion for syntheticity on the grounds that that one cannot know with a high degree of confidence that certain synthetic statements are true or false. Thus one can grant for the sake of argument that no empirical statement can *ever* be known to be true or false and nevertheless accept the proposed demarcation criteria from Popper:

A statement or theory is, according to my criterion, falsifiable if and only if there exists at least one potential falsifier—at least one possible basic statement that conflicts with it logically. It is important not to demand that the basic statement in question be true. The class of basic statements must be characterized in such a way that a basic statement describes a logically possible event of which it is logically possible that it might be observed' (Popper 1983, p. xx)

The mistake that Howson and Hübner make is to adopt a subsidiary myth, namely that Popper's modal account—i.e., a theoretical system is falsifiable if it can contradict a *possible* statement classified as 'empirical'—hinges on the modal account being satisfied, *viz.* that Popper's criterion is not one of falsifiability but of falsification occurring in practice. But it is far more appropriate to understand Popper's criterion not as one of *discovering that a theoretical system is false*, but rather that *the theoretical system contradicts a basic statement*—that is, a statement previously uncontroversially classified as 'empirical' by an epistemic community—thus maintaining a strict deductive relationship between a large conjunction of statements that are not obviously classified as 'empirical' and statements that are uncontroversially classified as such. If history were to be replayed with the knowledge that Popper's criterion would lead to this problem of equivocation, perhaps

rather than adopting the label ‘falsificationism’, a more appropriate label would have been ‘possible empirical contradictionism’, ‘conditional testability’ (or, to save space, ‘contradictionism’) and theoretical systems must be contradicted by a possible basic statement.

In sum, we see that these three versions of the objection from ill-fit fail to engage with the demarcation criteria Popper proposed on his own terms, conflating knowledge that a theoretical system is false with whether a theoretical system contradicts a possible basic statement. So much for this form of objection from ill-fit.

4.4 Falsifiability is too weak: Popper’s criteria for ‘empirical’ statements is arbitrary

A fairly subtle objection (that hinges on a subtle mistake) is exemplified as follows: ‘...rejecting of theories for Popper ... [is] a matter of mob psychology’. This leads to the uncomfortable conclusion that any acceptance of a basic statement and its corresponding theoretical system ‘paves the way for any arbitrary system to set itself up as an “empirical science” (Newton-Smith 1980, p. 152)¹².

We can rephrase this objection as follows: *the criterion of falsifiability is too weak, for Popper’s criteria for basic statements is arbitrary, thus any theoretical system can be classified as ‘empirical’!*

What appears at first glance to be a powerful objection from ill-fit, however, can be turned on its head: Popper’s demarcation criterion is dependent on his conception of ‘observation statements’ or ‘basic statements’; however, Popper leaves it up to the epistemic community to decide on whatever standards should be adopted for accepting or rejecting ‘observation statements’.

Some epistemic communities will track extra-linguistic matters of fact better than others, but if we are interested in *permitting* the chance that epistemic communities can seriously err in their classification of ‘observation statements’ or ‘basic statements’ as such (so long as they attempt to describe the existence

¹²Note that Popper objected to Neurath’s doctrine in much the same way, *viz.* ‘One is allowed ... simply to “delete” a protocol sentence if it is inconvenient’ (Popper 1959, p. 97).

or nonexistence of entities in a spatio-temporal region), Popper's criteria will invariably lead to some epistemic communities classifying theoretical systems as 'empirical' on this basis.

But, notably, this division does *not* correspond to any extra-linguistic matters of fact; rather, Popper's solution to the demarcation problem hinges solely on the possible acceptance or rejection of basic statements by an epistemic community. This solution relies on particular rules or norms followed by an epistemic community, even if these rules or norms are not particularly good. It is to be expected that for some epistemic communities, they will treat some statements as basic statements (and any theoretical system that has a deductive relation with these basic statements will be classified as 'empirical'); other epistemic communities will, due to their standards of acceptance or rejection, treat these basic statements (and related theoretical systems) as non-empirical.

Furthermore, some epistemic communities will have greater *epistemic access* than others, and will have available to them forms of technology that impinge on what exists (or what is purported to exist), such as telescopes, microscopes, SONAR, RADAR, compasses, timekeepers, and so on; other epistemic communities will be limited to, by their material and social conditions, to a comparatively smaller amount of basic statements. Thus what is classified as a 'basic statement' will be indexed to a particular epistemic community's epistemic abilities, broadly construed. This, however, is to be *expected* if a demarcation criterion is to be territorial *and* not map on to any extra-linguistic categories, but rather pick out what is to be classified as 'empirical' *for an epistemic community*.

Popper's later approach, as well as the approaches adopted by many of his students, setting out proposals for when it is appropriate to accept or reject basic statements, must thread the needle between Neurath's conventionalism and tracking truth: there are certain possible structures of epistemic communities that are more desirable than others because these possible epistemic communities have more appropriate standards and greater epistemic access than their rivals, thus their standards, abilities and 'basic statements' are just the sort that are desirable to instantiate when engaging in empirical inquiry. Whether a hypothetico-deductive approach such as Popper's can be successful is a different discussion for another day; however, these normative concerns do not undermine Popper's

criteria.

In sum, to target Popper's territorial criteria on *these* terms, that is, that it fails to provide a strong tether between basic statements and truth, thereby preventing epistemic anarchy, fails to grasp that the criteria describes the territorial boundaries of what can be learned from experience by particular epistemic communities. Newton-Smith's objection relies on taking the criterion of falsifiability as expressing some extra-linguistic or extra-social fact, separate from the epistemic community; but that is not what the criterion attempts to do, thus this objection from ill-fit is mistaken.

4.5 Falsifiability is too weak: it lets in metaphysical theories

Donald Gillies presents another subtle objection to the falsifiability criterion by way of the 'tacking paradox' or tacking problem (Gillies 1993, p. 210). In it, Gillies claims the falsifiability criterion fails to 'exclude obvious metaphysical statements', and this failure can be shown as follows: take an arbitrary theoretical system T that is falsifiable, then append to it a statement M that is metaphysical. Hence $T \cup M$ is empirical. But then M is empirical. Therefore metaphysical statements are empirical. But this conclusion is absurd, therefore the criterion of falsifiability is too weak.

A similar objection from Nicholas Maxwell is for the superfluosity of falsifiability:

If Popper's solution to the demarcation problem were basically sound, it would place a serious limitation on the scope and viability of natural philosophy, which is based on the *integration* of science and metaphysics. But it is not sound. It is quite fundamentally defective. (Maxwell 2016, p. 175)

Maxwell's objection is that certain assumptions in the natural sciences are accepted as fundamental in theory-preference, such as the principle of *unification*, thus

Being neither falsifiable nor verifiable, the assumption is metaphysical. ... *But this assumption, despite its metaphysical character, is nevertheless*

less such a secure part of scientific knowledge that endlessly many theories, empirically more successful than accepted theories, are rejected (or rather are not even considered) solely because they conflict with the assumption. (Maxwell 2016, p. 177).

This leads to an integration of metaphysics and science. We do not *need* falsifiability. It is easier to address Maxwell's criticism by first addressing Gillies'.

As Keuth (2005, p. 47) rightly notes,

But why should [the conjunction] be obviously metaphysical? Gillies seems to assume tacitly that, according to Popper's criterion of demarcation, scientific statements must not have any metaphysical content. Actually, the criterion excludes only statements that do not have any empirical content.

In short, one problem with Gillies' objection is as follows:

All falsifiable hypotheses have unfalsifiable consequences: logical truths inevitably, and weak metaphysical statements in most cases. It does not follow that these unfalsifiable statements qualify on their own account as scientific, nor that the falsifiable hypotheses in whose content they reside are unscientific. The presence of dead cells within its body does not deprive an organism of life, and the vitality of the organism is not transmitted to the cells that have died. (Miller 2006, p. 96)

Less metaphorically, theoretical systems can be classified as falsifiable or not; if they are correctly classified as falsifiable, the theoretical system has empirical content. But nothing forbids the theoretical system from having a far greater amount of non-empirical content (which it will inevitably have).

But consider a more problematic objection, which is a variation on the tacking problem: the falsifiability criterion does not exclude any obvious metaphysical statement *M*, for the conjunction of all statements will be, by Popper's criteria, classified as 'empirical'. Thus the falsifiability criterion is *trivial*, for it does not

exclude any statement from being classified as a member of an empirical theoretical system and thus 'empirical'.

While Miller and Keuth set out plausible objections to the tacking paradox, it's worth considering the limits of 'orthodox' Popperian replies: there is the almost universal failure to focus on Popper's neglected second criterion for whether member statements of a theoretical system are classified as 'empirical'.

A more 'heterodox' response to this objection would run as follows: Popper's *second* criterion of demarcation for *sentences* prevents Popper's criterion of falsification from reducing to a triviality in this instance, since although any metaphysical statement M may be conjoined with a theoretical system T , one can determine if M is empirical or not by seeing whether M , if conjoined with T , entails a statement classified as 'empirical' that was not present in T alone. Therefore there exists criteria that exclude some statements (if we choose to exclude them) as riders that are carried along into theoretical systems by the coattails of a theoretical system, and are identifiable as such in much the same way analytic statements or statements from mathematics or logic are similarly identifiable.

By analogy, passengers on a train may or may not have tickets to their destination; if the conductor requests a ticket and none can be provided, the conductor may request the passenger leave the train at the next stop. Thus Gillies' objection from ill-fit falls flat, and with it goes Maxwell's: demarcation criteria may ultimately be of little use to practicing scientists or philosophers; nevertheless, they serve a background function in being able to clarify which parts of an empirical theoretical system are predictive and which are not. As Popper notes:

... the significance of the demarcation [criteria], if any, should not be overrated. In spite of this, I contend that the problem of demarcation is highly significant. It is so, not because there is any intrinsic merit in classifying theories, but because a number of genuine and important problems are closely linked with it; in fact, all the main problems of the logic of science (Popper 1983, p. 161)

A solution to the demarcation problem is valuable, in part, because it reveals the underlying assumption made by many empiricists a theory of induction is desirable *in order to* classify theories as empirical or non-empirical. But such an

assumption is unwarranted. Furthermore, the significance of demarcation criteria helps clarify *which parts* of an empirical theoretical system have predictive power.

4.6 Falsifiability is too strong: it excludes conjunctions of basic statements

Brand Blanshard presents aversion of the objection from ill-fit as follows:

You can falsify, but not verify, the proposition that all swans are white; true. But unhappily, though you can verify, you cannot falsify, the proposition that *some* swans are white. Why not? Because, since it is a logically particular proposition, you can contradict it only by establishing a universal proposition, and this we have just seen to be impossible through empirical means. To falsify 'some swans are white' you must verify that no swans are white. (Blanshard 1962, p. 228)

Blanshard's version of this objection is, as follows: *statements of the form 'some X are Y' are not falsifiable yet they are obviously empirical; they can only be confirmed, therefore falsifiability is too strong!*

The reply to Blanshard is as follows: conjunctions of basic statements are themselves basic statements. And recognising that there are two criteria of demarcation, one for statements predicting basic statements, and one for theoretical systems, puts this objection to rest.

4.7 Falsifiability is too strong: it excludes isolated existential statements

A series of objections from ill-fit are, at least at first glance, quite debilitating to the Popperian programme:

This [demarcation criterion] commits him to the paradoxical thesis that all unrestricted existential propositions ...[e.g. 'There exists at least one *X* that is *Y*'] are metaphysical and nonempirical. To call

them all metaphysical is very curious, because most of them have nothing at all to do with metaphysics as understood by Aristotle or any other philosopher until the positivists of this century began to use 'metaphysical' in a grossly extended sense for purposes of abuse. (Kneale 1974, p. 206)

Thus isolated existential statements are not falsifiable, but isolated existential statements are not metaphysical, and it is an abuse of language to classify these statements as 'metaphysical'!

An obvious reply to this objection would be to note that the classification system set out by Popper is, *contra* Popper's use of the term 'metaphysical' in much of his writing, much better characterised as between 'empirical' and 'non-empirical': metaphysical statements, analytic statements, statements in logic and mathematics and some synthetic and *a posteriori* statements are categorised as 'not empirical' or 'non-empirical', not as 'metaphysical'¹³.

But suppose there is a stronger objection from ill-fit. How can we try to formulate it? Perhaps, as Kneale believes:

Suppose that on some occasion when I have been rashly dogmatic about the behaviour of fish out of water a biologist reminds me, in an unrestricted existential statement, that there are after all lungfish which can breathe on dry land (Kneale 1974, p. 207) (207).

This objection is not quite right either: here we have a case of equivocation, since what really happens here is that it is an elliptical statement taking the place of a statement about lungfish *that have been observed*. To point to a cat on a mat and say 'There exists a cat on a mat' is, at least intuitively, a loose expression for

¹³Miller (1994, p. 83) provides an elementary example:

... [the] hypothesis that physical objects go out of existence when no observer (person, animal, or instrument) is observing them [is synthetic]. Skillfully formulated, this hypothesis simply cannot be tested.... So it may not, Popper says, be introduced into empirical science.

a more detailed one specifying ‘There exists a cat on a mat *here at this time*’. Similarly, when one accepts an unrestricted (or isolated) existential statement of the form ‘There exists an X that is Y ’, it is frequently taken as an elliptical expression for ‘There exists an X that is Y at spatio-temporal region Z ’, or some other statement that satisfies Popper’s second criterion for the empirical status of individual sentences.

A third attempt by Kneale in setting out an objection from ill-fit is as follows:

Furthermore, there is a strange departure from the ordinary use of words in maintaining that hypotheses of natural law are empirical, because they are open to refutation by experience, while denying that unrestricted existential propositions can ever deserve the same title.
(Kneale 1974, p. 207)

In sum, we now have what appears to be a strong objection from ill-fit: *isolated or unrestricted existential statements are not falsifiable; however, they are obviously empirical, therefore falsifiability is too strong!*

Here we have two versions of an objection from ill-fit that has some weight to it. As Popper recognises,

Purely existential statements are not falsifiable—as in Rudolf Carnap’s famous example ‘There is a colour (‘Trumpet-red’) which incites terror in those who look at it.’ Another example is: ‘There is a ceremony whose exact performance forces the devil to appear.’ (Popper 1983, pp. xx–xxi)

Blanshard is correct in his assessment, but recall the neglected second criterion of demarcation for *sentences*: a sentence S is classified as ‘empirical’ so long as S , in conjunction with a theoretical system T , churns out more sentences that are classified as ‘empirical’ by an epistemic community than exist in T alone.

We can attempt to strengthen Kneale’s objection once more by introducing the neglected second criterion: *isolated or unrestricted existential statements are not falsifiable nor are they predictive; however, they are obviously empirical, therefore falsifiability and predictability are too strong!*

Popper's approach in *Replies to My Critics* and his *Postscript* is subtle in its execution. If we need not introduce a form of inductive logic as Carnap did, a more limited conception of what should be classified as 'empirical' than Carnap's will invariably leave out isolated existential statements, for isolated existential statements may be confirmable in principle, but isolated existential statements do not entail any basic statements (although they, like analytic statements, are trivially derivable from them).

Popper provided such an explanation several times, and it does not need to be elaborated on at any length, for as far as I can determine by examining the literature, there has been as of now no available reply to Popper's retort:

I hope this example will also provide a reply to the often repeated criticism that it is perverse to exclude purely existential statements from empirical science... My example consists of the following purely existential theory: "There exists a finite sequence of Latin elegiac couplets such that, if it is pronounced in an appropriate manner at a certain time and place, this is immediately followed by the appearance of the Devil—that is to say, of a man-like creature with two small horns and one cloven hoof." Clearly, this untestable theory is, in principle, verifiable. Though according to my criterion of demarcation it is excluded as non-empirical ... It would of course be easy enough for me to amend my criterion of demarcation so as to include such purely existential statements among the empirical statements. ... But I believe that it is better not to amend my original falsifiability criterion. For our example shows that, if we do not wish to accept my existential statement about the spell that summons the devil, we must deny its empirical character (Popper 1962, pp. 249–50).

Coupled with the objection against Ayer and Carnap's criteria in Church (1949), we are left with a compelling reason to think that introducing a further criterion of demarcation that includes isolated existential statements will invariably lead to criteria of demarcation that are far too broad.

Without further motivations to adopt a broader construal of what should be classified as 'empirical' then isolated existential statements are *not* obviously 'em-

pirical' statements (although they may be synthetic or *a posteriori* statements) because (1) they fail to satisfy Popper's criteria and (2) there is the competing intuition that some synthetic *a posteriori* statements of the form "There exists an *X* that is *Y*" are different in their construction than existential statements that do specify the existence or nonexistence of entities in spatio-temporal regions, *viz.* statements about, for example, ceremonies that invoke the devil, that prevent epistemic access of a community.

However, there is a further response that is quite simple, somewhat pedantic, and on its own provides sufficient grounds for rejecting this objection from ill-fit without appealing to conflicting intuitions or a further objection from ill-fit that Ayer or Carnap's solutions are too permissive: first, falsifiability applies to theoretical systems, not individual statements. Are isolated or unrestricted existential statements empirical if they are conjoined with a theoretical system? (That is, do they entail at least one basic statement?) No, because isolated existential statements do not specify any spatio-temporal region. Thus they are not empirically predictive.

Furthermore, a pedant may say, in response to the concern that a strictly existential statement is not falsifiable by noting that neither are isolated universal statements, or for that matter any statement that is isolated from a theoretical system, even if, on its own, it specifies the existence or nonexistence of certain entities or events at specific spatio-temporal locations.

Strictly existential statements are, in virtue of being isolated from such a theoretical system, *not* a large conjunction of theories, auxiliary hypotheses, background statements, and theory-laden observation reports; in fact, strictly existential statements are typified as being divorced from such a conjunction (although they are, like analytic sentences, derivable from it). Nor are strictly existential statements predictive. Thus isolated existential statements are not classified as 'empirical' according to these standards.

If we accept these two demarcation criteria, we would naturally refuse to accept these statements (along with analytic statements) as being empirically predictive; they are, like analytic statements, hangers-on that ride on the coattails of the empirical system. By the lights set out in these two demarcation criteria, isolated existential statements are not empirically significant.

An obvious rejoinder to this reply would be to note that one is simply appealing to the standards in dismissing classifying isolated existential statements as 'empirical', thus this reply is a form of circular reasoning. But is it a vicious form of circular reasoning?

Set aside how Popper's reply to Kneale puts to rest the attempt to appeal to intuitions (it would seem we have a *prima facie* reason to be sceptical of these intuitions), along with the concern that broadening criteria to include strictly existential statements will itself be subject to an objection from ill-fit. I believe it does provide a *further* reason: it demonstrates the *independence* of demarcation criteria from other conceptual categories: we have innumerable cases in which synthetic *a posteriori* statements are not classified as 'empirical'. Popper's demarcation criteria do *not* depend on distinctions between the analytic/synthetic, *a priori/a posteriori* or 'matters of fact' and 'relations of ideas'; this solution to the territorial demarcation problem is focused on the limitations of an epistemic community, namely what sentences are classified as 'basic', and which theoretical systems have a logical relation to these basic sentences. This is, I think, a surprising and valuable result, especially given the assumption that demarcation criteria will inevitably have a dependence relation on these other categories.

In sum, if it should be that the demarcation between 'empirical' and 'non-empirical' should lead to individuals like Kneale feeling unsettled that some statements that they believe should be classified as 'empirical' now being classified as 'non-empirical', they will need more than appeals to intuition to undermine proposed demarcation criteria.

4.8 Falsifiability is too strong: it excludes probabilistic statements

The last objection from ill-fit is, at least historically, thought to be a bugbear to Popperians: '...probability would deal a fatal blow to falsifiability.' (Hacohen 2002, p. 253). Donald Gillies (1971, p. 231) explains: '... statistical theories appear in much of reputable science; yet they are not, strictly speaking, falsifiable by the frequency evidence which is used in practice to test them'. The objection runs as follows: *probabilistic statements are not falsifiable (nor are they obviously empirically predictive), nevertheless probabilistic statements are classified as 'empirical'; therefore, Pop-*

per's criteria are too strong.

There is something genuinely concerning about this objection, but upon further reflection, recall that it is only the first step in the dialogue: §§8-9 of *Logic* set out a number of plausible conventionalist stratagems for treating probabilistic statements as empirical, such as introducing specific rules for acceptance and rejection of probabilistic statements into the theoretical system.

An examination of much of the literature in statistics, such as the Neyman-Pearson theory of testing (Neyman and Pearson 1967), as well as guiding rules in scientific practice reveals that as a matter of description of the practice of scientists, as Popper (1959, p. 204) notes, ‘... the physicist knows well enough when to regard a probability assumption as falsified’.

To be a bit more rigorous, if we are to take demarcation criteria as indexed to the standards and practices of an epistemic community, then if some communities of scientists are willing to treat probabilistic statements as falling outside the acceptable error margins of a set of basic statements, they are both classified as ‘empirically predictive’ when conjoined with a relevant theoretical system (for they provide a prediction of what frequencies should be expected) and are part of a theoretical system that is itself classified as ‘falsifiable’ (for if, hypothetically, the predictions did not come to pass, the probabilistic statement and its conjuncts, for all intents and purposes, would be treated as if they contradicted basic statements).

On the normative side, Donald Gillies (1971) tackled this problem head-on in articulating falsifying rules for probability statements; Deborah Mayo (1966) addresses other approaches towards providing normative rules based on Neyman-Pearson theory, as does Richard Braithwaite (1953).

Upon reflection of how the dialectic in this instance progressed, that is, Popper preempted a number of his critics by presenting a response to this objection from ill-fit in the second half of *Logic*, and much work by philosophers and statisticians since its publication has been done since to improve on this work; however, this objection from ill-fit often has been left hanging there, as if floating on air, with little to no acknowledgement outside this sub-discipline that a principled response to the objection was available. It is as if the investigation into objections to demarcation criteria began and ended with the positing of the objection.

4.9 A taxonomy of objections from ill-fit

Given the numerous different objections from ill-fit, here is a brief summary of which myths each objection adopts:

The objection from ill-fit	First myth: extra-social demarcation of science vs. non-science	Second myth: only individual theories are falsifiable	Third myth: there is only one demarcation criterion
Laudan (1983), Mahner (2013) and Papineau (2000): Lets in obviously false statements into the natural sciences. Therefore too weak .	X: Claims it is normative; it is territorial. Territorial criteria will let in false statements. X: Furthermore, about empirical science, not natural science.	(Makes no mention that theoretical systems are what is falsifiable.)	(No mention of the second criterion.)
Newton-Smith (1980): If any statement is classified as 'basic' then any theoretical system can be classified as 'empirical'. Therefore too weak .	X: Assumes extra-linguistic criteria for what is empirical and non-empirical. Truth: socially-determined classification system.	(Acknowledges that theoretical systems are falsifiable.)	(No mention of the second criterion.)

The objection from ill-fit	First myth: extra-social demarcation of science vs. non-science	Second myth: only individual theories are falsifiable	Third myth: there is only one demarcation criterion
<p>Kneale (1974): Isolated existential statements are synthetic <i>a posteriori</i>. Synthetic <i>a posteriori</i> is identical with 'empirical'. Therefore too weak.</p>	<p>X: Assumes what is 'empirical' is identical to synthetic <i>a posteriori</i>.</p>	<p>(Compatible in principle with theoretical systems being what is falsifiable.)</p>	<p>(Neglects the second criterion of demarcation, but nevertheless compatible with it.)</p>
<p>Howson and Urbach (2006): Theories can never be conclusively falsified. Therefore too strong.</p>	<p>X: No falsification need ever occur.</p>	<p>(No mention that falsifiability applies to theoretical systems.)</p>	<p>(No mention of the second criterion.)</p>

The objection from ill-fit	First myth: extra-social demarcation of science vs. non-science	Second myth: only individual theories are falsifiable	Third myth: there is only one demarcation criterion
Hacohen (2002): Probabilistic statements are not falsifiable [nor are they predictive] but classified as 'empirical'. Therefore too strong .	X: Assumes eternal, extra-linguistic or extra-social criterion; rather, dependent on epistemic community's standards and epistemic access.	Doesn't acknowledge theoretical systems are only falsifiable.	(Neglects the second criterion of demarcation.)
Kitcher (1982) and Howson and Urbach (2006): Individual theories cannot be falsified. Therefore too strong .	It is the empirical sciences, not the natural sciences;	X: Falsifiability applies only to theoretical systems.	(No mention of the second criterion.)

The objection from ill-fit	First myth: extra-social demarcation of science vs. non-science	Second myth: only individual theories are falsifiable	Third myth: there is only one demarcation criterion
Gillies (1993) and Maxwell (2016): Tacking paradox entails criteria let in metaphysical theories. Therefore too weak .	(Rightly notes that it is the distinction between empirical and non-empirical.)	(Acknowledges only theoretical systems can be falsifiable.)	X: Neglects second criterion of demarcation. We can determine which parts of a theoretical system are classified as 'empirical'.
Blanshard (1962): Conjunctions of basic statements are not falsifiable. Therefore too weak .	(Acknowledges dealing with empirical and non-empirical.)	Assumes only statements are falsifiable, not theoretical systems.	X: Neglects the second criterion of demarcation. If second criterion introduced, objection answered.

What can we see from this taxonomy?

First, almost all objections from ill-fit neglect to mention there is a second criterion of demarcation. This result is not surprising. Even many 'orthodox' Popperians neglect this criterion.

Second, the objections range in nuance: Laudan (1983) and Papineau (2000), for example, make at least two relatively basic errors (the assumptions that Popper's demarcation criteria are normative and demarcate natural science from non-science or pseudo-science), while Hacoen (2002) and Kneale (1974) miss out on

the subtle point that Popper's demarcation criteria aren't tied to extra-linguistic or extra-social matters of fact.

Third, all objections from ill-fit addressed here are dependent on accepting at least one of these three myths; in fact, all three myths are assumed at least once.

Fourth, the prevalence of types of objections predicated on assuming one myth does not always track the frequency of objections from ill-fit in the literature: the objections raised by Kitcher (1982) (predicated on the assumption that falsifiability applies only to individual theories) and Laudan (1983), are more frequently cited than more nuanced objections like the one raised by, for example, Kneale (1974).

Fifth, there are numerous, incompatible interpretations of Popper's criteria: for example, Blanshard (1962), Gillies (1993), Maxwell (2016) and Newton-Smith (1980) rightly acknowledge that the demarcation criteria deal with the distinction of 'empirical' and 'non-empirical'; Laudan (1983), Papineau (2000) and Kitcher (1982) fail to acknowledge this. Laudan (1983), Papineau (2000) and Kitcher (1982) fail to mention that only theoretical systems are falsifiable while Gillies (1993) and Newton-Smith (1980) explicitly acknowledge this (all others don't this explicit).

Sixth, some objections, such as the objection raised by Curd, Cover, and Pincock (2013), incorporate at least two objections raised previously ('[F]alsifiability is both too weak and too strong.').

Seventh, no objections that assume the second myth conclude the criteria are too weak; no objections that assume a version of the third myth conclude the criteria are too strong. However, it may not be difficult to intentionally construct misrepresentations of Popper's demarcation criteria and conclude that, assuming the second myth, that the criteria are too weak or that, assuming the third, that the criteria are too strong.

Eighth, it is trivial to generate a number of variations of these objections by adopting any number of the premises eight objections from ill-fit.

5 Where did these origins from ill-fit come from?

An inquisitive philosopher may ask, upon reflecting on all the misguided objections set out in §4: *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.

There is the further concern that Popper is not trustworthy in his autobiographical works, insisting in his intellectual autobiography (Popper 2002) and *Conjectures and Refutations* that he developed his theory of demarcation in 1929 when it is likely he did not develop the theory until 1931 or early 1932 (Hacohen 2006)

Here is, I believe, a far more reasonable explanation that is also parsimonious: philosophers don't usually take the time to read through the entire works of other philosophers. If the philosopher is generally considered to be old hat or outdated then why bother? This, of course, only describes the general attitude of philosophers towards another philosopher seen today; it doesn't describe *how* this attitude developed. If this dismissive attitude is not due to a conspiracy, then, where could it come from?

The objections from ill-fit looks *prima facie* promising upon a surface reading of Popper's 1959 book, *The Logic of Scientific Discovery* and his 1962 collection, *Conjectures and Refutations*, as well as a surface reading of a number of his other books. Thus there is something wrong: Popper's books practically invite the reader to engage in a rudimentary mistake in exegesis due to failures in signposting and the use of rarely explained or confusing technical terminology.

For example, there is, as mentioned previously, an almost constant conflation in Popper's writings. The technical term 'theory' stands for 'theoretical system' except when it does not. The term 'science' means, surprisingly enough, 'empirical science'. This is a genuine case of a philosopher that wrote poorly, and it requires a painfully extensive and broad reading of his published work to grasp the nuances to his thought on demarcation. However, such work is immensely time-consuming, and with little obvious benefit to do so. Thus philosophers and historians of philosophy see little reason to attempt such a task.

The reason for the perpetuation of these mistakes is, I believe, simple: *Karl Popper was not a clear writer*. That is not a particularly damning conclusion. It is difficult to be a clear writer. Many philosophers are not clear in their writing. However, in this instance, lack of clarity and precision has led to perpetuation of these myths: there exist numerous cases of Popper failing to signpost both that when he refers to ‘scientific theories’ he means ‘empirical-scientific theoretical systems’.

5.1 The origins of the first myth

Popper’s failings provide a helpful guide for present-day philosophers: be clear upfront about terminology, signpost important but subtle points, and be clear when one shifts from one problem to the other.

An attentive philosopher may interject at this point, reflecting on the apparent myth set out in §1: *What is meant by the ‘empirical sciences’, then? Surely Popper is speaking about the limits of the sciences, not the limits of empirical inquiry!*

There must be some *explanation* for why so many philosophers have routinely erred in basic exegesis. Here is one major problem with Popper’s translation of *Logik der Forschung*, as well as his choice of language and belated attempts at clarification in his later works, that has led to the perpetuation of the myth that falsifiability is a criterion that sets out the boundaries of the natural sciences from non-science (or pseudo-science).

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 of its technical use, 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.

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.

How did such a failure to signpost a key technical term enter the English translation of *Logik* and subsequent English books by Popper? The translators of the English translation of *Logik*—Popper, Julius and Lan 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.

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]'.

This very failure in understanding on Popper's part is exemplified in an end-note in *The Myth of the Framework*: 'Since in English "science" means "empirical science", and since the matter is sufficiently fully discussed in my book...' (Popper 1997, 29, en. 41). But this is not the case.

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.

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 directly helped this myth spread. 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. 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 'empirical scientific', '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 dis-

course, and reflected as such in the original German edition.

This exact same problem of using technical language involving the distinctions between ‘empirical science’ and ‘natural science’ without immediate clarification occurs in Popper’s *Die beiden Grundprobleme der Erkenntnistheorie*¹⁴, *Conjectures and Refutations* (Popper 1962, pp. 249–50), *Replies to My Critics* (Popper 1974), *Realism and the Aim of Science* (Popper 1983) and *The Myth of the Framework* (Popper 1997, p. 29).

In fact, after an extensive survey of Popper’s published oeuvre, any attempt to clarify what is to be demarcated leads to difficulty, since Popper will frequently refer to the empirical sciences, that is, *empirische Wissenschaft*, as ‘science’ (cf. Popper 1974, p. 981).

At other points Popper does not clarify what ‘pseudo-scientific’ means in particular contexts, that is, whether theories that are classified as ‘pseudo-scientific’ are theories that are pretenders of ‘empirical science’ or the natural sciences. This failure to clarify these key terms is exemplified in, for example, *Realism and the Aim of Science*: ‘... how can you distinguish the theories of the empirical sciences from pseudo-scientific or non-scientific or metaphysical speculations?’ (Popper 1983, p. 159). A charitable reading, perhaps the only one that makes sense, would be as follows: ‘how can you distinguish the [theoretical systems] that are [empirical] from [pseudo-empirical, non-empirical and] metaphysical speculations?’

In other contexts, once Popper has moved on to deal with normative and methodological problems, it is far more plausible to read Popper as setting out distinguishing marks of *desirable* modes of empirical investigation from *undesirable* modes. One likely answer to this misunderstanding is as follows: Popper turned his attention elsewhere in his later writings, specifically to the *normative* problem of demarcation; consequently, his later popular writing in English reflecting on his previously-held positions is at times incredibly opaque.

It is only determinable from context whether he is referring to whether certain *decisions or actions of an epistemic community* are ‘pseudo-scientific’ (i.e., dis-

¹⁴In set in draft form sometime between 1930 and 1933, published in German in 1979 and translated into English as *The Two Fundamental Problems of the Theory of Knowledge*, with the problem of demarcation clarified only slightly in the second half of the book (Popper 2009, pp. 383–85).

preferable) 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. Similarly, it is only through careful reading that one can see whether Popper is speaking of individual sentences or theoretical systems.

5.2 The origins of the second myth

Let us move to the myth that only individual theories were falsifiable (Due to the similarities in the origins of this myth, not as much space will be dedicated to providing details for how Popper failed to clarify his remarks). 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?

The answer, again, is that Popper is not clear in his speech. This isn’t to be faulted, since Popper, like many philosophers, had to develop the conceptual scaffolding with which to construct his philosophical structure. Early attempts will almost invariably struggle to articulate the underlying thoughts.

This opaqueness is most noticeable in his collected volume of 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).

How are we to understand this passage? Were one to read *Conjectures and Refutations* they may conclude two things: Popper’s falsifiability criterion applied to both singular theories and theoretical systems¹⁵, this is the sole criterion of demarcation, and Popper’s criterion demarcates the natural sciences 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.

¹⁵An interpretation made more subtle if we remember that Popper believed that his predictive criterion for individual statements was reducible to his criterion of falsifiability.

Of course, for almost every case of Popper clarifying that he meant ‘theoretical systems’ rather than singular ‘theory’, there are dozens of other instances immediately preceding it in which he uses the word ‘theory’ without any signposting. In this case, Popper’s failure to signpost his idiosyncratic use of the word ‘theory’ is revealed only under a close reading, requiring an investment in time, energy and resources that most philosophers cannot be bothered with: why spend any more time on the problem of demarcation or Popper’s proposed solution to it when it is already common knowledge that Popper’s solution is undermined by any or all of the objections from ill-fit set out in §4?

5.3 The origins of the third myth

Let us turn to the origins of the third myth: the almost universal neglect of the second demarcation criterion of predictability, sadly, is most plausibly due to another failure on Popper’s part. This failure is not due to a difficulty in grasping some deep conceptual aspect of his proposed solution to the demarcation problem, but (most plausibly) his failure to convey the implicit steps in his thinking: according to Popper, individual statements are classified as ‘empirical’ if they are predictive, but predictive statements are *also* falsifiable (in a way). This is an inevitable consequence of the criterion of predictability.

However, Popper does not note that this requires fixing the rest of the theoretical system in such a way that it becomes unchangeable background knowledge (at least for the moment). This is, however, an additional assumption, and one that he does not make explicit in his formulation of the second criterion. If, however, we do not make that additional assumption, we are left with the criterion of predictability as-is.

This criterion for individual sentences is, as explained previously, a special instance of Carnap and Ayer’s criterion for confirmability and verifiability, respectively¹⁶. But Popper (if we are to engage in a form of rational reconstruction) likely recognised that if the rest of the theoretical system remains fixed, then the criterion of predictability does in fact entail his criterion of falsifiability, but applied

¹⁶It is a shame that, as far as I can determine, only Sebastian Lutz (2013) has noticed this aspect of Popper’s demarcation criteria.

to individual theories, rather than theoretical systems as a whole.

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; and so on. These objections are numerous, widespread, and all predicated on assuming at least one of these three myths.

How did these objections become so prevalent? I argued that dismissing Popper's territorial problem based on the objections from ill-fit and characterising Popper's stated territorial problem and criteria as such are both predicated on mistakes in exegesis. But this pushes back the question of why these mistakes are so commonplace. These confusions are most likely due to Popper's difficulty in expressing his ideas clearly in English, such as his 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 and slogans 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 philosophy¹⁷.

Second, and more generally, philosophers and students in philosophy should

¹⁷They may be effective against proposed territorial criteria that demarcate science from non-science, or against a wholly different criterion presented by, for example, Antony Flew's article, 'Theology and Falsification' (Flew 1964), that bears the same name as Popper's falsifiability criterion, but has only superficial similarities.

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 to consider 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: the stated objections from ill-fit that are so prevalent in the philosophic literature are much ado about nothing and should be immediately retired.

References

- Ariew, Roger (1984). "The Duhem Thesis". In: *The British Journal for Philosophy of Science* 35, pp. 313–325.
- Ayer, A.J. (1936). *Language, Truth and Logic*. 1st ed. London: Victor Gollanz.
- (1946). *Language, Truth and Logic*. 2nd ed. London: Victor Gollanz.
- Bartley, W.W. (1990). *Unmeasured Knowledge, Unmeasured Wealth: On Universities and the Wealth of Nations*. La Salle: Open Court Press.
- Blanshard, Brand (1962). *Reason and Analysis*. London: Routledge.
- Boudry, M. (2013). "Loki's Wager and Laudan's Error". In: *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*. Ed. by M. Boudry and M. Pigliucci. Chicago: Chicago University Press.
- Boudry, M. and M. Pigliucci, eds. (2013). *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*. Chicago: Chicago University Press.
- Braithwaite, Richard Bevan (1953). *Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science*. Cambridge University Press.

- Carnap, R. (1956). "The Methodological Character of Theoretical Concepts". In: *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*. Ed. by Feigl and Scriven. Minneapolis: University of Minneapolis Press.
- Church, A. (1949). "Review of Ayer's *Language, Truth, and Logic*, 2nd edition". In: *The Journal of Symbolic Logic* 14.1.
- Curd, Martin, J.A. Cover, and Christopher Pincock (2013). *Philosophy of Science: The Central Issues*. Ed. by Martin Curd, J.A. Cover, and Christopher Pincock. 2nd. W.W. Norton & Company.
- Deutsch, David (2011). *The Beginning of Infinity: Explanations that Transform the World*. Viking.
- Feyerabend, Paul (1975). *Against Method: Outline of an anarchistic theory of knowledge*. London: Atlantic Highlands: HUMANITIES PRESS.
- Flew, A. (1964). "Theology and falsification". In: *New Essays in Philosophical Theology*. Ed. by A. Flew, R.M. Hare, and Basil Mitchell. New York: Macmillan.
- Gillies, Donald (1971). "A Falsifying Rule for Probability Statements". In: *British Journal of Philosophy of Science* 22, pp. 231–261.
- (1993). *Philosophy of Science in the Twentieth Century: Four Central Themes*. Oxford: Oxford University Press.
- Hacohen, Malachi Haim (2002). *Karl Popper - The Formative Years, 1902-1945: Politics and Philosophy in Interwar Vienna*. Cambridge: Cambridge University Press.
- (2006). "The Young Popper as a Scholarly Field: A Comment on Dahms, Hansen, and ter Hark". In: *Karl Popper: A Centenary Assessment*. Vol. 1. Ashgate.
- Hempel, C. (1950). "Problems and Changes in the Empiricist Criterion of Meaning". In: *Revue Internationale de Philosophie* 41.11.
- (1951). "The Concept of Cognitive Significance: A Reconsideration". In: *Proceedings of the American Academy of Arts and Sciences* 80.1.
- (1965). "Empiricist criteria of cognitive significance: Problems and changes". In: *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: The Free Press.
- Howson, Colin and Peter Urbach (2006). *Scientific Reasoning: The Bayesian Approach*. 3rd ed. La Salle: Open Court Press.

- Hübner, Kurt (1978). "Some Critical Comments on Current Popperianism on the Basis of a Theory of System Sets". In: *Progress and Rationality in Science*. Ed. by Radnitzky and Andersson. Dordrecht: Reidel.
- Keuth, Herbert (2005). *The Philosophy of Karl Popper*. Cambridge: Cambridge University Press.
- Kitcher, P. (1982). *Abusing Science: The Case Against Creationism*. Cambridge: MIT Press.
- Kneale, William C. (1974). "The Demarcation of Science". In: *The Philosophy of Karl Popper*. Ed. by Paul Arthur Schilpp. The Library of Living Philosophers. La Salle: Open Court Press, pp. 205–17.
- Lakatos, I. (1981). *The Methodology of Scientific Research Programmes*. Ed. by J. Worroll and Gregory Currie. Vol. 1. Cambridge: Cambridge University Press.
- Laudan, L. (1983). "The Demise of the Demarcation Problem". In: *Physics, Philosophy and Psychoanalysis*. Dordrecht: Reidel.
- Lutz, Sebastian (2013). "Criteria of Empirical Significance: A Success Story". In: *Preprint*.
- Mahner, M. (2013). "How to Demarcate after the (Alleged) Demise of the Demarcation Problem". In: *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*. Ed. by M. Boudry and M. Pigliucci. Chicago: Chicago University Press.
- Maxwell, Nicholas (2016). "Popper's Paradoxical Pursuit of Natural Philosophy". In: *The Cambridge Companion to Karl Popper*. Ed. by Jeremy Shearmur and Geoffrey Stokes. Cambridge University Press.
- Mayo, Deborah G. (1966). *Error and the Growth of Experimental Knowledge*. University of Chicago Press.
- Miller, David (1994). *Critical Rationalism: A Restatement and Defence*. La Salle: Open Court Press.
- (2006). *Out of Error: Further Essays on Critical Rationalism*. Burlington, VA: Ashgate.
- (n.a.) (n.d.). *Falsifiability*. In *Wikipedia*. URL: <https://en.wikipedia.org/wiki/Falsifiability>.
- Newton-Smith, W.H. (1980). "Comments on Miller 1980". In: *Applications of Inductive Logic*. Ed. by L. Jonathan Cohen and Mary Hesse. Oxford: Clarendon.
- (1981). *The Rationality of Science*. Routledge.

- Neyman, J. and E.S. Pearson (1967). "The testing of statistical hypotheses in relation to probabilities *a priori*". In: *Joint Statistical Papers of J. Neyman & E.S. Pearson*. Cambridge University Press, pp. 186–202.
- Nickles, T. (2013). "The Problem of Demarcation: History and Future". In: *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*. Ed. by M. Boudry and M. Pigliucci. Chicago: Chicago University Press.
- Papineau, David (1995). "Review of *Knowledge and the Mind-Body Problem* and *The Myth of the Framework*". In: *Times Literary Supplement*.
- (2000). "Review of *Karl Popper: The Formative Years, 1902-1945*". In: *New York Times Book Review*.
- Pigliucci, M. (2013). "The Demarcation Problem. A (Belated) Response to Laudan". In: *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem*. Ed. by M. Boudry and M. Pigliucci. Chicago: Chicago University Press.
- Popper, K.R. (1934/5). *Logik der Forschung: Zur Erkenntnistheorie der modernen Naturwissenschaft*. Vol. 9. Schriften zur wissenschaftlichen Weltanschauung. Vienna: Verlag von Julius Springer.
- (1959). *The Logic of Scientific Discovery*. Trans. by K. Popper, Julius Freed, and Ian Freed. References are to (Popper 2000). London: Hutchinson & Co.
- (1962). *Conjectures and Refutations: The Growth of Scientific Knowledge*. New York: Basic Books.
- (1963). "The demarcation between science and metaphysics". In: *The Philosophy of Rudolf Carnap*. Ed. by P.A. Schilpp. Vol. XI. Library of Living Philosophers. La Salle: Open Court Press.
- (1974). "Replies to My Critics". In: *The Philosophy of Karl Popper*. Ed. by Paul Arthur Schilpp. Vol. xiv. The Library of Living Philosophers. La Salle: Open Court Press.
- (1983). *Postscript to the Logic of Scientific Discovery: Realism and the Aim of Science*. Ed. by W.W. Bartley. Vol. iii. Originally circulated as private galley proofs in 1956/1957. London: Routledge.
- (1997). *The Myth of the Framework*. Ed. by M.A. Notturmo. London: Routledge.
- (2000). *The Logic of Scientific Discovery*. London: Routledge.
- (2002). *Unended Quest - An Intellectual Autobiography*. Open Court Press.

- Popper, K.R. (2009). *The Two Fundamental Problems of the Theory of Knowledge*. Ed. by Troels Eggers Hansen. London: Routledge.
- (2013). *The Open Society and Its Enemies: New One-Volume Edition*. Originally published in two volumes in 1945 by Princeton University Press. Princeton, NJ: Princeton University Press.
- Preston, J. (1994). “Methodology, Epistemology and Conventions: Popper’s Bad Start”. In: *PSA 1994*.1.
- Putnam, H. (1974). “The Corroboration of Theories”. In: *The Philosophy of Karl Popper*. Ed. by Paul Arthur Schilpp. Vol. xiv. The Library of Living Philosophers. La Salle: Open Court Press.
- Shea, Brandon (2018). *Karl Popper: Philosophy of Science*. URL: <http://www.iep.utm.edu/pop-sci/#H2>.
- Thornton, S. (2016). *Karl Popper*. Ed. by Edward N. Zalta. URL: <https://plato.stanford.edu/archives/win2016/entries/popper/>.
- Weibl, E. and P. Herdina, eds. (1997). *German Dictionary of Philosophical Terms*. Vol. 2: English-German. London: Routledge.
- Worrall, John (1995). “Revolutions in Permanence: Popper on Theory-Change in Science”. In: *Karl Popper – Philosophy and Problems*. Ed. by Anthony O’Hear. Cambridge University Press.