# The Demarcation Problem: Karl Popper's solution in the contemporary retrospective

**Dmytro Sepetyi**

**Abstract**. The article analyses post-Popperian debates about the problem of demarcation. The case is made that Karl Popper's proposition to demarcate empirical science from the rest («metaphysics») on the basis of falsifiability as the criterion of empirical character of theories remains a tenable way of seeing the most valuable aspect of the character of the scientific enterprise. It is explained that for the falsifiability criterion to be adequate for the purpose, we should consider it, as Popper did, as two-aspect — involving the logical content of theories and the critical attitude of researchers. Although falsifiability, in this sense, is not sufficient to neatly demarcate science from non-science, it can be considered as the most important necessary condition, which can be supplemented by some further requirements, for an empirical hypothesis to count as part of the body of science. In particular, it is argued that Lee McIntyre's proposition to see empirical science as a matter of what he calls «scientific attitude» perfectly fits with this falsifiability-based perspective. William Bartley's and Harry Laudan's deflationary approaches to the demarcation problem are criticised as failing to recognise the practical significance of distinguishing what is and what is not empirical science, and the necessity of some theoretical grounding (however imperfect) for practicing this distinction. The suggestions of Joseph Agassi, Imre Lakatos, and Thomas Kuhn purported at the improval of Popper's demarcational proposition are discussed, and the author's own proposition is advanced and explained. It is also pointed out that Popper's remark that the distinctive feature of scientific theories is openness to criticism and modification in its light serves as the basis for a wider conception of science, without the qualification «empirical».

**Keywords**: demarcation, falsification, science, empirical, metaphysics, pseudoscience.

*Актуальні проблеми духовності:*
*зб. наук. праць / Ред.: Я.В. Шрамко.*
*Кривий Ріг : КДПУ, 2024. Вип. 25. С. 48–73.*

## Introduction

The problem of how to distinguish science from nonscientific, especially pseudoscientific claims, known as *the problem of demarcation*, was among the most hotly discussed in the philosophy of science and epistemology of $20^{th}$ century, and still remains of considerable interest. Some evidences of this are two recently published books, the collection of articles under the editorship of Massimo Pigliucci and Maarten Boudry *The philosophy of pseudoscience* [22] and Lee McIntyre's book *The Scientific Attitude: Defending Science from Denial, Fraud, and Pseudoscience* [19].

The most intense debate occurred in the period from the 1920s to the 1970s; it was mostly turning around the positivistic doctrine about verifiability (the possibility of empirical confirmation) as the criterion of meaningfulness and Karl Popper's proposition (elaborated in polemics with positivism) to consider falsifiability (the possibility of empirical refutation) as the criterion of demarcation between empirical science and «metaphysics». Popper's and other criticisms had shown the radical inadequacy of the positivistic approach, on which verifiability was considered both as the criterion of meaningfulness and the criterion of scientific empirical character. In its turn, Popper's proposition about falsifiability as the criterion of demarcation was amply criticized, and these criticisms had shown that the proposition is, at least, imperfect, in that falsifiability cannot serve as *the necessary and sufficient condition* of a statement's belonging to (empirical) science. However, Popper's proposition was not superseded by any more successful one, which would command wider acceptance among scientists and philosophers of science, or even raise interest and intense debates comparable with those about Popper's proposition. Rather, after 1970-ies, the demarcation debate subsided and became dominated by the deflationist approach — the view that the demarcation problem is of little or no importance. Among others, this view was advanced and defended by one of Popper's most talented pupils, William Bartley [7], and the most influential case for it was made by Larry Laudan [17]. Nevertheless, the importance of science for the contemporary world and the trust most people have in it as a guide for decisions in technology, medicine, and other fields ensure that the evaluation of the claims for scientific status will retain its practical significance. This practical need requires for theoretical tools. So, we need an account, however imperfect, of what makes science that special kind of activity that it is. What is it that distinguishes science from other activities that result, like science, in systems of ideas, or statements, about the world

or some parts of it? Especially, because we tend to invest science with special trust as a guide in important life affairs (such as designing buildings and bridges, producing and prescribing medicines etc.), rely upon science, we should naturally be interested in the question: what makes science so trustworthy, or reliable? Apart from mere curiosity, this can help us to detect and disqualify those activities and systems of ideas – let us designate them as pseudoscience – which try to get our trust by assuring that they are scientific but lack that feature(s) that make science trustworthy (reliable).

The question about the distinctive feature(s) of science, that is, the criteria(on) of demarcation, is also important for the legislative and judicial regulation of important educational matters: the most remarkable example is the decision adopted in 1981 by the court of the US state Arkansas with respect to teaching creationism in school, as a purportedly scientific discipline, — the verdict, unfavourable for creationism teaching, was informed by the account that was prepared by prominent scientists and involved Popper's criterion of demarcation, falsifiability.

In my own experience, when philosophers or scientists try to explain what is distinctive of (empirical) science, they still most often appeal to Popper's criterion. I think that this shows that despite its drawbacks (much discussed and well-known to philosophers of science), Popper's criterion of demarcation picks out the most essential of what scientists usually perceive as distinctive for their professional activity. This suggests the expediency of the reevaluation of the main criticisms of Popper's criterion, as well as the possibilities to improve it and new relevant propositions. In this article, I will first outline what I think to be the best construal of Popper's claims about demarcation, and then discuss a range of criticisms, alternative and developing propositions, which include William Bartley's and Harry Laudan's deflationary approaches to the demarcation problem, and a number of propositions that purport to improve upon Popper's demarcational proposition (Lee McIntyre, Joseph Agassi, Imre Lakatos, Thomas Kuhn).

## 1. Popper's «demarcation criterion» as a necessary condition of belonging to *empirical* science

As a too well familiar story goes, Popper proposed falsifiability as the criterion of demarcation between science and «metaphysics». Every statement, or system of statements, that can possibly be falsified (refuted by the collision with the results of observations) is scientific; every

statement, or system of statements, that cannot possibly be falsified is unscientific, or «metaphysical». However, did Popper really proposed, and meant, this? The analysis of his relevant statements, as well as consideration of some too obvious criticisms, suggests that it is not quite so, in several respects.

Although Popper's proposition is usually described as concerned with the demarcation between *science* and *metaphysics*, this description is an over-simplification that is usually convenient but may also be misleading. Popper himself usually wrote, in this context, of «empirical science» rather than merely «science», and although he occasionally dropped the adjective «empirical», this was merely a matter of brevity and convenience. As for the other side of the distinction, although Popper sometimes described it by one word «metaphysics» (usually, when discussing the historical background of the issue — especially, the positivist aspiration to outlaw metaphysics as meaningless), more often he designated it as «mathematics and logic as well as "metaphysical" systems» [28, p. 11], metaphysics and conventionalist and tautological systems [28, p. 315], «all other statements — whether they are of a religious or of a metaphysical character, or simply pseudoscientific» [23, p. 39], etc. Thus, Popper intended to demarcate not science from metaphysics but *empirical* science from all other statements, theories, etc. For Popper, falsifiability is the distinctive quality of *empirical* scientific statements or systems.

However, Popper often described falsifiability somewhat differently — not as the criterion of demarcation between empirical science and «metaphysics» but *as the criterion of the empirical character* of theoretical systems.[1] This suggests that Popper's proposition has more to do with *demarcating empirical from nonempirical*l than with demarcating scientific from nonscientific. However, this still does not clarify much. We can apply the word «empirical» to all statements that appeal, in one way or another, to experience, observations, experiments. However, Popper's demarcational proposition was exactly intended to leave some such appeals beyond the pale of the empirical, or «empirical science». There are, so to say, *genuinely empirical* statements (theories) and sham (pseudo)

[1]So, when Popper proposed falsifiability as a criterion of demarcation for the first time, in *The Logic of Scientific Discovery*, he described the demarcation problem as «the problem of finding the criterion of the empirical character of science» [28, p. 34], and falsifiability as «a criterion for the empirical character of a system of statements» [28, p. 66]. See also *Open Society and Its Enemies*, where Popper mentions his proposition «that we solve the problem of demarcation by using falsifiability or testability, or degrees of testability, as criterion of the empirical character of a scientific system» [29, p. 283].

empirical statements. The pattern of the first is to be found in the paradigmatic empirical scientific theories, such as Einstein's theory. This is what makes for the glory of science! On the other side, there are theories that vigorously appeal to experience and draw their (undeserved) repute from this appeal, from the (false) impression that they are of the same (empirical scientific) species as Einstein's theory, although in fact their way of treating experience is entirely different. Examples of such «sciences» that motivated Popper were theories of Marx, Freud, and Adler.

Popper's explanations suggest that he was concerned with *the possibility of valid empirical testing* of a theory, its evaluation by the results of (experimental) observations — what kind of testing should qualify as *serious* testing (see [26, p. 174]) that makes a theory trustworthy, or reliable in a practically significant sense,[2] or supplies it with that kind of credentials that theories like Einstein's have. Most explicitly, Popper explained this in the book *Conjectures and Refutations*, where he identified his problem of distinguishing between science and pseudoscience with the problem of a genuinely empirical method: «I often formulated my problem as one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method — that is to say, a method which, although it appeals to observation and experiment, nevertheless does not come up to scientific standards» [23, p. 33-34]; «my "problem of demarcation" was from the beginning ... an urgent practical problem: under what conditions is a *critical appeal to experience* possible — one that could bear some fruit?» [26, p. 174]. His solution to this problem was that the serious empirical testing of a theory (and, accordingly, a genuinely empirical method) consists in attempts to *refute (falsify)* the theory [23, p. 36]. And obviously, such attempts are possible only if the theory at issue can clash with some conceivable observable events, that is, if it is falsifiable.

[2]This may seem to conflict with the point that is pretty often emphasised by Popper's followers — that on his view, scientific theories are *not reliable* (see, for example, Miller 1994; Miller 1999). This, however, is a misleadingly one-sided presentation of Popper's view that was in fact two-sided. When discussing the issue of the reliability of scientific theories, Popper merely pointed out that there is a sense of «reliable» in which scientific theories are «reliable», and there is another sense of «reliable» in which scientific theories are not «reliable»: «Of course, in choosing the best-tested theory as a basis for action, we "rely" on it, in some sense of the word. It may therefore even be described as the most "reliable" theory available, in some sense of this term. Yet this does not say that it is "reliable". It is not "reliable" at least in the sense that we shall always do well, even in practical action, to foresee the possibility that something may go wrong with our expectations» [25, p. 22]. As Joseph Agassi aptly noted, on Popper's view, «reason is unreliable in the sense that it can never be assuredly free of error, but there is nothing better to rely on. This is what Churchill said of democracy ...» [1, p. 91].

Popper's demarcation follows straightforwardly:

(E) empirical scientific statements (theories) are statements (theories) that allow serious empirical testing;

(T) serious empirical testing is attempts at falsification;

hence,

(D) empirical scientific statements (theories) are falsifiable statements (theories). In other words,

(D*) nonfalsifiable statements (theories) are not empirical scientific statements (theories).

Eventually, Popper's demarcational proposition boils down to the rejection of verificationism (the view that empirical testing of a theory consists in its *verifications, confirmations*) and assertion of falsificationism — the thesis that only attempts at falsification (or, at least, tests that can result in falsification) deserve to count as serious (scientific) empirical trials: «Confirming evidence should not count except when it is the result of a genuine test of the theory; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory» [23, p. 36].

However, there is a bit of looseness it the argument above. We can note that if we agree with (T), it does follow that for a statement (theory) to count as an empirical scientific statement (theory), it should be falsifiable; however, it does not follow that if a statement (theory) is falsifiable, it should count as an empirical scientific statement (theory). There can be reasons why we would be unwilling to count some statements (theories) as *scientific* empirical statements (theories), even if they are falsifiable. We would count them as empirical but not as scientific. One kind of such statements are theories (hypotheses) about particular events that are of no general interest; for example, a detective advances testable (falsifiable) hypotheses about the case she investigates. Another kind are statements that are known to be false even when first advanced, or were not even intended as solutions to any explanatory or theoretical problems. If I state that all elephants weigh less than 1 kg, this statement is falsifiable, and so empirical, but hardly anyone would recognise it as scientific.

We should conclude that falsifiability, even if it is accepted as the criterion of the *empirical character*, is not *the sufficient condition* of the empirical *scientific status*. However, there is no reason to think that Popper was ever looking for such a sufficient condition. For his purposes, it is enough if falsifiability is 1) *a necessary* condition for a theory to deserve the name of an empirical scientific theory and 2) a feature that sets apart empirical scientific theories from typical pseudoscientific theories, as well

as from metaphysical theories and mathematics.[3]

## 2. Two aspects of Popper's demarcational proposition and Lee McIntyre's account of the scientific attitude

An important feature of Popper's demarcational proposition is that it essentially involves not only the logical content of the position (theory or statement) at issue but also the attitude of its adherents with respect to the proposition. So it has two aspects — logical and socio-psychological. As far as I see, many criticisms (including such influential ones as those by A. Grünbaum [11] and L. Laudan [17]) fail to take adequately into account the second aspect. They are fixed on the logical side, although complaining sometimes of the «ambiguity» of Popper's falsifiability criterion.

Indeed, Popper sometimes described falsifiability as apparently purely logical matter of consequences that follow from a theory; however, he also often emphasized the importance of a (critical or fallibilistic) attitude of the adherents of a theory, their readiness to recognize refutation rather than defend their theory by any means (that can always be found, if one is intent upon it). He wrote that «empirical science should be characterised by its methods: by our manner of dealing with scientific systems» [28, p. 29]. This, however, is not a vicious ambiguity but adequate reflection of the conditions that make empirical testing possible in practice: first, a theory should satisfy logical conditions of falsifiability — it should entail some predictions that can conceivably conflict with observations; second, the adherents of the theory should be ready to admit that it is refuted, and to abandon it, when intersubjectively ascertainable events that contradict it really happen. Otherwise, if we are intent upon saving a theory come what may, it becomes *practically* unfalsifiable, even if it is *formally* falsifiable. Popper's demarcational proposition is concerned with falsifiability in practice rather than in pure logic. As Frank Cioffi aptly puts it, «Karl Popper's criterion of demarcation is not merely formal falsifiability but "severe attempts at refutation"» [10, p. 42].[4]

[3]Cf.: «Our criterion of falsifiability distinguishes with sufficient precision the theoretical systems of the empirical sciences from those of metaphysics (and from conventionalist and tautological systems), without asserting the meaninglessness of metaphysics (which from a historical point of view can be seen to be the source from which the theories of the empirical sciences spring)» [28, p. 315-316].

[4]See also Cioffi [10, p. 14-16] for a selection of Popper's statements that make it clear that he «holds that a theory which is formally falsifiable may, nevertheless, be pseudo-scientific» [10, p. 14]. I will reproduce here two of them:

So, Popper's falsifiability criterion is essentially two-aspect, and this two-aspectedness is perfectly sound. In other words, the empirical scientific character of a position is a matter both

1) of its being (or not being) formulated so that on a natural competent[5] reading it (in conjunction with empirically ascertainable initial conditions) entails some empirically ascertainable predictions

and

2) of the way — critical or apologetic — it is treated.

The point is that in the case of many theories that are of special interest as to their scientific or pseudoscientific character (such as the theories of Marx, Freud, and Adler), the same theory can be qualified as an empirical scientific *false* theory insofar as it is treated critically (it seems pretty clear that on the «normal» reading, quite a few predictions that Marx and Freud themselves «deduced» from their theories, turned out to be false), and as preudoscientific insofar as it, while making claims for being scientific, is treated apologetically by its adherents (Marxists and Freudists tend to disregard falsifications, explain them away by *ad hoc* devices, deflect criticisms by «unmasking» unsound motives of critics, such as class interest or subconscious resistance, ignoring falsifying evidence, treating refuted aspects of a theory as dispensable without compromising its identity, and even straightforward forgery).

In this light, it may be illuminating to consider the recent proposition by Lee McIntyre, to use as the touchstone of empirical science what he calls «the scientific attitude», which is nothing but *the readiness to learn from experience*. More precisely, «the scientific attitude can be summed up in a commitment to two principles:

(1) We care about empirical evidence.

(2) We are willing to change our theories in light of new evidence» [19, p. 47-48].

---

> It is impossible to decide by analysing its logical form, whether a system of statements ... is a refutable system ... Only with reference to the methods applied to a theoretical system is it at all possible to ask whether we are dealing with ... an empirical theory [28, p. 61].
>
> What characterizes the scientific approach is a highly critical attitude towards our theories rather than a formal criterion of refutability; only in the light of such a critical attitude and the corresponding critical methodology approach do «refutable» theories retain their refutability [27, p. 94].

[5]The required competence includes the linguistic competence and the competence in the theoretical field to which the position belongs.

McIntyre admits that this criterion (as well as Popper's criterion) cannot be the necessary *and sufficient* condition of empirical science — obviously, the scientific attitude can be — and often is — usefully taken in nonscientific activities (such as work of a detective, for example, or even the simplest cases of our everyday learning from experience). However, he argues that even considered as a merely *necessary condition*, the criterion is useful in adjudicating between the claims for scientific status, recognizing pseudoscience, or even seeing the difference between good science and bad science. (The difference between bad science and pseudoscience need not be clear-cut; it is rather a matter of an institutional background of a theory and its authors.)

McIntyre recognizes that his proposition is very much along Popper's lines (and inspired by Popper), but he seems to present it as a revision of Popper's criterion of demarcation rather than as identical with it. I think that there is no genuine difference, except that McIntyre's criterion is not the whole of Popper's but its socio-psychological aspect.

To be «willing to change our theories in light of new evidence» is the attitude that is required for falsification of a theory to be possible. And it is clear that this attitude can be applied with a chance to succeed only to theories that satisfy the logical requirement of falsifiability — in order to be able to learn from experience, by changing our theories in the light of new evidence, it is necessary that our theories entail consequences that can conceivably conflict with experience.

For illustration's sake, let us consider a case discussed by McIntyre in his book *The Scientific Attitude* — a bright example of the scientific approach applied by the Hungarian physician Ignaz Semmelweis, to find out the cause of childbed (puerperal) fever and develop efficient means to prevent this dangerous decease [19, p. 52-55].

The problem faced by Semmelweis can be described as follows. Childbed (puerperal) fever afflicted many women who delivered their babies in maternity clinics, and it often resulted in their death. In clinics, it happened much more often then with women who delivered their babies at home. Besides, in the clinic where Semmelweis worked, there was a great difference in the incidence between two wards — 29% versus 3%. The problem was to discover the cause that could explain these facts. After several wrong guesses (that failed tests), Semmelweis has struck at the correct one. He noted that one of the differences between the two wards was that in the one with the higher incidence of the fever, the deliveries were handled by medical students, whereas in the second ward they were performed by midwives. He conjectured that the students, who

were often coming to the delivery ward directly from performing autopsies (and no measures of disinfection, even such as washing hands, were known to be required), somehow brought the decease agents with them and transferred them to the pregnant women. (The germ theory of disease was not yet known and accepted.) Semmelweis introduced a simple practice of disinfection — ordered the medical students to wash their hands in chlorinated water before performing deliveries. The result was excellent — the mortality rate among the women and babies in the clinic dropped more than 7 times — from 18% to 2,5%.

However, the medical establishment met this important discovery with striking resistance and obscurantism. This was because Semmelweis's hypothesis conflicted with the medical «paradigm» of that time, the bulk of the theories accepted by the medical profession (the authority of these theories was due entirely to the ages-long tradition and was never subjected to empirical testing), and because it was taken as discrediting medical professionals (who, as carriers of decease agents, turned out responsible for many deaths). For decades, Semmelweis's colleagues refused to recognise his discovery, and his fate was tragic: he was fired from the clinic, and then led unsuccessful bitter polemics, suffered breakdown, was incarcerated in an insane asylum, where he was brutally maltreated and died after two weeks, in 1865. The recognition came several decades later, when Louis Pasteur and Robert Koch developed and successfully tested the germ theory of decease, which provided the lacking theoretical foundation for Semmelweis's discovery.

Obviously, in this story it was Semmelweis, and not the defenders of the dominant medical «paradigm», who represented the scientific approach. This can be explained, as McIntyre proposes, in terms of the willingness to learn from experience; however, we can also note that this perfectly fits Popper's falsifiability proposition. Semmelweis could learn from experience only insofar as his hypothesis and his attitude satisfied the logical and psychological aspects of Popper's notion of falsifiability. The hypothesis entailed some predictions that could conceivably turn out false, and if this happened, the hypothesis would be recognised false. Namely, the hypothesis entailed that the use of the means of disinfection should considerably decrease the incidence of the fever. Presumably, if this did not happen, Semmelweis would abandon the hypothesis, just as he abandoned several preceding ones. (If he would not, then his attitude would not be scientific — there would be no willingness to learn from experience.) So I think that this example shows that McIntyre's proposition to consider the willingness to learn from experience as the distinctive feature (the necessary

condition) of empirical science is tantamount to Popper's proposition to consider falsifiability as such a feature (condition), taking into account the two-aspectedness of this proposition. Besides, this example shows that it is Popper's falsificationist account rather than Kuhn's conception of normal science and scientific revolutions, that fits our notion of how a genuine scientist should behave.

## 3. Bartley's and Laudan's deflationist approaches

William Bartley argued that Popper's problem of demarcation, between empirical science and the rest, (and, hence, his demarcational proposition) is «relatively unimportant, at least for purposes of evaluation and criticism», «has little evaluatory importance» [7, p. 43, 47]. His argument was roughly that the demarcation between science and non-science was clearly intended for purposes of evaluation of theories as good or bad, but Popper himself recognised that «metaphysics» is not necessarily bad;[6] therefore (Bartley suggested) we need not bother about the demarcation between the «scientific» and the «nonscientific» and better look for a more general demarcation, between good and bad theories. Bartley proceeds by identifying *goodness* with *criticizability* (which he considers as a generalisation of Popper's falsifiability) and *badness* with noncriticisability (as a generalisation of Popper's nonfalsifiability). From this, he moves to the discussion of the ways to criticise a theory (empirical or not) and strategies to avoid criticism.

I think that Bartley's argument for the replacement of (and his ambition to supersede) Popper's demarcation problem (between empirical science and the rest) with a more general demarcation problem (between good and bad theories, or criticisable and uncriticisable theories) fails. The fact that the distinction between (empirical) science and the rest (if it can be achieved) falls short of demarcating good from bad, does not entail that this distinction cannot be useful for a much more modest, but still worth pursuing, purpose — that of distinguishing theories that are *good is some important respect* from theories that *are not good in that*

[6] Cf.: K. Popper: «It cannot be denied that along with metaphysical ideas which have obstructed the advance of science there have been others—such as speculative atomism—which have aided it. And looking at the matter from the psychological angle, I am inclined to think that scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy; a faith which is completely unwarranted from the point of view of science, and which, to that extent, is "metaphysical"» [28, p. 16].

*respect*. I suggest that Popper's demarcational proposition aims at this modest purpose. Admittedly, we would not need it *if we had the general demarcation between good and bad* theories (and other positions). However, that ambitious new demarcationist project is hardly feasible, and Bartley's own discussion gives reasons to be sceptical about it. I think that there are at least two grave flaws in his elaboration of the project. First, the proposition to demarcate good theories from bad on the basis of a single property, criticisability, is confused and circular, because criticism involves the evaluation of different good/bad properties. Second, although there are various ways to criticise a theory and avoid criticism that deserve to be discussed, there can be no demarcation between theories that are criticisable and those that are not: given the variety of ways a theory can be criticised, all theories (and other positions) are criticisable. Let us consider these points in a bit more details.

1) When we try to evaluate theories as good or bad, what is meant by this evaluation? Presumably, there are many respects in which a theory (or some other position) can be judged bad. We may judge that it is good if a theory is clearly formulated, interesting, rich in content, empirically falsifiable... (The list can be continued.) And, of course, it is good if a theory at issue is true. Theories that lack those qualities can be judged as bad or not so good, especially if there are alternative theories available that do have these qualities. It is important to note that this goodness/badness is a relative matter. Theories can be formulated more or less clearly — the more the better. They may be more or less rich in content. If a theory about some happenings in the world is formulated in such vague ambiguous terms that it is difficult to see what observable events could possibly falsify it (it is unfalsifiable or «metaphysical» in this sense), it would be well to replace it with a theory with clear empirical consequences (that is, a falsifiable, empirical theory). However, it is often impossible to do so with respect to many important questions; presumably, we do not want to qualify mathematics and all philosophical theories as bad because they are not empirically falsifiable. In other words, there are features that are desirable for a theory *if possible*, and the evaluation of a theory as good or bad is a matter of evaluation of these features and comparison with the available alternative theories, as possessing or failing to possess these features to higher or lower degrees. We cannot usefully replace all this with the apparently unidimensional binary alternative of «criticisable»/«uncriticisable». Moreover, this apparent unidimensionality (and binarity) is delusive, because criticisms of a theory involve those features that we consider as desirable and possible for theories in this

area; to criticise a theory is to argue that it lacks these features or is inferior to some alternative theory that has these features to a higher degree. Accordingly, there is a diversity of the ways to criticise a theory that *reflects (maps)* the diversity of those features that we consider as desirable/undesirable and possible for theories (positions) in various areas.

2) The very variety of the respects in which a theory can be found bad and the corresponding ways of criticism ensures that any theory (position) can be criticised, that is, is criticisable. Hence, the demarcation between criticisable and uncriticisable theories is impossible. It is easy to prove this by assuming the opposite and assuming Bartley's core thesis, that uncriticisability is bad. Assume that 1) there is an uncriticisable theory T and 2) uncriticisability is bad. If so, then to point out that T is uncriticisable is to criticise it. Hence, T is criticisable. Hence, our assumption was false, *q.e.d.*

Later, Bartley himself recognised the nonexistence of the criterion of demarcation between good and bad positions:

> What, then, is the criterion of demarcation between a good idea and a bad one? There is none. There are, of course, certain qualities that are highly desirable in theories, and whose absence signals danger. These include testability and high empirical content. But these are not criteria: their presence is not required, and a theory lacking in them may turn out to be excellent. There are some objectionable characteristics in theories, and these include inconsistency and incoherency. But their contraries are not criteria of goodness: consistency and coherency are desired, but they do not, in and of themselves, make a theory a good one [8, p. 206].

This, I think, cancels his proposition to replace Popper's specific demarcation between what is and what is not empirical science with the demarcation between good and bad, or between criticisable and uncriticisable. And it undermines Bartley's suggestion that criticisability can work as a generalisation of falsifiability. There is a distinction between those claims that are empirically falsifiable and those that are not: there are claims that are clearly empirically falsifiable, and there are claims that are clearly empirically unfalsifiable (as mathematical and most philosophical claims). Admittedly, there is also «the grey zone» of the claims about which it is not clear whether they are falsifiable (it is a matter of interpretation). However, the problem with criticisability is not that of «the grey zone» — it is that there are no uncriticisable positions at all.

Now it remains to say a bit more about the real significance of Popper's demarcation problem and proposition. Popper's demarcational proposition was clearly not intended to distinguish any good theory from any bad theory. It was intended only to distinguish those theories that have some

*specific credentials* — those that make paradigmatically empirical scientific theories, such as Newton's or Einstein's theory, trustworthy — from those that do not have these specific credentials, and do not belong, so to say, to the same species as paradigmatically empirical scientific theories. This is especially important with respect to those theories — properly called pseudoscientific — that pretend to belong to the same species, and so try (often very successfully) to parasitize upon the high repute earned for science by such theories as those of Newton and Einstein. From what Popper wrote on this topic, we can see that there were basically two motivations for his demarcational proposition:

1) to distinguish genuine empirical testing that underlies the progress of empirical sciences from the claimed but spurious empirical support;

and

2) to capture that great difference he felt to be there between such theories as Einstein's on the one hand and those of Marx, Freud and Adler, and the ways these theories achieve and sustain their repute (especially, the attitude of their followers).

Nothing in Bartley's argument shows that Popper failed in these respects.

Larry Laudan was another philosopher who argued, more influentially than anyone else, for the deflationist approach to the demarcation problem.

Laudan's argument crucially depends on the proposition that the demarcation criterion should «specify a set of individually necessary and jointly sufficient conditions for deciding whether an activity or set of statements is scientific or unscientific» [17, p. 118]. Laudan first describes this as an ideal requirement but then asks «Would something less ambitious do the job?» and argues that it would not. The conclusion should be that either we can have the ideal demarcation criterion, or we cannot have a demarcation criterion worth having at all. However, this conclusion and the argument Laudan advances in its support involve misunderstanding of «the job» Popper's criterion of demarcation was intended for.

Laudan quotes Popper who sometimes described the motivation for his criterion of demarcation as «wish to distinguish science from pseudoscience» [23, p. 33; 17, p. 119], and interprets this as the wish to have the universal criterion that always allows telling whether a claim (theory etc.) is scientific or not. But why interpret it so? The context in which Popper wrote this allows for another interpretation that would make it desirable to have at least *a demanding necessary condition — one that, while letting in empirical scientific theories, expels as much pseudoscience as possible*, if we fail to achieve the ideal of «a set of individually necessary and jointly

sufficient conditions». Really, as I pointed out above (at the end of the discussion of Bartley's approach), there were basically two motives for Popper's demarcation criterion, and two main (interconnected) problems it was intended to solve. For both, having a demanding necessary condition would be good enough.

Popper's wish to distinguish science from pseudoscience was not a matter of neat abstract theoretical distinctions; it was a matter of having an instrument that «does the job» in particular cases *when the need arises in fact*. And such cases are nearly always those when *some theoretical system makes the claim that it is scientific*. If so, having the criterion of demarcation it the sense of demanding necessary (sufficient or not) conditions can well «do the job» required — to establish that the system is pseudoscientific (if it is). In particular, this applies to the cases that Popper described as those that have motivated his demarcational proposition — cases of Marxism, Freudism, and Adlerism, as markedly unlike such paradigmatically scientific theories as those of Newton and Einstein.

The much discussed case of legislative/judicial application of Popper's criterion of demarcation, in the case concerned with teaching «creation science» in the US state of Arkansas (1981), also well fits the bill: all that the plaintiff side had to make a case for was that the «creation science» does not satisfy *a necessary condition* of being scientific, falsifiability. Sufficient conditions did not, and had not to, come in. (Admittedly, the claim that Darwinist evolutionary theory is scientific was not established in the debate, but that claim was not at issue. If, hypothetically, the creationists would want to challenge the scientific status of the Darwinist evolutionary theory, it would be up to them to present a convincing argument that the evolutionary theory does not satisfy *a necessary condition* for qualifying as scientific. Again, sufficient conditions would not come in.)

Another consideration Laudan advances is «that the labelling of a certain activity as "scientific" or "unscientific" has social and political ramifications which go well beyond the taxonomic task of sorting beliefs into two piles. ... it has consequences which are decidedly non-epistemic», and so «[p]recisely because a demarcation criterion will serve as a rationale for taking a number of *practical* actions which may well have far-reaching moral, social and economic consequences, it would be wise to insist that the arguments in favor of any demarcation criterion we intend to take seriously should be especially compelling» [17, p. 120]. However, I think that this is a bad argument against looking for a criterion of demarcation and having and using *some such criterion*, *even if* the arguments for it are not «especially compelling». Considering this question from the viewpoint of moral, social

and economic consequences, we should be aware that having and using *no means to distinguish science from pseudoscience* can have much more detrimental consequences than having and using imperfect means for this purpose. Especially, when the claim is made by the proponents of a theory that it is scientific (and science at its best) and the repute of the theory is built on this (as in typical cases of pseudoscience, including those that motivated Popper), we need *some means, however imperfect*, to evaluate this claim. And hence, it is worthwhile to try to develop and improve such means to our best. Popper's criterion of demarcation should be seen and evaluated in this perspective.

The conclusion Laudan draws from his discussion is that «the problem of demarcation ... is spurious» [17, p. 124] and so «we ought to drop terms like "pseudoscience" and "unscientific" from our vocabulary; they are just hollow phrases which do only emotive work for us» [17, p. 125]. I find this conclusion radically inadequate: to drop these terms, we need first drop the terms «science» and «scientific». Does Laudan really suggest that these terms also should be dropped from our vocabulary as «just hollow phrases which do only emotive work for us»? If he does, then I guess that his proposition has no chance of being followed — in the contemporary world, science is too important for this. Whether we like it or not, the claim that something is «science», or «scientific», will be used to suggest that it has a high-quality mark, special credentials to be trusted. Now if there are statements, theories etc. deservedly having such credentials and so appropriately called «science», or described as «scientific», then there are some other statements, theories, etc. that are not scientific, that is, are unscientific. They may be true (many of them are), but they do not have those special credentials we usually associate with the word «science». And those of them that pretend to be science can be appropriately described as «pseudoscience».

Laudan ends his article by admitting that, despite all he said, «[i]nsofar as our concern is to protect ourselves and our fellows from the cardinal sin of believing what we wish were so rather than what there is substantial evidence for ... then our focus should be squarely on the empirical and conceptual credentials for claims about the world» [17, p. 125]. The pity is that he fails to see that Popper's «criterion of demarcation» or, more precisely, «the criterion of the empirical character» was proposed exactly as an account of «the empirical credentials for claims about the world», of what deserves to count as «substantial evidence». His proposition was about the primary condition of such substantial empirical evidence to be possible: it is possible only if the claim at issue risks empirical refutation

(≡is falsifiable). If it is, then we can test it with the evidence, which can either refute the claim or corroborate it. If it does not risk, then there cannot be any empirical test, and cannot be empirical evidence for it. Popper's demarcation efficiently boils down to the principle that «[c]onfirming evidence should not count except when it is the result of a genuine test of the theory; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory» [23, p. 36].

## 4. Some proposed developments

In several last decades, there were a number of propositions aimed at the development of a better demarcational criterion, capable of sorting what we would probably like to qualify as empirical science from what we would not in a more neat way than falsifiability. Some of them propose a complex criterion that involve many distinct features (see, for example, Mario Bunge's propositions to distinguish pseudoscience by 12 conditions [5] and science by «at least ten different features» [6, p. 245]); some others are highly technical (see, for example, [18]). I think that these approaches are not very promising, because an important requirement for the demarcation criterion is that it should in an intuitively plausible way identify the common feature (or reasonably few features) of scientific empirical theories that underlies the progress of empirical sciences toward ever more explanatorily and predictively successful theories. Because of this, the demarcation criterion, or its core, should be pretty simple. Popper's proposition, whatever may be its shortcomings, satisfies the requirement of simplicity, intuitive plausibility and illuminative power (bringing light on the progress of science), and it seems that no other proposition on the philosophical market does this better.

A possible way to move forward is to think of supplementing (rather than replacing) Popper's demarcational proposition. We can consider falsifiability as 1) the criterion of the empirical character of a theoretical system and 2) a necessary but not sufficient condition for qualifying as empirical science. Then, we can think of supplementing it with other requirements that would qualify a theory, or a statement, as *scientific* empirical, rather than merely empirical. Let us consider several such propositions.

Joseph Agassi [2, p. 34-35] proposed that empirical science can be characterised by the following two criteria: 1) «the problems of science are related to explanations of facts, or, more generally, to comprehension of the

world», and 2) «the criticism of science is empirical». He identified Popper's view as such that «characterised science by the second criterion alone». Agassi's further suggestion is that Popper «thought ... that when we take care of the second criterion, that will also take care of the first: he thought, in other words, that good explanations are well open to empirical criticism, and *vice versa*». «On this, — Agassi claims (regrettably, without explaining and arguing), — he is in error.» Note that Agassi's suggestion about what Popper thought is logically deficient: its second part, which follows the phrase «in other words» and so is intended to be a mere rephrasing of the first part, is not in fact identical with (or follow from) it; only «*vice versa*» is (follows). The view that 2) implies 1) («when we take care of the second criterion, that will also take care of the first») does not entail that 1) implies 2) («good explanations are well open to empirical criticism»). So we need to consider only the first part — the proposition (supposedly held by Popper) that if a theory is empirically criticisable («falsifiable», in Popper's terms), then it is «related to explanations of facts, or, more generally, to comprehension of the world». If this is false, then the proposition to supplement Popper's criterion of demarcation with 1) seems reasonable; otherwise, such a supplementation is redundant. However, for a theory to be empirically falsifiable, the theory (or its conjunction with empirically ascertainable initial conditions) should entail some empirically testable predictions: if these turn out to be false, the theory (or the system that includes the theory and the description of the initial conditions) is falsified, otherwise it explains the facts observed, and so is «related to explanations of facts and to comprehension of the world». So, Popper's assumption that 2) implies 1) seems true after all.

Imre Lakatos [16] made another demarcational proposition that, as far as I see, should be taken as a clarification rather than an alternative to Popper's. He suggests that the distinctive feature of (empirical) scientific theories is their capacity to generate new unexpectable predictions. However, does this really differ from Popper's falsifiability criterion? Surely, for a theory to be empirically falsifiable, there should be some empirical predictions that follow from it (in the conjunction with initial conditions) and that are not already known to be the case. If a theory entails nothing but what we already know to be the case, then it does not risk to be falsified, that is, it is unfalsifiable.

Perhaps we need to consider a possibility of a new theory that generates *some empirical predictions* but they are *not unexpectable*. This can be so if the same predictions follow from those theories we already hold. That means that the new theory we consider has no new empirical content, as

compared with those theories that we already hold. It is unlikely that such a theory has scientific value. However, we can still consider the possibility that it has scientific value because it has some nonempirical advantages over its so far accepted empirical «duplicates» (perhaps, the advantage of simplicity, or unification). In that case, the new theory should not be denied the status of an empirical scientific theory.

A further point made by Lakatos has to do with the well known objection against Popper's falsificationism, that an apparent contradiction between a theory and observable events (or «basic statements») used to test it is not and should not always be taken as a refutation (falsification) of the theory (because the report of the observable events may be false, or the fault may be not with the theory but with the specification of the initial conditions), and there is no clear unambiguous rule to distinguish a genuine falsification from a merely apparent one. This admitted, there is no clear distinction between theories that are falsifiable and those that are not. So Lakatos claims that «Popper's criterion ignores the remarkable tenacity of scientific theories» [16, p. 3-4]. However, this objection is easily answerable. Indeed, as Popper explained many times, an apparent conflict between a theory and a basic statement *is not* to be equated with a falsification of the theory. We need yet to make our judgement on where to put the blame, and this may involve further discussions, research, and conjectures, and this process can take much time ... What is important, however, is that a theory is at least problematised, or challenged, by an observational report. And, I suggest, this is enough for our demarcational purpose. Perhaps, we would make things clearer if we use some weaker term — let's say, «empirical criticisability» (the term suggested by Joseph Agassi [2, p. 35]) — rather than «falsifiability» to designate this demarcational criterion.

Yet another relevant proposition was made by Thomas Kuhn, who described it as a list of the conditions of «maturity» of «fields which aim to explain in detail some range of natural phenomena». The list includes:

1) Popper's demarcation criterion, falsifiability, «without which no field is potentially a science»;[7]

2) considerable and consistent predictive success: «for some interesting sub-class of phenomena, whatever passes for predictive success must be consistently achieved»;

3) «predictive techniques must have roots in a theory which ... simultaneously justifies them, explains their limited success, and suggests means for their improvement in both precision and scope»;

[7]So Kuhn recognises that falsifiability is a necessary condition of science.

4) «the improvement of predictive technique must be a challenging task, demanding on occasions the very highest measure of talent and devotion» [13, p. 245-246].[8]

I propose to consider a more intuitive and less technical approach — a theory (hypothesis) qualifies as an empirical scientific theory if it satisfies the following conditions:

1) it is falsifiable;

2) it is relevant to some problems that are there within the existing complex of empirical sciences;

3) at the moment when it is advanced, it does not look hopeless in the light of the rest of scientific knowledge.

The condition 3) can be detailed as the disjunction: // — either 3.1) it is a theory tentatively accepted as true because it has already passed successfully severe empirical tests, and was not falsified;// — or 3.2) it is a new promising scientific theory — one that // 3.2.1) satisfies the conditions 1) and 2), // 3.2.2) agrees with the bulk of well-corroborated scientific knowledge, // 3.2.3) was not well tested as yet but is going to;// — or 3.3) it is a scientific theory recognised as false — one that once satisfied the condition 3.1 or 3.2 but was eventually falsified (especially, if this falsified scientific theory retains its scientific importance because it approximates truth very well in wide important areas and has the advantage of simplicity over those theories that are presently accepted as true, — like Newton's physical theory after it was superseded by Einstein's theory).[9]

The requirement 3.2.2 for a new hypothesis to qualify as scientific (before it is subjected to empirical testing designed specially for that

[8]Note that this proposition is not logically dependent on or continuous with the theory that made Kuhn famous — the theory of normal science and scientific revolutions [14]. On the contrary, Kuhn formulated it in response to the criticisms of his conception of normal science by Popper and others [9; 15; 24; 31], and it is a development of Popper's proposition. And it contradicts Kuhn's provocative thesis that evoked most vigorous Popperian criticisms: «it is precisely the abandonment of critical discourse that marks the transition to a science» [12, p. 6].

[9]Note that except for new scientific theories that were just advanced and not tested as yet, this mirrors Popper's specification of the «three requirements for the growth of knowledge», which are

1) that «[t]he new theory should proceed from some *simple, new, and powerful, unifying* idea about some connection or relation (such as gravitational attraction) between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitational mass) or new "theoretical entities" (such as field and particles)»,

2) «that the new theory should be *independently testable*»,

3) that «it should be successful in some of its new predictions» and «is not refuted too soon — that is, before it has been strikingly successful» [23, p. 240-241, 247].

purpose) is a generalisation of the point Mario Bunge makes with respect to testing in medicine: a hypothesis is to be taken seriously by scientists, and subjected to empirical testing, only if it agrees with the bulk of the contemporary scientific knowledge, because severe testing of hypotheses that do not satisfy this condition would be wasting time, effort, and costs.[10] (This is especially appropriate in cases, which are usual in contemporary mature sciences, when testing a hypothesis would require *much* time, effort, and costs.)

As for the case 3.3), such theories are to be characterised as scientific only with the complementary characterisation «false». If a scientific theory was falsified but its partisans ignore or conceal this, and promote the theory as if it is true, this may qualify as pseudoscience.

In conclusion, we can try to outline a wider notion of science, not merely empirical science. We may (and often do) tend to qualify as science disciplines that do not satisfy the falsifiability criterion. The clearest examples are mathematics and logics; some social sciences and humanities may also be the case.[11] Can we find the criterion of scientific character in a relevantly wide sense that goes beyond *empirical* sciences? It is unlikely that we can find such a criterion except a pretty vague one that allows for a wide divergence in judgements. Some necessary but not sufficient conditions (such as intrinsic coherence and agreement with the bulk of the empirical scientific knowledge) can be formulated, but they do not seem to be sufficiently selective. More characteristic but vague conditions are critical attitude and systematic character. Note that Popper, besides proposing falsifiability as the criterion of the empirical character of theoretical systems, formulated the notion of science, in the wide sense, as systematic search for truth based on critical attitude. In particular, in *The Poverty of Historicism* Popper described «scientifically minded technology» as «systematic approach ... based on critical thought as well as on experiment», and identified scientific methods with *critical methods* [30, p. 87], and in *Realism and the Aim of Science* he described the method of science without mentioning falsification and falsifiability:

[10] Cf.: M. Bunge: «Scientific therapies ... must be plausible in light of extant knowledge. ... The plausibility ... of therapy consists in its compatibility with the bulk of biomedical knowledge, and is assessed prior to any trials» [4, p. 129].

[11] The classificatory practice of one of the two leading citation databases, Web of Science, is characteristic: first, there are three main indexing databases – Science Citation Index, Social Sciences Citation Index, Arts & Humanities Citation Index, – so social disciplines, although called «sciences», do not qualify as «science»; second, mathematical disciplines are indexed in the first database, so they qualify as «science».

> The only things which the partners in an argument must share are the wish to know, and the readiness to learn from the other fellow, by severely criticizing his views ... and hearing what he has to say in reply. ... *the so called method of science consists in this kind of criticism.* Scientific theories are distinguished from myths merely in being criticisable, and in being open to modifications in the light of criticism [26, p. 7].

## References


1. *Agassi J.* Popper and His Popular Critics. Springer, 2014.
2. *Agassi J.* Science in Flux. Dordrecht, Boston : D. Reidel Publishing Company, 1975.
3. *Agassi J.* The Nature of Scientific Problems and Their Roots in Metaphysics. The Critical Approach to Science and Philosophy. London : The Free Pree of Glencoe, 1964.
4. *Bunge M.* Medical Philosophy. London, New York : World Scientific, 2013.
5. *Bunge M.* What is Pseudoscience? *The Skeptical Inquirer.* 1984. Vol. 9. P. 36-46.
6. *Bunge M.* What is science? Does it matter to distinguish it from pseudoscience? *New Ideas in Psychology.* 1991. Vol. 9(2). P. 245-283.
7. *Bartley W.* Theories of demarcation between science and metaphysics. *Problems in the Philosophy of Science.* Amsterdam: North-Holland Publishing Company, 1968. P. 40-64.
8. *Bartley W.* The Retreat to Commitment. 2-nd ed. La Salle, Illinois : Open Court, 1984.
9. *Feyerabend P.* Consolations for the specialist. Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970. P. 197-230.
10. *Cioffi F.* Psychoanalysis, pseudo-science and testability. Popper and the Human Sciences. Dordrecht, Boston, Lancaster : Martinus Nijhoff Publishers, 1985. P. 13-44.

11. *Grünbaum A.* Is freudian psychoanalytic theory pseudosScientific by Karl Popper's criterion of demarcation? *American Philosophical Quarterly.* 1979. Vol. 16(2). P. 131-141.

12. *Kuhn T.* Logic of discovery or psychology of research? Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970. P. 1-23.

13. *Kuhn T.* Reflections on my critics. Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970. P. 231-278.

14. *Kuhn T.* The Structure of Scientific Revolutions. Chicago : University of Chicago Press, 1962.

15. *Lakatos I.* Falsification and the methodology of scientific research programmes. Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970. P. 91-196.

16. *Lakatos I.* Science and pseudoscience. The Methodology of Scientific Research Programmes. Cambridge University Press, 1978. P. 1-7.

17. *Laudan L.* The Demise of the demarcation problem. *Physics, Philosophy and Psychoanalysis.* Dordrecht: D. Reidel, 1983. P. 111-127.

18. *Laudan M.* Science and pseudoscience. How to demarcate after the (alleged) demise of the demarcation problem. Philosophy of Pseudoscience. Chicago, London : The University of Chicago Press, 2013. P. 29-44.

19. *McIntyre L.* The Scientific Attitude: Defending Science from Denial, Fraud, and Pseudoscience. Cambridge, London : The MIT Press, 2019.

20. *Miller D.* Being an absolute skeptic. *Science.* 1999. Vol. 284. P. 1625-1626.

21. *Miller D.* Critical Rationalism: A Restatement and Defence. Chicago and La Salle, Illinois : Open Court, 1994.

22. *Pigliucci M.*, Boudry M. (eds.) Philosophy of Pseudoscience. Chicago, London : The University of Chicago Press, 2013.

23. *Popper K.* Conjectures and Refutations. London : Routledge and Kegan Paul, 1962.

24. *Popper K.* Normal science and its dangers. Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970. P. 51-58.

25. *Popper K.* Objective Knowledge. An Evolutionary Approach. Oxford University Press, 1972.

26. *Popper K.* Realism and the Aim of Science. London, New York : Routledge, 1983.

27. *Popper K.* Remarks on the Problems of Demarcation and Rationality. Problems in the Philosophy of Science. North Holland Publishing Company, 1968. P. 88-102.

28. *Popper K.* The Logic of Scientific Discovery. New York : Harper & Row, 2005.

29. *Popper K.* The Open Society and Its Enemies. Vol. 2. London : George Routledge & Sons, 1945.

30. *Popper K.* The Poverty of Historicism. London : Routledge & Kegan Paul, 1957.

31. *Watkins J.* Against «Normal science». Criticism and the Growth of Knowledge. Cambridge, London : Cambridge University Press, 1970.

# Проблема демаркації: розв'язання Карла Поппера у сучасній ретроспективі

**Дмитро Сепетий**

**Анотація**. У статті проаналізовано постпопперівські дебати щодо проблеми демаркації. Обґрунтовано думку, що пропозиція Карла Поппера відмежовувати емпіричну науку від решти («метафізики») на основі фальсифікабельності як критерію емпіричного характеру теорій залишається доречним способом бачення найбільш цінного аспекту характеру наукової діяльності. З'ясовано, що для того, щоб критерій фальсифікабельності був адекватним поставленій меті, його слід розглядати, як це робив Поппер, як двоаспектний, що включає логічний зміст теорій та критичне ставлення дослідників. Хоча фальсифікабельність у цьому сенсі не є достатньою для чіткого розмежування науки і ненауки, її можна розглядати як найважливішу необхідну умову, яка може бути доповнена деякими іншими вимогами для того, щоб емпірична гіпотеза могла вважатися науковою. Зокрема, показано, що пропозиція Лі Макінтайра розглядати емпіричну науку як таку, що визначається описаною ним «науковою настановою», цілком вписується в цю фальсифікаційну перспективу. Піддано критиці дефляційні підходи до проблеми демаркації Вільяма Бартлі та Гаррі Лаудана, як такі, що не відповідають практичній значущості розрізнення того, що є і що не є емпіричною наукою, а також необхідності теоретичного обґрунтування цієї розрізнювальної практики, хоча б і недосконалого. Обговорено пропозиції Джозефа Аґассі, Імре Лакатоша та Томаса Куна, спрямовані на вдосконалення демаркаційної пропозиції Поппера, а також запропоновано і пояснено власну пропозицію автора. Також зазначено, що зауваження Поппера про те, що визначальною рисою наукових теорій є відкритість до критики та модифікації в її світлі, може слугувати основою для ширшої концепції науки, без кваліфікації «емпірична».

**Ключові слова**: демаркація, фальсифікація, наука, емпіричний, метафізика, псевдонаука.

*Надійшла до редакції 11 вересня 2024 р.*

**Сепетий Дмитро Петрович**
Кафедра суспільних дисциплін
Запорізький державний медико-фармацевтичний університет
бульвар Марії Примаченко, 26
м. Запоріжжя
69035

**Sepetyi Dmytro**
Department of Social Studies
Zaporizhzhia State Medical and Pharmaceutical University
Marii Prymachenko blvd., 26
Zaporizhzhia
69035

https://orcid.org/0000-0003-2110-3044

dsepetij@ukr.net

https://doi.org/10.31812/apm.7730