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ARTICLE

Demarcation without Dogmas

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Abstract

This paper reviews how research on the demarcation problem has developed, starting from Popper's criterion of falsifiability and ending with recent naturalistically oriented approaches. The main differences between traditional and contemporary approaches to the problem are explicated in terms of six postulates called the *traditional assumptions*. It is argued that all of the assumptions can be dismissed without giving up on the demarcation problem and that doing so might benefit further discussions on pseudoscience. Four present-day research movements on evaluating the boundaries of science are introduced: (1) philosophy of pseudoscience, (2) social epistemology of dissent, (3) agnotology, and (4) evaluation of expertise. Researchers working within these areas have abandoned some or all traditional assumptions.

KEYWORDS

agnotology, demarcation, expertise, naturalistic turn, pseudoscience, social epistemology

1 | INTRODUCTION: WHY DEMARCATION MATTERS

Science[1] is one of the most trusted epistemic authorities (Wellcome Global Monitor, 2018, p. 50). Due to the esteem it enjoys, it is no wonder that its status has been repeatedly challenged and misused (see, e.g., Proctor & Schiebinger, 2008; Pigliucci & Boudry, 2013a; Harker, 2015). On the one hand, there is science *denialism*, such as climate change scepticism, the anti-vaccination movement, and holocaust denial, which attacks well-established scientific theories and practices. On the other hand, there is the promotion of *pseudotheory*, the attempt to get doctrines like homoeopathy and intelligent design accepted as sciences even though they have no warrant for such merit[2] (Hansson, 2017). Both types of pseudoscience have harmful effects

[1]Following Boudry (2017, p. 38) and Hansson (2013, pp. 63–65), we will use the term science as a rough equivalent to the German concept *Wissenschaft* which includes all branches of academic research. Hansson (id., p. 65) argues that "the creation of a new category for the 'pseudohumanities' is unwarranted because the phenomenon overlaps and largely coincides with that of pseudoscience."

[2]Besides promoting a pseudotheory, both also encompass a form of science denialism, namely, rejecting scientific knowledge about chemistry and evolution (Perakh & Young, 2004, pp. 186–187).

on health, environment, education, and society (Pigliucci & Boudry, 2013b, p. 3). Thus, Imre Lakatos was quite right when he declared that "the demarcation between science and pseudoscience is not merely a problem of armchair philosophy: it is of vital social and political relevance" (Lakatos, 1978 [1973], p. 1).[3] This seems particularly true today because evidence indicates that pseudoscientific content is spreading rapidly through social media (see Allgaier, 2019).

To avoid the perils of pseudoscience, we must recognise and separate it from science. This, in a nutshell, is what the demarcation problem is about. However, some novel approaches on demarcation do not discuss *pseudoscience* explicitly. Instead, they aim to draw a line between trustworthy and untrustworthy by other means. Despite this, we treat them as approaches to the demarcation problem because the criteria employed in them can also be applied to pseudosciences. Hence, those approaches can be used to solve the demarcation problem, as we argue in later sections.

Demarcation can be roughly divided into two types, which we call *general* and *specific*. *General demarcation* is about providing blunt instruments for laypersons. Because of the continuous growth of information, no one can be an expert in everything or evaluate all claims by themselves. Therefore, a division of epistemic labour is necessary, and something has to be accepted on trust. However, this raises a problem: who should we, as non-experts, trust? We need guidelines on recognising reliable epistemic authorities and suspending our trust without becoming experts ourselves.

Due to our cognitive limitations, general demarcation is usually based on heuristic principles or rules of thumb. A demarcation criterion is not useful for non-experts if they cannot remember, understand, or apply it. A trade-off of cognitive simplicity might be occasional false positives and negatives. However, even imprecise criteria can be helpful if they guide predominantly in the right direction. Thus, in general demarcation, the term *general* refers to three things: First, the criteria are meant for the general public instead of experts. Second, the criteria may be general in nature, as in ignoring some of the details. Third, they should have a general scope. That is, they should apply to a wide variety of cases instead of only a few.

Whereas general demarcation is about giving blunt tools for non-experts, *specific demarcation* concentrates on formulating reliable surgical instruments for expert use. These more sophisticated tools are meant to be applied in precise tasks with a limited scope. General criteria might, of course, be applied in specific demarcation, even when detailed knowledge about a specific science is required. At times, a detailed explanation is needed of why some method, practice, claim, or theory is pseudoscientific. Situations like these might occur, for instance, in courtrooms (see, e.g., Lee, 2006; Jupe & Denault, 2019) or within the academic debates in the philosophy and history of science. Explaining in detail why some instance of pseudoscience is pseudoscience may demand in-depth knowledge about the field of inquiry that it deals with. This could require one to answer such questions as: What kind of methods qualified scientists use in the field? Why and how do these practices differ from the pseudoscientific ones? What scientific claims do pseudoscientific theses contradict? How are scientific claims justified? More examples abound.

When the demarcation problem emerged in the 1930s (Popper, 2005 [1935]), the general and specific tasks were not separated. Back then, philosophers believed that one could develop a single decisive marker of science for separating it from pseudoscience. However, such a criterion has not been found, and the traditional approaches to demarcation have encountered heavy criticism (see Laudan, 1983; Soler et al., 2014, pp. 15–16). The most famous critic was undoubtedly Larry Laudan (1983), who declared that the demarcation problem is a mere pseudo-problem. Similar views are still expressed today (see Harker, 2017, pp. 248–250).

[3] Laudan (1983, pp. 118, 124) has argued that instead of demarcating science and pseudoscience, one should distinguish between more and less epistemically warranted claims. Although at first blush Laudan's suggestion might appear detrimental for the demarcation problem, the difference between the two distinctions seems to be superficial. In the end, both aim to distinguish reliable claims from unreliable ones and, arguably, Laudan's aims could be achieved with methods developed for demarcating science from pseudoscience.

We will call the classical attempts of solving the demarcation problem *the traditional solutions*. Unlike Laudan and his followers, we – with many others – do not think that the failure of the traditional solutions was due to the unattainability of the project as a whole (see, e.g., Pigliucci & Boudry, 2013b, pp. 1–3; Hansson, 2013, p. 62; Ladyman, 2013, pp. 48–52). Instead, the lack of success was, at least in part, a consequence of six strict and unnecessary presumptions on which the traditional solutions relied. We shall call these presumptions *the traditional assumptions*. Some of these were challenged relatively early on (see Kuhn, 1974 [1965], pp. 802–8030); however, after the so-called naturalistic turn in philosophy of science (see Bechtel, 1993; Longino, 2006; Soler et al., 2014), more and more of them have been questioned. Still, the assumptions cast a long shadow. Philosophers have not entirely freed themselves from them even today.

In this paper, we will examine the historical development of the demarcation problem from the perspective of the traditional assumptions. In section 2, the assumptions are presented, along with an analysis of the traditional solutions. Section 3 is dedicated to a short overview of how the naturalistic turn changed the way philosophers approach demarcation. There we will introduce four novel research movements that have retained demarcation objectives while discarding many of the traditional assumptions. These approaches are (1) philosophy of pseudoscience, (2) social epistemology of dissent, (3) agnotology, and (4) evaluation of expertise. In section 4, we argue that the traditional assumptions are not necessary for demarcation endeavours. Thus, renouncing them does not force us to give up on the demarcation problem, and doing so might benefit further discussions on pseudoscience.

2 | THE TRADITIONAL ASSUMPTIONS

The traditional solutions to the demarcation problem are based on several assumptions that determine what the solutions should be like. As such, they pose unnecessary demands on how the problem should be approached. The assumptions, which are still influential in current analytic philosophy, are characteristic of how the logical positivists approached science. Depending on one's goals and interests, there are several possible ways to formulate the traditional assumptions. We ended up with the following six because they often recur in the literature:

1. **Defining only science:** Demarcation criteria need only to explicate what science is. The contraries of science, such as pseudoscience, are defined merely as something that does not meet the standards of science (see Soler et al., 2014, p. 15).
2. **Necessary and sufficient conditions:** The definition of science must consist of individually necessary and jointly sufficient conditions. Necessary conditions are particularly emphasised. Eligible standards of science are required to be accurate, unequivocal, strict, and exact (see Laudan, 1983, pp. 112, 118–119; Hansson, 2013, p. 71).
3. **Universality:** The criteria of science are universal. The same criteria have to apply to all sciences (see Feyerabend, 1993 [1975], p. 247; Soler et al., 2014, p. 11).
4. **Focus on end products:** The criteria should explicate what the end products of science are like. Possible end products can be theories, axioms, sentences, successfulness, progress, ability to solve problems, and so on (see Longino, 2002a, p. 38; Soler et al., 2014, pp. 14–15; Imbert, 2014, p. 176).
5. **Scarcity of criteria:** Only a few criteria are needed.[4] The first paradigmatic proposals consisted of only one criterion (e.g., Schlick, 1948 [1932], pp. 483–498; Carnap, 1959 [1932], pp. 62–66, 73, 76; Popper, 2005 [1935], pp. 17–20; Popper, 1974, pp. 980–981). Gradually, it became more common to capture science with a short list of conditions (e.g., Lakatos, 1978a

[4]Mahner (2013, p. 32) has made a similar point.

[1970], p. 34; Kuhn, 1974 [1965], pp. 801–805; Thagard, 1978, pp. 227–228; Kitcher, 1984 [1982], p. 48; Bunge, 1984, pp. 39–40; Dutch, 1982, pp. 7–12; Radner & Radner, 1982; Hansson, 1983, pp. 36–58).

6. **Focus on formal features:** The criteria should explicate the general logic of science, usually understood as the scientific method. The scientific method consists of a general argument form (or forms) such as modus tollens. The evaluation of theories may require their rational reconstruction. The reconstruction reveals whether the logical form of the theories is such that theories can be justified (not necessarily discovered) by using the scientific method (see Popper, 1989, pp. 82–83, Longino, 2002a, p. 11; Boudry, 2013, p. 92).

It is a matter of degree whether a solution to the demarcation problem should be considered as traditional. In addition, none of the six assumptions are individually necessary. In other words, traditional solutions presuppose more of the assumptions than novel ones, and the gradual differences in the old and new approaches reveal fundamental changes in how philosophers have addressed the problem.

Karl Popper is conventionally credited as the first philosopher to tackle the modern demarcation problem.[5] Before him, logical positivists had formulated their verification principle, but they were not focused on the demarcation between science and pseudoscience. Instead, the positivists were trying to sort out scientific statements from metaphysical ones. The basic idea behind verificationism was that metaphysical statements are meaningless because they cannot be confirmed or disconfirmed (see Schlick, 1948 [1932], pp. 482, 503, 505; Carnap, 1959 [1932], pp. 62–66, 72–73, 76–77; Carnap, 2005 [1928], p. 327; Hempel, 1950, p. 43; Ayer, 1951 [1936], pp. 31, 35–39).

Popper (2005 [1935], pp. 42, 249–252; Popper, 1974, pp. 965–974) criticised verificationism by noting that it is easy to provide confirmation for any given theory if one is only looking for that.[6] He proposed that we should instead take falsifiability or refutability as the criterion of science. Falsifiability is, therefore, a narrower criterion than verification because verification allows both confirmation and disconfirmation. A theory is pseudoscientific if it cannot be proven wrong with any conceivable tests (Popper, 2005 [1935], pp. 17–20; Popper, 1974, pp. 980–981; Popper, 1989, pp. 33–41). Popper's criterion meets all of the traditional assumptions: it consists of a singular formal and universal condition that focuses on end products. Popper also claimed that his criterion is both necessary and sufficient, although some have interpreted it to be merely necessary (Popper, 1989, p. 82; Feleppa, 1990, p. 142).

After Popper, Thomas Kuhn and Lakatos proposed their approaches to demarcation. According to Kuhn, Popper only described revolutionary science because he focused on testing and crucial experiments. For Kuhn, the significant feature of science is its capability to solve puzzles[7] (Kuhn, 1974 [1965], pp. 801–805). Lakatos' idea is somewhat similar. He believed that "a research programme" has to be progressive in order to count as scientific. In Lakatos' view, there are two types of scientific progress: theoretical and empirical. Theoretical progress comes from new theories that predict more than their predecessors. Empirical progress, in turn, requires the confirmation of these predictions. Lakatos thought a research program is scientific if it is at least theoretically progressive; otherwise, it is pseudoscience (Lakatos, 1978a [1970], p. 34).

[5]There have been multiple demarcation debates during the history of science and within philosophy of science. Even the concept of *pseudoscience* and other synonymous words were used before Popper (see, e.g., Rupnow et al., 2021 [2008], p. 12). To be precise, the term dates back to the end of the eighteenth century (Hansson, 2021). However, Popper can be credited for bringing the demarcation between science and pseudoscience at the heart of philosophy of science.

[6]However, Schlick (1936, p. 341), for instance, claims that verifiability and falsifiability are merely two sides of the same coin.

[7]According to Kuhn, "[p]uzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity or skill in solution" (Kuhn, 1974 [1965], p. 36).

Kuhn's and Lakatos' proposals also meet most of the traditional assumptions. "Puzzle-solving" and "progress" are requirements for the end products of research, although both philosophers were also interested in other factors. Moreover, Kuhn and Lakatos never stated that their criteria should apply only to some specific sciences. Thus, presumably they are meant to be universal. Because Kuhn and Lakatos made no additional qualifications, their criteria appear to be necessary, sufficient, or both. Like falsifiability, these standards seem to be intended as decisive features of science. However, the fact that puzzle-solving and progressiveness seem to be matters of degree makes it questionable whether they could actually function as necessary or sufficient conditions. Thus, arguably, in their solutions the demand for necessary and sufficient conditions is relaxed. The only other traditional assumption that Kuhn and Lakatos neglect is focusing on formal features. Kuhn clearly does not presume a general logic of science because he does not explicate what kind of problems need to be solved or how they should be solved. Likewise, Lakatos does not apply formal tools in analysing the progressiveness of research programmes.[8] In addition, neither of them engage in rational reconstruction or anything of the like.

Other traditional solutions were all more or less inspired by Popper, Kuhn, and Lakatos. However, in the 1970s and 1980s, there was a shift from single to multicriteria approaches (see Bunge, 1984, pp. 39–40; Dutch, 1982, pp. 7–12; Radner & Radner, 1982; Hansson, 1983, pp. 36–58; Hansson, 2013, pp. 71–72).[9] Despite the increase of demarcation conditions, the multicriteria proposals held on to most of the traditional assumptions. For example, Paul Thagard stated that a theory or a discipline posing as science is pseudoscientific if and only if it satisfies the following two criteria: (1) It is significantly less progressive than alternative disciplines or theories and unable to solve the problems outlined for it; and (2) Its practitioners do not try to solve problems or evaluate their theories by comparing them to other theories, and the practitioners are selective in considering confirmations and disconfirmations (Thagard, 1978, pp. 227–228). Both of Thagard's criteria seem to contain several parts, but he takes the features within the criteria as parallel.

Thagard's suggestion is very similar to Kuhn and Lakatos', with two exceptions concerning the traditional assumptions. First, his second criterion is not only about the end products of science. Instead, it focuses on research practices. Second, Thagard's account does not give the jointly necessary and sufficient conditions of science but pseudoscience. However, the definition of pseudoscience is clearly connected to the criteria of science. If the two conditions are necessary and sufficient for pseudoscience, then it is a necessary condition of science that it does not fulfil at least one of them. After all, every sufficient criterion for pseudoscience provides, by negation, a necessary condition for science. In this sense, the definitions of science and pseudoscience are still treated as two sides of the same coin.

A few years later, Philip Kitcher offered his list of demarcation criteria. He thought good science has three essential features: (1) Its auxiliary hypotheses can be tested independently; (2) it is unified so that it applies a small set of methods to a broad class of cases; and (3) it is fruitful in the sense that incomplete theories open up new lines of research (Kitcher, 1984 [1982], p. 48). Kitcher's proposal consists of a few criteria that form universal, necessary, and sufficient conditions of good science. Moreover, (auxiliary) hypotheses and incomplete theories can be seen as end products of science, so even the fifth traditional assumption is fulfilled. In addition, Kitcher thinks that explanatory unification is achieved through formal argument patterns (Kitcher, 1989, pp. 432–435). Therefore, all of the traditional assumptions are more or less met. However, in one sense, Kitcher diverts from the assumptions. He states that the distinction between science and pseudoscience is a matter of degree: "Where bad science becomes egregious

[8]However, it is possible to formulate his notion of progress using formal means (see Schurz, 2018).

[9]There had been a few forerunners of the multicriteria approach. Among them are Langmuir (1989 [1953], pp. 16) and Gruenberger (1964, pp. 1414–1415).

enough, pseudoscience begins" (Kitcher, 1984 [1982], p. 48). Kitcher does not explicate where this threshold lies, but he nevertheless clearly takes the distinction between good science, bad science, and pseudoscience to be gradual. Usually, the traditional solutions insist on clear-cut criteria without fuzzy borderlines.

When multicriteria approaches became common, Laudan published his famous article, "The Demise of the Demarcation Problem" (1983). In it, he claimed that it is impossible to give a satisfying answer to the problem. However, Laudan's pessimistic conclusion was based on the very same assumptions as the solutions he rebuked. He believed that the demarcation problem cannot be answered because certain traditional assumptions cannot be met. Laudan claimed that demarcation criteria must spell out universal, necessary, and sufficient conditions of science (Laudan, 1983, pp. 112, 118–119). According to him, merely necessary conditions would not do because they only help to identify unscientific activities. Such criteria would not tell what to trust because they would not say what counts as proper science (id., p. 118). Likewise, only sufficient conditions would be unsatisfactory because the fact that something "failed to satisfy a set of *merely sufficient* conditions for scientific status would leave it in a kind of epistemic twilight zone – possibly scientific, possibly not" (id., p. 119, italics in the original). With sufficient conditions, one can only say that something is scientific but not that something else is not. Therefore, in Laudan's mind, such conditions do not give satisfactory grounds for discarding pseudoscience; both necessary and sufficient criteria are required. Laudan also states that demarcation criteria ought to tell why science has a "surer epistemic warrant" or "evidential ground" compared to non-science (id., p. 118). Thus, the tendency of defining pseudoscience through science is clearly visible.

3 | NOVEL APPROACHES TO DEMARCATION

Laudan's (1983) criticisms were highly influential. In fact, they caused the interest in the demarcation problem to decrease for a couple of decades (Mahner, 2013, pp. 29–30; Pigliucci & Boudry, 2013b, p. 2). There were some publications on the topic,[10] but the proposals received no general support. However, at the same time when Laudan's article came out, fundamental changes, which also had a substantial effect on the demarcation problem, were taking place in philosophy of science.

The 1970s and 1980s served as a stage for an important revolution in philosophy. This shift is conventionally referred to as the naturalistic, practice, or social turn (see Bechtel, 1993; Longino, 2006; Soler et al., 2014). Here, naturalism is understood in Quinean terms. According to Quine, it consists of the rejection of first philosophy. Reality is to be identified and described in science and not in something that precedes it (Quine, 1981, p. 21, 67). Philosophy and science lie in the same continuum; there is no clear distinction to be made between the two (Quine, 2013 [1960], p. 3). The naturalistic turn has its roots in the historically oriented philosophy of science that began with Kuhn's *The Structure of Scientific Revolutions* Kuhn (1964 [1962]). In the 1960s, it became common for philosophers to use historical analysis in justifying their normative claims on science (Lakatos, 1978 [1973]; Soler et al., 2014, p. 12). To the naturalists, there is no clear distinction between normative or descriptive solutions to questions of demarcation. After all, descriptions of trustworthy epistemic activities can be used as criteria. As Kuhn (1970, p. 237) answered when asked whether his views concerning scientific development are descriptive or prescriptive,

> The answer, of course, is that they should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish.

[10]These include, for instance, Hansson (1983), Grove (1985), Dolby (1987), Thagard (1988), Rothbart (1990), Glymour and Stalker (1990), Derksen (1993, 2001), Vollmer (1993), Ruse (1996), Reisch (1998), and Resnik (2000).

Naturalism arose, in part, from criticising philosophy of science that was heavily influenced by logical positivism. Before naturalism, philosophers often relied on rational reflection in order to contrive universal norms for science. This led to conceptions that were distant from actual scientific practices. Naturalistic philosophers insisted on formulating their views based on what scientists are really doing and not on what philosophers think they are or should be doing (see Feyerabend, 1993 [1975], pp. 148, 193, 261; Soler et al., 2014., p. 15).

Naturalism forced philosophers to abandon the idea of science as a homogeneous endeavour. Instead, it was accepted that methodologies vary across time and fields of study (Soler et al., 2014, p. 11, Feyerabend, 1993 [1975], p. 247; Laudan, 1983, p. 125).[11] After this, there has been little demand for universal conceptions on what all science should be like (Longino, 2002a, p. 37; Soler et al., 2014, p. 18; Israel-Jost & Kinzel, 2014, pp. 117–118). It also became mainstream to see scientific normativity instrumentally: success should be evaluated through how well individual research cases achieve their goals because diversity in science prevents strict generalisations (Laudan, 1990, p. 47; Maffie, 1990, p. 333; Quine, 1992, p. 19).

The shift of focus to actual scientific practice and its empirical study paved the way for interdisciplinary research in philosophy. It became habitual for philosophers to use insights from social sciences, psychology, cognitive science, and economics. Consequently, more attention has been paid to the social interaction between researchers, the communal aspects of science, and its self-corrective processes (see Longino, 2002a, p. 38; Longino, 2006; Soler et al., 2014, p. 20; Imbert, 2014, p. 176; Godfrey-Smith, 2009, pp. 126–128). There were also changes in methodology. Traditional philosophy of science mainly used methods from other fields of philosophy, such as logic, epistemology, and philosophy of language. Within the last decades, the philosopher's toolbox has broadened to include, for example, participant observation, experiments, and historical methods.

Naturalism also has had its effect on the demarcation problem. The traditional solutions have been criticised, from a naturalistic point of view, predominantly for two deficiencies. First, many of the solutions are based on simplified and erroneous conceptions of science and pseudoscience, such as crude falsificationism (see, e.g., Hansson, 2006). Second, they are too loose or sketchy for many real-world cases (see, e.g., Laudan, 1983, pp. 111–112). However, the interest in the demarcation problem has resurrected (see, e.g., Pigliucci & Boudry, 2013a). There are also ongoing debates on the pseudoscientific status of specific theories, such as creationism and climate change scepticism (see, e.g., Pennock & Ruse, 2009; Kojonen, 2016, chap. 2; Björnberg et al., 2017; McKinnon, 2016). Identifying problems in distinguishing science from pseudoscience in such concrete cases is one of the main reasons for shifting to the naturalistic approaches.

To better understand the current approaches to demarcation, we will take a closer look at four of them: philosophy of pseudoscience, social epistemology of dissent, agnotology, and evaluation of expertise. This list is not meant to be exhaustive. The items on it are merely four salient contemporary approaches that have received a fair amount of attention. But they illustrate well how the naturalistic turn has affected research on demarcation.

3.1 | Philosophy of pseudoscience

The authors of *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem* (2013) propose "something of a new philosophical subdiscipline, the Philosophy of Pseudoscience" (Pigliucci & Boudry, 2013b, p. 2). As the book's name implies, philosophy of pseudoscience is a self-declared successor for the old demarcation problem. The authors reject Laudan's

[11]There are also proponents of naturalism who do not claim that methodologies vary – at least essentially – across time and fields of study. For example, some methodological naturalists argue that the unity of science is methodological in nature (see, e.g., Schurz, 2014, p. 46–50, 268–272). In addition, some claim that there are meta-methodological principles, such as evaluability, that all sciences share (Turunen et al., 2022, 93–98).

conclusion that the demarcation problem is impossible to solve. Instead, they tackle the problem pseudoscience-first: they do not consider it necessary to start with a definition of science and then define pseudoscience on its basis. Pseudoscience is seen as an interesting phenomenon in its own right (see Boudry, 2011, pp. 3, 246; Pigliucci & Boudry, 2013b, p. 2). This already shows that the first traditional assumption, defining only science, has been forsaken. However, the philosophy of pseudoscience is a diverse branch. All who participate in it do not automatically disregard the same traditional assumptions, although some clear trends are visible.

Defining only science is not the only traditional assumption that philosophers of pseudoscience have given up on. For example, Maarten Boudry – following Laudan, John Dupré, and Koen Vermeir – has suggested that there is no need for clear-cut necessary and sufficient conditions. "Pseudoscience" can be a family resemblance concept with fuzzy boundaries (Laudan, 1983, p. 124; Dupré, 1993, p. 242; Vermeir, 2009; Boudry, 2011, p. 246; Boudry, 2013). The abandonment of necessary conditions is also evident with other writers. For instance, recent multicriteria proposals often provide disjunctive lists of mistakes that pseudosciences make. Martin Mahner's (2013, pp. 38–41) "cluster approach" is an extreme example of this. He provides almost 20 non-necessary "science indicators" and states that there might be even 30 to 50 of them. No research field will satisfy all of the indicators. Instead, demarcation is based on a stipulation of how many of them science needs to satisfy. It is also possible to emphasise some indicators over others so that they have different relative weights. Therefore, no single criterion or set of criteria has to be universal or necessary. Moreover, Mahner's proposal is obviously not scarce, and many of his indicators do not focus on end products or formal features of science. To sum up, Mahner's approach meets only one of the traditional assumptions: he is defining pseudoscience through science, but in this he is an exception to many other philosophers of pseudoscience.

3.2 | Social epistemology of dissent

After the naturalistic turn, there has been a growing trend of combining findings from history and sociology of science with normative philosophical epistemology (Longino, 2002a, p. 208). Social epistemologists have formulated various social criteria for knowledge production, and these criteria have been used to address demarcation questions (see, e.g., Fuller, 2002 [1988], pp. 187–189; Longino, 2002a, pp. 156–159). Among them are studies on scientists' obligation to engage with dissent. Many social epistemologists value dissent because it enables the exclusion of errors and promotes creativity. Therefore, it is widely agreed that scientists should take dissenting views into account. Yet, this obligation needs to be restricted (Koskinen & Mäki, 2016, pp. 25–26). Otherwise, scientists would be forced to participate in endless debates with denialists, which would considerably slow down research.

Several suggestions have been offered on dealing with detrimental dissent (e.g., Longino, 1990, pp. 66–81; Longino, 2002a, pp. 128–131; Longino, 2002b; Solomon, 2008; Kitcher, 2011, chap. 8; Intemann & de Melo-Martín, 2014; Biddle & Leuschner, 2015, p. 273). For example, Helen Longino (2002a, pp. 129–131) has outlined four social standards for evaluating the objectivity of research communities. According to her, the standards expose crucial problems in pseudoscientific practices – such as creationists' – and thus the criteria can be used to withdraw scientists' obligations towards dissenters (id., pp. 156–159; Koskinen & Mäki, 2016, pp. 25–26). Under Longino's (1990, 76–79) criteria, a research community is objective only if:

1. The community has publicly recognised forums for criticism, such as conferences and journals.
2. Beliefs and theories in the community change over time due to criticism.

3. The community has publicly recognised standards for evaluating theories, hypotheses, and observational practices.
4. Equal intellectual authority is recognised within the community.

Longino's proposal still fulfils many of the traditional assumptions. Her criteria are necessary (although not sufficient) and presumably universal (Longino, 2002a, p. 129). However, it is unclear whether Longino's norms also fulfil assumptions of defining only science and scarcity of criteria. Four conditions are not a lot, but it is still more than what many of the traditional solutions had. Further, Longino is not defining science but the *objectivity* of a scientific community (Longino, 1990, pp. 77–78). However, it would count as splitting hairs if one were to claim that because of this, her standards are not really about science — or at least about good or objective science.

In effect, Longino's proposal dismisses only two traditional assumptions: focus on end products and formal features. Nonetheless, this already gives her account more leeway compared to, for instance, Kuhn's position. Kuhn posed restrictions on scientific results on what research should accomplish in order to be scientific, that is, it should solve puzzles. Longino's proposal does not specify what outcomes scientific inquiry should have or what methods researchers ought to use to achieve them. Scientists can produce whatever conclusions with whichever means they see fit as long as their research is organised in a way that enables effective discursive interaction (Longino, 2002a, p. 129).

Others have proposed different tools for delimiting scientists' obligations towards non-relevant dissenters. For instance, Justin B. Biddle and Anna Leuschner (2015), p. 273) suggest that the following criteria identify epistemically detrimental dissent from a hypothesis:

1. The non-epistemic consequences of mistakenly rejecting the hypothesis are probably severe.
2. The dissent is based on research that violates established scientific standards.
3. The dissenting research focuses, at least in part, on avoiding producer risks at the expense of public risks.
4. Producer risks and public risks fall mainly on distinct groups.

Biddle and Leuschner state that their criteria form "jointly sufficient conditions for epistemically problematic dissent in general" (Biddle & Leuschner, 2015, p. 263). Thus, the assumption of universality is fulfilled, and so is the demand for sufficient conditions. Nevertheless, Biddle and Leuschner do not claim that the criteria are necessary. The requirement for merely sufficient conditions still counts as a significant step away from the path of the traditional solutions. However, the criteria seem to have a somewhat limited scope of application. Biddle and Leuschner formulated their account particularly against climate change scepticism (id., p. 262), and the criteria work pretty well from that perspective. However, they do not apply to, for example, the anti-vaccination movement and Holocaust denialists because producer risks are not similarly relevant in these forms of denialism.

Biddle and Leuschner do not define science but, instead, detrimental dissent – which is common in pseudoscience. Moreover, there is no mention of formal features. Concerning scarcity of criteria, Biddle and Leuschner's account is in the same boat as Longino's. Four conditions are not a lot, but there could be even fewer. The criteria mention the end products of science: detrimental dissent is defined as opposing hypotheses that have significant non-epistemic consequences. However, Biddle and Leuschner are not claiming that for a hypothesis to be scientific it must produce substantial practical effects. In addition, the criteria state that "dissenting research […] violates established conventional standards" (Biddle & Leuschner, 2015, p. 273), but Biddle and Leuschner say nothing about what the conventional standards should be like. On the contrary, they outsource the evaluation of the standards to the scientific community. Therefore, Biddle and Leuschner are not really taking a stance on what end products of science are or should be, and the traditional assumption of focusing on them is not satisfied.

3.3 | Agnotology

Perhaps of all the new naturalistic approaches to demarcation, agnotology[12] has received the most publicity. Agnotologists – who can be philosophers, sociologists, or historians, just to name a few – study ignorance, and especially its intentional production. The term *ignorance* is used in a variety of ways within agnotology. It refers, for example, to doubt, uncertainty, biases, and the lack or incompleteness of knowledge. Ignorance can be created artificially to serve, for instance, financial and ideological interests (Smithson, 1985, p. 168; Proctor, 1995, p. 8). Often the motivation behind this is that it is more effective to attack scientific research than taking part in policy debates (Michaels, 2008, p. 92).

Although agnotologists are not discussing "pseudoscience" explicitly, we share Manuela Fernández Pinto's (2014, p. 113) view that they are concerned with *a* problem of demarcation: distinguishing between "good" or "bad" and legitimate or illegitimate research. Typically, agnotological inquiries are based on case studies in which some form of pseudoscience or other unreliable party is critically examined. In these inquiries, agnologists strive to demonstrate why the research or party in question is unreliable. Moreover, many of their objects of study, such as climate change scepticism and tobacco disease denialism, are also considered forms of pseudoscience (see, e.g., Hansson, 2017, pp. 39–43). Hence, if agnotologists demonstrate how pseudoscientific research is unreliable, they implicitly contribute to *the* demarcation problem. At the very least, their results can be applied by those who are intentionally engaging in this question.

Famous agnotological case studies include the tobacco industry's campaign against the evidence on health effects of smoking and the oil industry's offensive on climate science (Oreskes & Conway, 2008, 2010; Proctor, 1995, 2006). Usually, the producers of doubt are not claiming that mainstream scientists are mistaken. Instead, they try to create an impression that scientific conclusions are insufficiently justified because they are not "ultimate" or "final." This strategy rests on an intentional misunderstanding of scientific fallibilism. Science never ends up with absolutely conclusive results, and it nevertheless makes sense to act in accordance with our best current scientific knowledge. Another way of manufacturing ignorance is by fabricating the appearance of ongoing controversy, even though scientists have achieved a consensus (Oreskes, 2004; Harker, 2017).

Agnotologists rarely endorse *any* of the traditional assumptions. They do not explicitly define what science is. Agnotological studies usually presume that some position is better justified than another one (Pinto, 2014, pp. 113, 212). In other words, it is commonly taken for granted that some piece of research is good science. Agnotologists explain how and why its conclusions were not accepted as knowledge; however, people instead remained doubtful or unsure about them. Agnotology focuses almost entirely on social processes and therefore not on formal features or end products of science (id., pp. 11–12). The lessons learned from them are not claimed to be universal. Nor do they offer necessary or sufficient conditions. Because agnotologists do not try to formulate standards for science, there can be no scarcity of criteria either.

3.4 | Evaluation of expertise

Collins and Evans (2007), pp. 113–133) have yet another approach to demarcation. They have developed a normative theory of expertise (Collins & Evans, 2002, pp. 236–237). So, instead of defining science or pseudoscience, they try to answer the question: whom should we trust?

Collins and Evans are explicit about taking part in the discussion on the demarcation problem. They introduce "a new demarcation criterion," which they call "the family resemblance

[12]The term *agnotology* is formulated by adding a negator prefix *a-* to the word *gnosis* which means "knowledge" (Proctor, 2008, p. 27). And, naturally, the suffix *-logy* refers to the study of something.

rule" (Collins & Evans, 2007, p. 128). As the name already gives away, the rule is inspired by Wittgenstein. It goes as follows: "Except where specific new findings demand a break, the intentional stance of a science must be to maintain continuity as far as possible with the existing science" (ibid). In a nutshell, the criterion says that one should trust experts only if they are willing to integrate their views with pre-existing science. A person or institution that violates this principle is most likely pseudoscientific (Id., pp. 128–131).

The rule focuses on the intentions of expert candidates. According to Collins and Evans, even the most reformist scientists should

> have the intention to change as little as possible consistent with their new theories and findings. They do not want to overthrow the scientific method, nor the greater body of scientific findings, nor the major social institutions of science, nor the existing data of science. (Collins & Evans, 2007, p. 130)

However, their new demarcation criterion does not demand dogmatic institutional or theoretical conservatism. It merely requires researchers to respect the previous success of science and the practices that enabled it.

Harry Collins and Robert Evans (2007, pp. 127–132) demonstrate the applicability of the family resemblance rule by using it to distinguish proper science from intelligent design, astrology, and unfounded LSD psychology. Lynch (2014, p. 100) has suggested that Collins and Evans' criterion can be used to separate artificially created scientific controversies from real ones.

What about the traditional assumptions? Collins and Evans use their rule to define science, and as a sole criterion it also fulfils the assumption of scarcity. It is somewhat difficult to say whether the criterion is meant to be universal, but it seems that it is. However, the family resemblance rule does not focus on end products or the formal features of science. It does not take any stance on methodology, theory formation, or anything parallel (Collins & Evans, 2007, pp. 128–131). The criterion concentrates on mental states – the researchers' intentions. Because the rule is a family resemblance rule, it does not appeal to necessary and sufficient conditions. Moreover, it is pointed out that the criterion is "vague" (ibid). Therefore, it does not draw clear-cut boundaries between science and pseudoscience.

Alvin Goldman has also offered instructions for identifying experts.[13] He lists five "sources of evidence" that a non-expert could use in judging whether someone posing as an expert is trustworthy (Goldman, 2006 [2001], p. 93):

(A) Arguments presented by the contending experts to support their own views and critique their rivals' views.
(B) Agreement from additional putative experts on one side or other of the subject in question.
(C) Appraisals by "meta-experts" of the experts' expertise (including appraisals reflected in formal credentials earned by the experts).
(D) Evidence of the experts' interests and biases vis-à-vis the question at issue.
(E) Evidence of the experts' past "track records."

These sources of evidence are supposed to help novices but not to be universal. Goldman is not convinced that his list is decisive under challenging cases (Goldman, 2006 [2001], p. 34). Furthermore, Goldman's list does not define science; it does not focus on end products or

[13]There have been other attempts as well, such as Scholz's (2018). He recommends understanding as an important symptom of expertise. However, Goldman's list is a better example of new demarcation endeavours because his aim is, explicitly, to offer usable criteria for who is trustworthy and who is not.

formal features; and the conditions are not necessary or sufficient but merely heuristic rules of thumb. The only traditional assumption that the sources of evidence could meet is the scarcity of criteria. A list of five characteristics is not long, but it is not a short one either. Indeed, Goldman's criteria do not seem to meet any of the traditional assumptions.

It is also noteworthy that Goldman's criteria are about general demarcation. Often the solutions to the demarcation problem do not distinguish between the two different projects, namely, general and specific demarcation. It is merely assumed that one set of standards can take care of everything. Paying particular attention to the needs of laypeople and recognising that their needs might not be exactly the same as those of specialists is something missing from the traditional solutions.

4 | THE DEMISE OF THE TRADITIONAL ASSUMPTIONS

We suspect that the traditional assumptions are to be blamed, at least in part, for the failure of the traditional solutions. Excluding Popperian falsificationism, none of the solutions have gained widespread support among philosophers, scientists, or laypersons (e.g., Alters, 1997; Pigliucci & Boudry, 2013b, p. 2). And although falsificationism is relatively popular outside philosophy, within philosophy it is all but universally condemned.[14] In this section, we will argue that all of the traditional assumptions are either untenable or unnecessary. We will show that giving up on them does not force us to give up on the demarcation problem. However, it is important to note that we are not saying the traditional assumptions are false. Instead, we are merely pointing out that they are unjustified and not needed for demarcation. If we presuppose that our results should be of a specific kind, this will cause a needless bias on our approach. This is what we are trying to avoid.

Let us start with the first assumption, defining only science. It is generally accepted that pseudoscience is not a mere contrary of science. "Non-scientific" is a considerably broader term than "pseudoscientific." Several activities are neither scientific nor pseudoscientific, such as fine arts, religion, politics, and soccer (see Mahner, 2013, p. 31; Hansson, 2013, pp. 62–65; Hansson, 2016, pp. 1–2). One needs some way to distinguish pseudoscience from these other ventures. The conventional route has been to define pseudoscience as non-science claiming to be science. However, Sven Ove Hansson has suggested a broader definition of pseudoscientific statements that consists of three criteria. First, they have to fall within the domain of science. Second, they must be utterly unreliable and therefore untrustworthy. Third, the proponents of pseudoscience claim that the statements are the most reliable knowledge on the subject matter (Hansson, 2013, pp. 70–71).

Now, one should note two things about Hansson's definition: (1) It is rather apparent that one does not need to define science to determine that some pseudoscientific claim is unreliable. Generally speaking, the determination of reliability does not start with a definition of science, and (2) defining the domain of science is not the same as defining science. Otherwise, everything within the domain of science would be science; therefore, even pseudosciences would count as sciences. Thus, someone who accepts Hansson's broad definition, or something similar to it,[15] does not have to define pseudoscience as something masquerading as science.

Why, then, should we accept Hansson's view? According to him, the conventional definition does not cover some clear cases of pseudoscience. Hansson gives an example of a homoeopath who does not claim to be doing science but still insists on possessing more reliable knowledge than science can offer (Hansson, 2013, pp. 69). It would be very odd indeed if such a manoeuvre would suffice to turn homoeopathy into something other than pseudoscience. After all, its

[14]For example, Kuhn (1964 [1962], pp. 77–80, 144–147; Kuhn, 1974 [1965]), Lakatos (1974), pp. 246–250; Lakatos, 1978a [1970]), Putnam (1975), Feyerabend (1993 [1975], pp. 50–51, 145, 155), Ruse (1977), pp. 645, 647–648, 650–651), Kitcher (1984 [1982], pp. 42–49), Hacking (1983), pp. 114–115), Laudan (1983), p. 121), Howson and Urbach (2006 [1989], pp. 104–105, 131–132), Agassi (1991), Hansson (2006), and Mahner (2007), pp. 518–519) – just to name a few – have criticised Popper's falsificationism.

[15]For example, J. W. Grove (1985, 219) has defended a broad definition of pseudoscience akin to Hanssons'.

pseudotheory remains intact. Thus, to conclude, the study of pseudoscience does not have to start with a definition of science.

Another reason for not concentrating on only science is connected to three other traditional assumptions: necessary and sufficient conditions, universality, and scarcity of criteria. Recall Boudry's (2011, p. 246) view that "science" and "pseudoscience" should be seen as family resemblance concepts. If this approach has any merit, then there is no single monolith called *science* but a plurality of *sciences* connected by overlapping similarities. Boudry also proposes that there are *paradigmatic* instances of sciences and pseudosciences. Paradigmatic pseudosciences can also be very different from one another. Think of, say, intelligent design, Holocaust denial, ancient astronaut hypothesis, homoeopathy, the anti-vaccine movement, astrology, or climate change scepticism. Because there are different forms of pseudoscience, one cannot rule out the possibility that different criteria are needed to distinguish them from science.

To show that Boudry is on the right track, one must argue that pseudosciences form a heterogeneous category. Fortunately, this is not a difficult task because pseudosciences can be pseudosciences in different ways. For example, Hansson (2017) distinguishes science denialism such as climate change scepticism and the anti-vaccination movement from pseudotheories such as intelligent design and nationalistic pseudohistory. Besides this distinction, pseudosciences can also be dissimilar in other ways. For instance, Holocaust denialists usually do not postulate any previously unknown technologies or causal mechanisms, unlike the proponents of the ancient astronaut hypothesis. Postulating such outlandish causal factors already suffices to make the latter a pseudoscience, whereas the former requires other criteria. The very fact that different standards can be used to identify different pseudosciences indicates that pseudosciences can differ significantly from each other.

There are also other problems with the assumption of universality. A criterion can be helpful in making distinctions in some contexts, even if it does not work everywhere. One could, as an example, use some standards for evaluating the quality of historical research and others for medical science. In other words, specific demarcation criteria can have a limited scope (Mahner, 2013, p. 36). Furthermore, science diverges from pseudoscience on several levels because different factors influence the epistemic quality of research. For example, pseudoscientists' argumentation could be flawed; their data collecting might be inadequate; their research communities might be biased; and relevant alternatives to their theories might not have been properly examined.[16] Problems like these have some common denominators, but it is very difficult to say what, if anything, conjoins them all.

However, we are not claiming that all generality should be excluded from demarcation. Indeed, what we call *general demarcation* requires rules of thumb applicable in a wide range of cases. But generality does not require universality. For instance, parapsychology and historicity of miracles appeal to supernatural factors, whereas climate denialism does not. Therefore, supernatural factors can function as general indicators for something being pseudoscientific even if they are not universal conditions.

One of the problems with the traditional solutions is that they employ a top-down strategy, commencing from general principles that are applied to singular instances. Because of this, as we already mentioned in section 2, the traditional solutions start by assuming that the solution to the demarcation problem must be of a certain kind. Instead of assuming the conclusion, we recommend an approach that does not require any of the traditional assumptions.

We suggest a bottom-up strategy in which more general principles are distilled from singular instances of success and failure.[17] The procedure could, for instance, go as follows: First, in a

[16]Even scientists occasionally are guilty of such errors. Naturally, in these cases, the research would not count as good science. Still, bad science in which, for example, data collection is flawed, is not automatically pseudoscience. Scientists can make mistakes without becoming pseudoscientists. It is therefore not justified to condemn something as pseudoscience based on a single or a few errors. This also makes it more difficult to say when the threshold of pseudoscience has been crossed.

[17]The idea for the bottom-up strategy comes from Uskali Mäki (2005). He suggests a similar procedure for determining the global features of scientific realism. The difference between our approach to demarcation and Mäki's handling of realism is that we are not striving for global or universal features.

Mahnerian manner, one begins by listing some indicators of science and pseudoscience. However, contra Mahner, the second step does not consist in weighting the indicators. Instead, one organises the indicators under different categories. The idea is to identify local cases of success or failure and then see if they share any common features. This way, one can produce empirically informed general tools for evaluating unclear demarcation cases. This approach is very similar to the method of rational reconstruction[18] proposed by Gerhard Schurz (2014, pp. 20–21).[19] One possible difference between our bottom-up strategy and Schurz's rational reconstruction is that we might be more open to the idea that there is a wider variety of indicators of science and pseudoscience and that the indicators are not necessary for all sciences or pseudosciences. Schurz (id., chap. 2) takes sciences to be methodologically unified, whereas we remain agnostic about the matter.

Next, let us take a closer look at necessary and sufficient conditions. According to Laudan, necessary conditions are needed for identifying pseudosciences and sufficient conditions for determining proper sciences. Moreover, demarcation is based on "individually necessary and jointly sufficient conditions" (Laudan, 1983, p. 118). Sebastian Lutz has convincingly argued that this is not so even if one is attracted to necessary and sufficient conditions. He pointed out that in order to say that *A* is scientific whereas *B* is not, all that is needed is one sufficient condition that *A* fulfils and one distinct necessary condition that *B* does not fulfil (Lutz, 2011, p. 126; Lutz, 2012, p. 358). Even though Lutz is clearly right, we are willing to take one step further: criteria of either type would already suffice for making demarcations and, perhaps, neither are needed.

If we have necessary conditions of science – without sufficient ones – we can judge as untrustworthy those epistemic projects that claim to be scientific but do not meet the conditions. Policymakers and laypeople can ignore what the supporters of such research programmes are claiming. For instance, Collins and Evans (2007, p. 128) have suggested that intended continuity with the existing science is a necessary condition of science. If this proposal is accepted, then gravitational waves, parapsychology, and astrology can already be excluded from the sphere of science.

The same holds if we are endowed with sufficient conditions of pseudoscience: something that fulfils them does not deserve our trust. Similarly, merely sufficient conditions of science would also be valuable. Demarcation is often needed precisely when practitioners of a proper science (e.g., biology, medical science, history) are forced to fight off a pseudoscientific group (e.g., intelligent design, the anti-vaccine movement, Holocaust denial). If the proper sciences meet the sufficient conditions of science, whereas the pseudosciences do not, it seems obvious which group should be trusted. Thus, merely sufficient conditions of science would already be very welcome. In addition, a criterion that is neither necessary nor sufficient can be helpful. As long as satisfying it increases or decreases the scientific credibility of research, the criterion can serve as an instrument for demarcation. After all, as Perakh and Young (2004), p. 185) and Kitcher (1984 [1982] p. 48) have suggested, the difference between science and pseudoscience is gradual.

Someone might still object: Perhaps demarcation is a matter of degree, but there is nevertheless some threshold that must be exceeded. Suppose that there is a plurality of sciences and pseudosciences. Furthermore, the membership of each category is determined by proximity to prototypical members, say, the paradigmatic instances of sciences and pseudosciences that Boudry (2011, p. 246) proposed. Even in this case, there is a threshold of satisfactory similarity that must be achieved. The threshold can be seen as a sufficient criterion of science or pseudoscience. Furthermore, it is always possible to list all epistemically relevant properties of

[18]Schurz' method of rational reconstruction should not be confused with Lakatos' (1978b [1970]) rational reconstruction or its more positivistic predecessors.

[19]We thank an anonymous reviewer for bringing this to our attention.

prototypical sciences or pseudosciences. And when something has these attributes, it fulfils the sufficient conditions for being science or pseudoscience. Hence, sufficient conditions would exist, albeit they would be disjunctive because there are different types of sciences and pseudosciences. Thus, in order to be a member of science A, it is sufficient to have properties a_1, a_2, …, a_n; and in order to be a member of science B, it is sufficient to have properties b_1, b_2, …, b_n; or in order to be a pseudoscience of type C, it is sufficient to have properties c_1, c_2, …, c_n, and so on.

At first blush, this argument might seem appealing. Unfortunately, however, it is not sound because prototype categories do not usually permit monotonic reasoning.[20] One might be inclined to try to evade non-monotonicity by invoking ceteris paribus clauses. This, however, is problematic because it would require ruling out infinite lists of conditions, and we usually cannot specify in advance everything that belongs on the list (Brandom, 2001 [2000], p. 88). Therefore, something could satisfy all the properties of some paradigmatic science and yet have an additional property that renders it pseudoscientific. And it is possible that we cannot determine in advance all the properties that could turn potential sciences into pseudosciences.[21]

For example, assume that someone would take a prototypic physical theory and explain its anomalies with actions of supernatural agents. This individual might even try to support their claim by means generally accepted within the community of physicists working on the theory. Nevertheless, many would be inclined to think that they are already guilty of practising pseudoscience despite their research otherwise fulfilling most, or even all, of the requirements of bona fide physics.[22]

Non-monotonicity might raise worry: if the concepts of science and pseudoscience are prototype categories that do not necessarily permit monotonic reasoning of their members, then does not that make all demarcation endeavours futile? No, it only shows that it is impossible to generate formally complete definitions of science and pseudoscience. But we can still give good arguments on what kind of ventures belong to these denominations. After all, it would not be sensible to claim that, for instance, empirical science is worthless because the reasoning executed in it is non-monotonic. Moreover, non-monotonic inferences are unproblematic even though they are fallible. Indeed, we make such inferences on a daily basis.

Although necessary or sufficient conditions are not inevitable for science, as our arguments show, it is still possible that such criteria could exist. For example, it is plausible that there are at least some necessary conditions of science, such as the intersubjective evaluability of theories (Hietanen et al., 2020, pp. 533–534, 539, 541–543; Turunen et al., 2022) or the need to support scientific conclusions with reasons. Similarly, some sort of deceptive quality, such as pretending to be trustworthy knowledge, could be necessary for pseudoscience (see, e.g., Gardner, 1957, chap. 1; Mahner, 2007, p. 547–548; Hansson, 2013, pp. 70–71). Also, there seem to be sensible thresholds for science and pseudoscience. These thresholds can be taken as sufficient conditions,

[20]Reasoning is non-monotonic when the fact that the inference $p \rightarrow q$ is a good one does not guarantee the goodness of the inference $p \wedge r \rightarrow q$. To get a better understanding of non-monotonicity, consider the following examples from Robert Brandom (Brandom, 2001 [2000], pp. 87–88, negation sign changed):

'1. If I strike this dry, well-made match, then it will light ($p \rightarrow q$).
2. If p and the match is in a very strong electromagnetic field, then it will not light ($p \wedge r \rightarrow q$).
3. If p and r and the match is in a Faraday cage, then it will light ($p \wedge r \wedge s \rightarrow q$).
4. If p and r and s and the room is evacuated of oxygen, then it will not light ($p \wedge r \wedge s \wedge t \rightarrow q$)'.

Reasoning in the formal sciences, like mathematics and logic, is generally monotonic, but as a rule inferences in the empirical sciences are not – with the occasional exception of physics (Brandom, 2001 [2000], pp. 87–88).

[21]We are grateful to Risto Tiihonen for this point (personal correspondence).

[22]We are not claiming that ontological naturalism is a necessary precondition of science. For more on this, see Fishman (2009), Boudry (2011), pp. 15–18, 235–238), and Fishman and Boudry (2013). Still, evoking supernatural explanations can nevertheless serve as a strong indicator for something being pseudoscience.

but the conditions are open-ended and fallible. However, if it turns out that there are no such conditions to be found, then it will not make demarcation endeavours impossible. One can do everything that is needed in specific demarcation cases without fulfilling Laudan's strict conditions. More extensive principles can be distilled from these cases, and they in turn can be used as tools in general demarcation.

Next, let us focus on formal features and end products of science. Usually, philosophers who have focused on formal features in tackling demarcation have used monotonic formal logic. However, because reasoning in empirical research is customarily non-monotonic, this approach is not the best one to take (although non-monotonic logics can still be applied for the task). In addition, Boudry (2013, pp. 91–92) has argued that the propositional content and logical structure of pseudoscientific theories cannot be comprehensively evaluated without considering the belief system in which they are embedded. This is because "there is no way of telling where the proper theory ends and where the obfuscations by its defenders begin" (id., p. 91). One is also forced to look at the psychological defence strategies that pseudoscientists use to salvage their doctrines from refutations. Kitcher (1993, p. 196) tends to agree on this with Boudry. An even stronger reason for not focusing on formal features comes from social epistemology. It has been convincingly argued that to portray science realistically one has to consider its communal practices (Longino, 1990, pp. 66–81; Longino, 2002a, p. 10, 38; Soler et al., 2014, p. 19). This is why logical form and end products of science are no longer considered to determine the boundaries of science on their own, even if pseudosciences are recognisable by their use of fallacious argumentation (see Cook et al., 2018).

5 | CONCLUSIONS: DEMARCATION IS DEAD, LONG LIVE DEMARCATION!

In this article, we have examined the shift from traditional to novel solutions through consideration of the demarcation problem. We explicated the main differences between these approaches in terms of six postulates we call the traditional assumptions. Because the novel, more naturalistic takes on demarcation usually reject all or at least some of the assumptions, these approaches have formulated normative principles that correspond better to actual science or pseudoscience.

The traditional assumptions are unnecessary if the novel solutions can successfully be used in specific or general demarcation. Moreover, in the previous section, we gave additional arguments for abandoning them and showed that rejecting the assumptions does not lead to the demise of the demarcation problem. Because the traditional assumptions are disposable, and Laudan's critique of demarcation endeavours was built on them, we need not worry about his conclusions. To be fair, Laudan was right in the sense that the passing of the old ways is long overdue. But despite this, the novel approaches show great promise. In any case, the rumours of the death of the demarcation problem have been greatly exaggerated.

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