The Multicriterial Approach to the Problem of Demarcation

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Abstract

The problem of demarcating science from nonscience remains unsolved. This article executes an analytical process of elimination of different demarcation proposals put forward since the professionalization of the philosophy of science, explaining why each of those proposals is unsatisfactory or incomplete. Then, it elaborates on how to execute an alternative multicriterial scientific demarcation project put forward by Mahner (2007, 521–522; 2013, 29–43). This project allows for the demarcation not only of science from non-science and from pseudoscience, but also of different types of sciences and of scientific fields (e.g., formal sciences, natural sciences, social sciences) from each other. This article also offers arguments in favor of accepting two types of scientific demarcations, namely epistemic-warrant scientific demarcations and territorial scientific demarcations, and argues in favor of accepting a territorially broad scientific demarcation.

Keywords Scientific demarcation · Non-science · Pseudoscience · Broad science

1 Introduction

This work takes the main problems of scientific demarcation (SD) to be those of establishing criteria based on reflection on science and its history to: (1) most generally, intensionally demarcate science from non-science (i.e., the problem of defining “science”); and: (2) in particular, to intensionally demarcate science from pseudoscience.¹ As these remain unsolved so far, this article outlines a project to solve them.

To sharpen and improve our understanding of the phenomenon of science would be beneficial for human well-being. SD criteria would better inform reliable philosophical and scientific decisions as well as judgments pertaining to a wide array of everyday human

¹ In line with common philosophical thought (Hansson 2017), this article defines a “pseudoscience” as a non-scientific doctrine or field that is presented by its theoreticians or practitioners as if it were scientific or a science, or that is easily taken to be so by a given epistemic community. My intended intensional demarcation of pseudoscientificity is not exhausted by this definition, in part because it depends on the intensional demarcation of scientificity.

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activities, including educational, financial, medical, legal, regulatory, and political endeavors, so the matter is of profound social relevance.

Section 2.1 through Sect. 2.3 of this work execute an analytical process of elimination by considering the historical development of the SD problems, evaluating different SD proposals put forward since the professionalization of the philosophy of science, and explaining why each of those proposals is unsatisfactory or incomplete. Some of the objections that I will make are well-known, but a review of this literature is essential to address the problems at hand. The main aim of this article is to present a comprehensive SD project in which, among other contributions, the proposals incipiently put forward by Mahner (2013) and Hansson (2013) are theoretically elaborated on, expanded, developed, and adjusted up to the point at which they are ready for execution. Section 2.4 takes up, elucidates, and expands the alternative multicriterial SD proposal put forward by Mahner (2007, 521–522; 2013, 29–43). Section 3 criticizes Hansson's work, introduces the concept of epistemic-warrant scientific demarcations (EWSDs), refutes the idea that EWSDs and diagnostics of pseudoscientificity are necessarily normative, indicates a territorial aspect of EWSDs, argues in favor of accepting two types of SDs (namely, EWSDs and territorial SDs), presents arguments in favor of a territorially broad SD, advances a definition of science in the broad sense (SBS), and indicates how properties of scientificity of different levels are articulated within SBS. Section 4 indicates why common sense (CS) is not scientific, and explains how SBS relates to CS and how epistemic tasks are distributed between SBS and CS. From Sect. 2.4 to Sect. 5 it is indicated why Laudan's (1983) diagnosis of the demise of the SD problem was premature, why Laudan's proposal is a proposal for an epistemic-warrant and territorially broad demarcation, why the Mahnerian project seems to be a promising endeavor for seeking after an epistemic SD criterion,² how philosophers can defend an epistemic SD criterion and derived normative prescriptions, and how to demarcate, within that project, not only science (in the broad sense) from non-science and from pseudoscience, but also different types of sciences and of scientific fields (e.g., formal sciences, natural sciences, social sciences) from each other. Section 5 also indicates the width of practical applicability that a SD is achieved through the completion of this project.

2 Direct Attempts at SD Since the Twentieth Century

2.1 Monocriterial Proposals

Although attempts to define science date from at least Aristotle's *Posterior Analytics* (Laudan 1983, 122),³ the SD problem entered the academic field of the philosophy of science in the twentieth century. The term "criterion of demarcation" ("*Abgrenzungskriterium*")

² That is, a criterion that determines a demarcation between science and non-science with respect to what is "special about the knowledge claims and the modes of inquiry of the sciences", i.e., about exclusive or typical properties that these claims and methods enjoy "in a way that exhibits an epistemic guarantee or evidential basis that is [epistemically] safer for science than for non-science" (Laudan 1983, 118).

³ Aristotle's SD is unsatisfactory. Firstly because of its ambiguity in his demands for scientific knowledge to be about what is universally or just "for the most part" (*hōs epi to polu*) constant (*Nicomachean Ethics*, 1139a5–15; *Metaphysics*, VI.2, 1027a20; Parry 2014). Secondly, because of the need to expand on his work, which for example did not encourage experimentation (Andersen and Hepburn 2016).

in the original German) was coined by Popper in texts of *The Two Fundamental Problems of the Theory of Knowledge* (2010) that circulated in 1930–1933.

Popper's problem of scientific demarcation was that of "finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other" (1959/2002, 11). His solution was the in-principle falsifiability of sets of propositions (the theories) of empirical science, i.e., their capability "of conflicting with possible, or conceivable observations" (1962, 39), plus avoidance of abusing ad hoc definitions or ad hoc hypotheses to evade falsification (which is a conventionalist stratagem according to falsificationism) (1962, 37). He conceived this as a necessary and sufficient composite criterion to indicate scientificity.

For our purposes, Popper's proposal suffers from the following problems:

- a. every falsified theory would be considered scientific (but to solve this one it would suffice to require that a theory had been corroborated numerous times in different situations);
- b. the pragmatism problem that any theory falsifiable in principle but forever unfalsifiable in practice should be considered empirically scientific;
- c. in testing a theory, falsifiable assumptions that are not parts of that theory are also simultaneously tested. This is called contrastive holism. Because of this, what is falsified in the testing of a theory may be not a part of that theory;
- d. the problem, indicated by Bunge (1983, 201) and Hansson (2013, 74), that this criterion excludes those parts of scientific activity that are not concerned with the testing of theoretical constructs, such as some fact-finding or explorative research or experimentation, and "[s]tudies (...) driven by open-ended questions" (Hansson 2013, 74), because it cannot be applied to them; and
- e. Popperian falsificationism also admits common sense as scientific (see Sect. 4).

To sum up, falsifiability, as a criterion for any demarcation of the kind intended here, can be excessively weak or excessively strong, and is neither a sufficient nor a necessary condition. Nonetheless, falsifiability might be fruitfully incorporated into a list of properties of scientificity used to demarcate science as will be indicated in Sect. 2.4.

Lakatos (1970) reformulated the problem of SD in terms of the theoretical progressiveness of scientific research programs. These are sequences of successive theories that share a common hard core, "each theory having at least as much content as the unrefuted content of its predecessor" (Lakatos 1970, 118). According to Lakatos, to be entirely progressive a program must be both theoretically and empirically progressive, and if it is not entirely progressive, then it is degenerating. The theoretical progressivity of a scientific research program comes entirely from the occurrence of a shift (within the program) by which each new theory in the program has an excess of empirical content over its predecessor theory, including making novel predictions. In this case, a theoretically progressive "problem-shift"—or "theoryshift"—has occurred. Empirical progressivity comes entirely from the fact that at least some of these novel predictions are corroborated. In this author's proposal, good science is that of entirely progressive research programs, while bad science is constituted by those degenerating programs for which all "the novel predictions that they deliver turn out to be" falsified (Musgrave and Pigden 2016). Regarding the demarcation of science (good and bad) from pseudoscience, according to Lakatos, "[w]e 'accept' problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we 'reject' them as 'pseudoscientific'" (Lakatos 1970, 118). So, Lakatos establishes two SDs

(on the same continuum): one normative, between progressive (good) and degenerating (bad) science; and another one, between “scientific” and “pseudoscientific” problemshifts.

As indicated by Newton-Smith (1981), Lakatos’s proposal is theoretically insufficient because it does not include some non-empirical aspects of science, namely conceptual aspects, which include questions of intelligibility, and of meaning. Newton-Smith correctly pointed out that “any model of science must leave room for the differential assessment of theories in terms of their power to avoid conceptual difficulties and not just in terms of their power to predict novel facts and explain known facts” (Newton-Smith 1981, 89, 208–236⁴). Lakatos’s proposal is also pragmatically unsatisfactory due to the exclusively retrospective nature of its evaluations, which makes it impotent to offer “instantaneous” advice to scientists and philosophers about decisions to take and assessments to make in the present. So Lakatos’s is not a satisfactory proposal to achieve our demarcatory goals, but the properties mentioned in it might be fruitfully incorporated into our list of properties that are typical of science (cf. Sect. 2.4).

Bunge (1983) rejected seven SD proposals: consensualism, the empirical content doctrine, the *success* view, formalism, falsificationism, methodism, and sociologism. Consensualism—the idea that “science is uncontroversial or at least aims at ‘a consensus of rational opinion over the widest possible field’ (Ziman 1979, 3)” —is inadequate because (a) “every field of active basic research teems with controversy, and (b) consensus is at most an uncertain indicator of truth, not a goal of research. What is true is that science has means for settling controversies in the long run”. The empirical content doctrine, which claims that there is no place in science for speculation, was refuted by the emergence of theoretical scientific disciplines. The *success* view—which “claims that for science only success counts”—misinterprets the goals of science, which are epistemic reliability, empirical adequacy, explanation, successful prediction, truth, depth, systemicity, and the like. The formalist view—which holds that a body of beliefs “is scientific only when it has been thoroughly mathematized”—is too wide in some cases (e.g., some formal economics) and too narrow in others (e.g., explorative science, experimental science, young science). We already (in this sect.) discussed Bunge’s objections to falsificationism. Methodism—the idea “that the sole requisite for science is adopting the scientific method”—is problematic because “if every application of the scientific method were indeed a piece of scientific research, then testing the mental ability of atoms (...) would pass for scientific research provided certain precautions were observed”. Bunge correctly discards sociologism—the proposal that science is what scientists do—, because “occasionally scientists indulge in nonscientific activities (...), or even straight pseudoscience” (1983, 200–203). I agree with all of these Bungen assessments and reasons.

In *Systematicity*, Hoyningen-Huene proposed a new monocriterial SD in which “[s]cientific knowledge differs from other kinds of knowledge, in particular from everyday knowledge, primarily by being more systematic (...) about the same subject matter[s]” (2013, 14, 23) (his analysis also comprehends science as an activity, including methodological and sociological aspects of the way in which this knowledge is produced). By his own admission, Hoyningen-Huene defines systematicity in general and in the abstract in a tenuous and vague way, as a property of things that are “not purely random or accidental, (...) not chaotic, not arbitrary, not anyhow made or risen, not completely unmethodical, not completely unplanned, nor completely unordered. Rather, it embodies some kind of order”

⁴ Newton-Smith referred here to theory choice, but the same can be applied to the demarcation between science and non-science.

(Hoyningen-Huene 2013, 26). He correctly indicates that the possession of systematicity in general and in the abstract is a necessary but not sufficient condition for scientificity. He argues that a necessary and sufficient conjunction of conditions for knowledge or a theory to be scientific, or for a discipline to be a science, at time t , is the following: it possesses a sufficient amount of systematicity concretized in certain specific (scientific) ways indicated by him (2013), and, over a given period of time before t , that systematicity did not increase substantially less than the scientific systematicity of another contemporaneous discipline dealing with roughly the same subject matter (2013, 2018). Hoyningen-Huene affirms that science is demarcable because it is the sufficiently (and contextually the most) scientifically systematic knowledge/activity. Of course, this would be a very poor statement if it were not accompanied by a specification of the criteria demarcating scientific systematicity from other types of systematicity. And although Hoyningen-Huene takes important steps in that direction with his concretizations, his conception of scientific systematicity is used to refer to a set of more concrete properties that make up science but that we could and should identify more clearly (although they do confer to science a certain greater systematicity compared to other types of knowledge and of activities). He adopts an abstract concept of systematicity so vague that allows him to classify and group as systematic all or most of the properties that have traditionally been considered to be key characteristics of science, which compels him to trivially yield the conclusion that the essence of science is scientific systematicity.

2.2 The Multicriterial Proposals of Hempel and Kuhn

Hempel advanced a proposal for multicriterial SD, arguing that philosophers should analyze differences in degree between theoretical systems in aspects such as (a) “the clarity and precision” of their formulation; (b) their “explanatory and predictive, power (...) in regard to observable phenomena”; (c) “the[ir] formal simplicity”; and (d) “the extent to which the theories have been confirmed by experiential evidence” (Hempel 1951, 74; Ayer 1959, 129). In order to achieve SD, we should take into account all of these aspects by incorporating corresponding properties into a list of properties that are typical of science (cf. Sect. 2.4).

Kuhn proposed an approximate SD based on five “standard” (1977, 322) scientific desiderata or epistemic values from a set of values which he deemed “canons that make science scientific” (1977, 324). Each of these “values” can be descriptively thought of as being one or more properties, or “criteria for evaluating scientific belief” (1993, 338), and used in a SD criterion. Kuhn’s selected group of “values” is:

1. theoretical accuracy of fit to nature—i.e., empirical adequacy—within an intended domain of application (“consequences deducible from a theory should be in demonstrable agreement with the results of existing experiments and observations” in its intended domain, and that agreement should be “not only quantitative (...) but qualitative as well”). He indicated that “predictive and explanatory powers” depend on this kind of accuracy;
2. consistency—internal and external, the latter being compatible with other theories contemporaneously accepted;
3. broad scope;
4. simplicity and the capability to bring “order to phenomena that (...) would [otherwise] be individually isolated and, as a set, confused”; and

5. fruitfulness of research findings of “new phenomena or previously unnoted relationships among those already known” (1977, 321–323).

He mentioned a sixth epistemic value: the capability to be used to explain (Kuhn 1977, 336).

All of these properties are worth incorporating into our list of scientificity properties (cf. Sect. 2.4). But Kuhn indicated (1977) that his list, by itself, is incomplete, and insufficient as a guide for good-theory choice because of at least six other problems, namely:

1. “imprecision” of the “values”, and the resulting lack of consensus among different legitimate assessments about the degree of presence of a given property in a given theory;
2. conflict between “values” in the absence of a consensus about how to “weigh” the respective degrees of importance of conflicting properties;
3. some of the “values” might select theories that we intend to discard;
4. we would need to specify what areas of a theory are more important for satisfaction of a “value” if different areas of the theory satisfy that “value” to different degrees (alternatively, we could aim to have a process to calculate the total degree to which a given theory satisfies a given “value”);
5. we would need to specify in what sense we mean the name of a “value”, but this can be solved by using different words, phrases, or indexes for different senses; and
6. there can be legitimate disagreement over whether the possession of some of these properties can legitimately be deemed unnecessary for some fields to be considered scientific (Kuhn 1977, 324).

These six obstacles for the use of these “values” in standardized decision procedures for theory choice specification are what compelled Kuhn to call these “values”, i.e., “[c]riteria that influence decisions without specifying what those decisions must be” (1977, 330).

Some of these six problems appear to be obstacles for the use of these “values” as criteria for SD, as they would be insufficient not only to determine theory choice, but also to specify scientificity and non-scientificity. Nonetheless, multicriteriality is not hopeless: further investigation, substantially extending Kuhn’s list, and incorporating other provisions will help us overcome these obstacles, a possibility Kuhn admitted (1977, 326).

We have reviewed the major attempts to demarcate science since 1930, and found that none of them succeeds. Popper’s does not work because of the five problems mentioned when discussing his proposal. Lakatos’s is unsatisfactory because it ignores conceptual aspects that are necessary to distinguish science from gibberish that predicts successfully. Hoyningen-Huene’s account is incomplete because of its unspecificity. Hempel’s proposal was just an outline of a project. And Kuhn’s suggestion is only incipient and afflicted by the problems just mentioned.

2.3 From Multicriteriality to Anarchy and Capitulation

The lack of a solution to the SD problems is partly because the proposals offered could not account in full and exclusively for the extension of the pre-analytical notion of science applied by the scientific and philosophical international communities: it was always possible to offer examples of theories, disciplines, or practices that were not selected as scientific by the criterion or set of criteria proposed, although it was intended that they were (on

the basis that they were already pre-analytically deemed scientific); or that were selected by the criterion or set of criteria suggested, but were actually intended to be excluded (on the basis that they were pre-analytically considered to be non-scientific).

Feyerabend proposed a plurimethodic (“methodological anarchist”) approach to science. In *Against Method* he argued: “given any rule, however ‘fundamental’ or ‘rational’, there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite” (1993, 14). If that is the case, that does not imply that the violated rule is not, generally speaking, reliable, or at least sufficiently justified, and therefore advisable, so his is not a compelling argument for avoiding the inclusion of properties related to method in a SD, leaving the way open for the project presented in Sect. 2.4. The successful demarcation that will be born out of this project, in turn, will show that not just anything goes.

As a result of the failure to find a solution, Laudan declared that the problem of SD was in itself spurious, and rejected the idea that any epistemic criterion of SD could succeed (1983, 124). In spite of its pervasive influence, all his diagnosis did was showing that it is difficult to demarcate science from non-science by using a single simultaneously necessary and sufficient condition, as, taken individually, “none of the criteria which have been offered thus far promises to explicate the distinction” (Laudan 1983, 124), which is also shown here. We will get back to Laudan’s dismissal in Sect. 5.

2.4 Rehabilitating the Multicriterial Approach

The failure of monocriterial SDs led other philosophers of science to propose multicriteriality, notably Bunge (1982, 372; 1983; 1984; 1991, 245–247; 2003, 259–262). Bunge’s list of criteria of scientificity—which will be presented and partially used in a future work—was further developed by Mahner (2007, 2013).

Thagard (1988) advanced a list of proposed properties of science indicating that they belonged to the “conceptual profile of science”. He contended that “we should aim for a list of features that are typical of science” (1988, 159). In Kuhn’s words, “[d]isciplinary characteristics permit its identification as (...) science”, locating every science “close to other scientific disciplines and at a distance from disciplines other than science. That position, in turn, is a necessary property of all referents of the (...) term ‘science’” (2000, 214). This would even allow for science to be deemed a natural kind, while accepting a “moderate essentialism” (Mahner 2007, 521–522; 2013, 39–40).

Consistently with the above, Mahner (2007, 521–522; 2013, 40, 42) proposed, to achieve SD, the project that is taken up in this work. It consists of: (a) the composition of a broad “list” (set) of accepted characteristics, conditions, or properties of science, or indicators of scientificity (most of them, by themselves, unnecessary and insufficient)⁵ which

⁵ Hansson (2017) mentions 15 proposed lists of SD criteria that can be considered. Fasce (2017) has called attention to the scarce amount of coincidence among these lists, worrying that it might be indicative of a lack of consensus among their authors. Nonetheless, there is also an almost complete lack of contradiction among them, which is due to the existence of a tacit generalized consensus in favor of the vast majority of the properties in these lists. The lack of coincidence is mostly due to variations in subject matter, or in approach, or in particular interest, or in degree of philosophical sophistication, or to differences in the degrees of importance adjudicated to different properties in incomplete lists, as none of these short lists is presented as exhaustive, presumably only those properties considered to be the most important being included in them.

might be collectively used to establish a demarcation between those theories, cognitive fields (CFs),⁶ practices, etc. which are scientific and those which are not; and (b) calibrating the list of properties, and, in particular, ascertaining a boundary or “cut”: the minimum score (measured in total aggregated amount of properties featured) that a theory, CF, practice, etc. would have to feature in order for it to be legitimately deemed scientific.⁷ All this would be done by performing comparative analyses of a wide range of several clear cases of particular sciences and of non-sciences of different subtypes (natural sciences, social sciences, pseudosciences, non-pseudoscientific non-sciences, etc.).

This proposal requires to be further elucidated and expanded with the following considerations. We need the “cut” value if we want to be able to say that the assessed unit is either scientific or non-scientific in the simplifying manner required by classical logic (because without the “cut”, in many cases we would have to say, for example, that the assessed unit is 60% scientific and 40% non-scientific, and therefore both scientific and non-scientific at the same time and in the same sense, which would be more accurate but also more difficult to manage). The identification of the clear cases of particular sciences and of non-sciences will be done according to the assessments made by the current international philosophical and scientific consensus. Admittedly, philosophical consensuses are never unanimous, but the consensus about the extension of science is sufficiently extended and territorially-covering to serve as an acceptably firm base for intensional SD. In this way we are going to be basing our intensional demarcation on a consensus about extension. A consensus should be deemed reliable to provide a base for philosophy only to the extent that that consensus can be shown to be the best available way to track (or the best available indicator of) epistemic reliability or at least success of outcomes, which is our ultimate grounding.⁸ This course of action is consistent with the naturalistic contention that a criterion of scientificity “should be based on reference to criteria that are empirically observed to regulate the practices of science” (Ladyman et al. 2007, 33). The “cut” value for each type of unit would be established to be the scientificity value of the lowest-valued clear case of a scientific unit of that type, provided that there were no clear case of a non-scientific unit of that type that had an equal or greater value. For example, the “cut” value for CFs would be established to be the scientificity value of the lowest-valued clear case of a scientific CF, provided that there were no clear case of a non-scientific field that had an equal or greater value. The properties can be classified under different categories, such as practices, aims, methods, logical aspects, conceptual aspects, products, and socio-institutional certification (cf. Erduran and Dagher 2014; Irzik and Nola 2014).

In this proposal, there are many different ways or manners in which a theory, CF, practice, attitude, etc. can be scientific or non-scientific, by possessing or failing to possess different subsets of (applicable) properties in our whole demarcatory set of properties of scientificity, and reaching or failing to reach the “cut” value (Mahner 2007, 522; 2013, 40). Sciences of different subtypes are widely different to each other in several respects, and

⁶ A “CF” is defined here, partially following Bunge (1984, 36), as “a sector of (...) activity [purportedly] aiming at gaining (...) knowledge”.

⁷ We might be able to attain our intended SD without having to establish the “weight” (degree of importance) of each property relative to each other’s, and without considering non-binary degrees of possession of each property.

⁸ Epistemic reliability is evidenced by success of outcomes, such as empirical adequacy, successful predictions, and functioning technological applications. All other properties of science (for example, properties related to method) are to work towards epistemic reliability, and should be deemed properties of scientificity, or not, with respect to the achievement of epistemic reliability.

there is important variation among different stages of a science's development. That is why some properties of scientificity might be not equally relevant to, or might have not the same "weight" in, all sciences or across all stages of a science's development. Consequently, our demarcatory set of properties of scientificity could include properties that should be differentially computed (as indicated by the clear cases of sciences to be used as standards) for CFs of different subtypes or in different stages of their respective developments.

If this "multidimensionality" of science is the case, then one can legitimately ask why would we believe that the notion of science can be explicated as a single concept (let alone for now the problem of whether any such explicatum would correspond to a natural kind). The investigative project advocated in this work aims to find out whether there are sufficient reasons for holding such a belief.

If we do find out a set of (known) properties of scientificity that makes all scientific CFs, theories, practices, attitudes, etc. be scientific in the same sense (this is contingent, and therefore would have to be empirically discovered), then (1) it would be possible and useful to compare very different fields, theories, practices, etc. with regard to their respective degrees of scientificity; and (2) a single total ordering of, say, CFs with regard to their respective degrees of scientificity would be possible as long as one knew the "weights" of all the properties in each CF's respective subset of possessed properties of scientificity (having considered the particular stage of development and the particular subtype of CF in question). (1) and (2) would be possible even if these fields, theories, practices, attitudes, etc. were scientific (or non-scientific) in many different ways.

With the implementation of this method, (1) some of the CFs consensually considered to be clearly scientific will not feature all the relevant properties listed in sufficient degrees, (2) some legitimate young science may be left outside of the domain identified as scientific, and (3) some pseudosciences will feature some properties of scientificity (cf. Kuhn 2000, 214; Mahner 2013, 39). We will probably be dealing with a vague concept: borderline cases will probably exist. These weaknesses are tolerable. That is why an epistemic justifiability profile of a CF, theory, practice, attitude, etc., if accomplished in this way, although it must be reasonable, might not reach the status of a clear-cut or definitive assessment. Given the acknowledged limits of the endeavor, it should not be considered necessary (at least in the short term) that the list of properties be exhaustive or unchangeable, provided that it conveys a sufficiently complete representation of science.

Also, because science is a historically and culturally situated activity, the degree of pseudoscientificity of a given discipline, theory, practice, etc. can vary socially and historically (Thagard 1978, 227; 1988, 169–170; Pigliucci 2013, 18; Hansson 2013, 74; Mahner 2013, 34–36; Hoyningen-Huene 2013, 206), because the epistemic circumstances—from which we take our reasons for believing—alter as we (appear to) learn more, or forget what we have (appeared to) learn, or as we justifiably believe differently.

3 Descriptivity, Normativity, and Territoriality in SDs

The idea that reliability is key to demarcate science from pseudoscience is defended by Hansson (2013). Two conceptual points against Hansson should be made. Firstly, he equates reliability with being "as close to correctness as can currently be achieved" (2013, 66), but neither correctness nor being "as close to correctness as can currently be achieved" is equivalent to reliability. Some descriptions made using the Ptolemaic model of the Solar System are reliable to a certain extent, but now we know that model to be incorrect and we

have a more correct model (although correctness can be used as evidence of reliability). Secondly, Hansson equates reliability with epistemic warrant (2013, 67 and 70), but, again, these two notions are not equivalent. Epistemic warrant (justification) is even weaker than reliability, as it does not necessarily lead to reliable results (for example, in Gettier cases).

Unlike Hansson (2013, 73–75), this article maintains the hypothesis (subjected to eventual falsification or improvement) that science (or at least our best examples of it) is currently, in general, mature enough for properties related to method to be included into a general and timeless definition of science.

The demarcation of science from non-science, and the demarcation of science from pseudoscience, are SDs based on properties that can be correlated to higher or lower degrees of epistemic reliability or success of outcomes. For example, biology, as a science, features certain properties that art lacks and that make biology be epistemically more reliable than art; and astronomy features certain properties that astrology lacks and that make astronomy predict better than astrology.

The SDs of this type are descriptive and not normative, because all they require are solely descriptive propositions indicating that a property is or is not sufficiently possessed or that a condition is or is not sufficiently met, and that there is a correlation between that property or condition being sufficiently possessed or met and higher or lower degrees of epistemic reliability or success of outcomes. I will call these SDs epistemic-warrant scientific demarcations (EWSDs).⁹ Therefore, a definition or demarcation of science, or of non-science, or of pseudoscience, need not rely on nor imply any commitment to values or norms.¹⁰ Any such definitions or demarcations would only become normative upon the inclusion of, or their conjunction with, normative content.

Of course, EWSDs can be used to derive normative prescriptions, for example of the form “We should believe *x* and disbelieve *y*”.

Several positions on EWSD can be combined with different positions on what Boudry (2013, 79–81) calls territorial demarcation, which “is concerned with a classification of knowledge, or a division of labor between different disciplines, and not with epistemic warrant” (justification)—unless some fields offer less justification than others. For example, if physics and chemistry offer the same amount of epistemic warrant, then demarcating between them is not an exercise in EWSD, but a mere exercise in territorial demarcation (and because physics and chemistry are both sciences, it is also an intra-scientific demarcation).

Particular instances of EWSD (e.g., concluding that ethology is a scientific CF) can be territorial in the sense that the particular sets of epistemic properties on which they are based can be exclusively or better epistemically suited to respective definite fields.

⁹ This name was chosen because scientific epistemic warrant (scientific justification) is the best way available to us to arrive at propositions that can be the content of our most reliable beliefs on issues about reality that are difficult for us to accurately evaluate (i.e., issues which are the purview of science). Scientific epistemic warrant is achieved when CFs, theories, practices, attitudes, etc. are sufficiently endowed with the above-mentioned properties that can be correlated to higher degrees of epistemic reliability (cf. Mahner 2013, 37).

¹⁰ Even if we use the prefix “pseudo-”, that does not necessarily imply that we are making a normative assessment, because the fact that something is false may be good, bad, or neither good nor bad. An example of it being good is if Socrates had drunk false poison. Incidentally, this is also an example of a situation in which it would even have been good to present something false as the real thing. Of course, all concepts that are denoted by words are normative in the trivial sense that there are rules about how we ought to use those words, but Resnik (2000, 251) is wrong in conflating this sense with other senses of “normative”.

For example, ethology is epistemically reliable in part because it has to take into account learned behaviors, but this property is not well suited to other scientific CFs, such as chemistry.

Some of the most broadly comprehensive and usual CFs mentioned in the academic bibliography on philosophy of science are: common sense (CS), natural sciences, social sciences, psychology, “hard” sciences, “soft” sciences, technological sciences, empirical sciences, the humanities (including philosophy, human history, etc.), formal sciences, mathematical sciences, complexity sciences, and science in the broad sense (SBS, also known as broad science¹¹), which includes all the other ones mentioned except for CS (cf. Sect. 4). Each of these CFs encompasses certain disciplines, attitudes, practices, and intended kinds of outputs (except for CS, which is not disciplined enough to encompass disciplines), so different fields could be characterized by respectively different sets of properties (cf. Kuhn 1977, 331). Some fields completely engulf other, smaller fields (i.e., subfields); for example, entomology is a subfield of zoology, which is in turn a subfield of biology.

Certain disciplines (e.g., behavioral neuroscience, which includes biology and psychology) cross over putative territorial borders, making the identification of those borders problematic. Also, there is interdependency, continuity, overlapping, and cross-fertilization existing between these CFs, which is for example evident between philosophy (including metaphysics) and science (Boudry 2013). Quine proposed that our total science, “from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic” (1961, 42–43), belongs to one and the same “fabric” or “web”, in Laudan’s words, that all of it is “cut from the same epistemic cloth” (1983, 124; 1990). Because of this, authors such as Bunge (1983), Hoyningen-Huene (2013), Hansson (2013, 62–64), and Boudry (2017) proposed to demarcate science as SBS (i.e., what Hansson called the “community of knowledge disciplines”), a superdomain that—like the German-language notion of *Wissenschaft*—includes all the “systematic and critical investigations [appropriately] aimed at acquiring the best possible understanding” of reality, including nature, ourselves, our societies, our physical and thought constructions (Hansson 2017), and anything that interacts with those spheres and leaves detectable traces of those interactions (Boudry 2011; 2017; Boudry and Leuridan 2011). Some issues about reality are difficult for us to accurately evaluate, which is why we need systematicity and criticality beyond CS in order to (better) sort them out. SBS includes every CF able to regularly lead an epistemic agent or community to hold beliefs that are sufficiently, and contextually the most, reliable, or at least the most intersubjectively¹² epistemically justified (and better epistemically justified), on issues that are difficult for us to accurately evaluate (cf. Hansson, 2013, 70). (The possession of this ability is a high-level property/criterion of scientificity that does not specify how to arrive at the class of beliefs required. We humans have discovered that sufficient possession of certain low-level properties leads to the possession of the high-level property. The possession of all these low-level properties implies the possession of good conceptual, methodic, empirical, and socio-institutional credentials, which are specified by respective low-level properties.) SBS is the superdomain of all the disciplines that provide the most reliable and fertile—or at least the better founded and most credentialed—non-common-sense knowledge. So SBS encompasses

¹¹ Hansson used the term “science(s) in a broad sense” (2013, 64).

¹² Nagel (1961, 449) defined “intersubjective validity” as the property of having publicly accessible supporting evidence.

all disciplines falling under all the territorially narrower legitimate notions of science, it includes all propositional parts of the humanities, and all CFs that satisfy the necessary and sufficient condition of having the aforementioned ability in their respective domains (cf. Boudry 2013, 93). Hansson (2013; 2017) contends that the CF of SBS is the most adequate one to consider in order to understand the epistemic contrasts implicated in the demarcation of science from pseudoscience. Also, generally speaking, the CFs within SBS seem to be increasingly interdependent.

4 Science and Common Sense

The superdomain of SBS excludes intellectual activities (and their fruits) that have been seen at times as contiguous with science, e.g., common-sense reasoning, common-sense knowledge-seeking, common-sense knowledge acquisition, and common-sense knowledge (also known as everyday, ordinary, or quotidian knowledge). Any SD that fails to capture *any* conceptual difference between CS and science is not of our intended kind of demarcations, because our pre-analytical as well as our philosophically and scientifically informed notions of science imply that there is at the very least *some* difference between science and CS, and therefore the extensions of those notions of science do not include common-sense knowledge and practices. Common-sense knowledge and practices can sometimes be as empirically determined, justified, and reliable, or more so, than scientific ones, and they can be so in ways similar to those of science. CS crosses over to science, and is used in science. Science can discover facts that are also discoverable by CS, and the converse is true to a certain extent. Some elements of CS can change in time, or be simultaneously different in different epistemic communities, some of these changes and variations being merely due to CS becoming loaded with scientific knowledge or with beliefs of other kinds. CS can be protoscientific in the sense that it is previous to science and, in conjunction with certain other provisions, gives rise to science. None of these facts count against science, or preclude us from conceptually separating those components. “[T]he scientist is more careful” and “systematic” than “the common [hu]man” (Quine 1957, 5–6). This increased degree of care is the reason why science is more epistemically potent than CS (Haack 2007, 24–25, 94–109). “[T]he scientist is more narrowly focused on issues of truth and objectivity and, in the hope of contributing to these goals, clearer” (Hylton and Kemp 2019).

Some authors go further, indicating that science departs from CS and natural language to the point of being “unnatural” (e.g., Newton-Smith 1981, 210–212; Cromer 1993; Wolpert 1993; McCauley 2000). This position is compatible with my position expressed in the immediately previous paragraph.

Regarding cases of CFs, knowledge, practices, etc. whose correct categorization appears to be difficult to ascertain (e.g., the professional investigation of crimes or accidents), we may find that they fall wholly either on one side or the other of the line (territorially) demarcating science from CS, but very close to it; or that some of them fall within a third, “middle zone”, having the respective “weights” of characteristics typical of each of these two realms in sufficiently similar aggregated amounts. None of these alternatives would be a fatal problem for the type of SD proposed here.

In any case, an epistemic community or agent needs science only to the extent that it/she cannot discover or be sufficiently certain (in a reliable or at least intersubjectively-justified epistemic manner) just by using CS (but it/she cannot always be sufficiently certain by using science). Common-sense knowledge on issues that are easy for us to evaluate can

be as certain as scientific knowledge on issues that are difficult for us to evaluate, or more so. Boudry (2017) correctly indicates that the line of demarcation between CS and science is relative to (a) the specific endowments of a particular epistemic community—its cognitive and perceptual capabilities—and (b) “the standards of evidence and explanatory depth we demand in any given situation”; to which should be added (c) the degree of precision sought.

5 Conclusions and Practical Applications

We should not give complete credit to Laudan’s 1983 obituary, as the developments reviewed here show that it was premature. But Laudan’s paper inadvertently gives weight to arguments in favor of the territorially broad SD. Indeed, Laudan’s conclusion is that, because (a narrower territorial) demarcation appears to be unreachable, what ought to be demarcated instead is reliable knowledge from non-reliable knowledge; well-founded beliefs from those that are not; claims with conceptual and empirical credentials in their favor, from claims lacking such credentials; and beliefs that are heuristically fertile from beliefs that are not. That is a proposal for an epistemic-warrant and territorially broad demarcation. And it turns out that Laudan’s conclusions are precisely some of the premises leading other authors to propose to demarcate science broadly! (Although Laudan’s proposal was territorially too wide: it referred to “reliable knowledge”, but this would include common-sense knowledge, which is non-scientific, as argued in Sect. 4).

It seems that we can epistemically demarcate SBS from other intellectual activities and outputs. Both types of SDs, territorial and epistemic-warrant, are seemingly feasible of SBS, and from non-SBS. By using the territorially broad SD, in which all of SBS *is* “cut from the same epistemic cloth”, one could defend an epistemic SD criterion, and derived normative prescriptions.

Regardless of the advantages that a territorially broad SD could yield, any demarcation of the CF of SBS could be narrowed down by the introduction of different sets of constraints in order to adapt it to demarcate respectively different scientific subfields. More specifically, different subdivisions of the CF of SBS could be demarcated naturally, i.e., in a non-arbitrary way that corresponds to actual philosophical and scientific reflection and practice. For example, adding the requirement that the justified or reliable beliefs be only about nature would demarcate the subfield of the natural sciences. And it appears that respective EWSDs are feasible for such territorial subdivisions. This is why using the concept of SBS will allow us not only to demarcate SBS from non-SBS and from pseudoscience, but also to demarcate different types of sciences and of scientific CFs within SBS (e.g., formal sciences, natural sciences, social sciences, human history) from each other, so as to not overlook the important specificity, with respect to properties, of such subfields.

Were this project to be taken forward and successfully completed, we would be equipped with an intensional SD that works as intended, for the first time in the history of the philosophy of science. The way of demarcating science proposed in this article would constitute a directly applicable practical decision procedure on the scientificity or non-scientificity of theories, CFs, practices, attitudes, testimonies, etc., able in principle to be utilized and applied in any context, for example by governments, regulatory agencies, courts, tribunals, and health care and education providers.

This work agrees with Resnik (2000) that “any particular definition of science may be evaluated in terms of the consequences of accepting that definition” (2000, 262). This is

due to the fact that the use of a more reliable definition of science is more likely to better help us to serve our interests and reach our goals, whatever they be. Of course, this kind of evaluation is appropriate only within the limits of, and with respect to, a theoretical definition of science that must feature other content, as success is only an indicator of scientificity, and neither a sufficient nor a necessary condition for it, as history clearly shows.

However, Resnik (2000) also contends that our definition of science should vary by context, as different definitions of science may have different effects on the degree to which we accomplish different goals. But analytical explicative definitions are, by their very nature, inherently theoretical, not practical. This does not impair our capacity to “explore and evaluate different points of view and (...) discuss new or controversial ideas” (cf. Resnik 2000, 263). Resnik’s position stems from the fact that he conflates a definition of science, on the one hand, with different degrees of stringency that we might need to apply in order to get the best results in different situations, on the other. His confusion is generated by the fact that he writes about practical decisions regarding what should be admitted in different contexts, and he conflates admissibility (or utility) with scientificity. Moreover, his contention that “[s]ince the decision to call a human activity ‘science’ has important social, political, and practical implications, one should not expect that an apolitical theory or definition of science should be able to solve the demarcation problem” (2000, 265) is simply a non sequitur, as it is very common for apolitical theories to have important social, political, and practical implications, e.g., our apolitical theory of disease etiology leads us to vaccinate children.

But it is correct to recognize that different degrees of stringency could be the most appropriate in different situations, or for the achievement of different goals. As above-mentioned, the SD procedure advocated here could be adapted to yield conclusions assigning non-binary degrees of scientificity to the assessed units, and therefore different institutions could decide to require different degrees of scientificity in order to greenlight or to fund the same project, product, treatment, etc. Also, standards of scientificity featuring different degrees of stringency could be stipulated for purposes or matters of correspondingly different degrees of importance (e.g., a government could stipulate a requirement of a higher degree of scientificity in matters related to human health than in matters only related to non-human animal health).

The demise of the SD problem was prematurely diagnosed because of the insufficiency of the tried proposals, and due to skepticism about multicriteriality’s prospects. Philosophers of science should not have capitulated so soon. It is our hope that this article has indicated a clear way ahead in the form of an executable project to achieve SD.

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Compliance with Ethical Standards

Conflict of interest The author does not have any conflicts of interest to declare.

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