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Epistemological scientism and the scientific meta-method

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Abstract
This paper argues that the proponents of epistemological scientism must take some stand on scientific methodology. The supporters of scientism cannot simply defer to the social organisation of science because the social processes themselves must meet some methodological criteria. Among such criteria is epistemic evaluability, which demands intersubjective access to reasons. We derive twelve theses outlining some implications of epistemic evaluability. Evaluability can support weak and broad variants of epistemological scientism, which state that sciences, broadly construed, are the best sources of knowledge or some other epistemic goods. Since humanities and social sciences produce epistemically evaluable results, narrow types of scientism that take only natural sciences as sources of knowledge require additional argumentation in their support. Strong scientism, which takes sciences as the only source of knowledge, also needs to appeal to some further principles since evaluability is not an all-or-nothing affair.

Keywords Demarcation · Epistemic access · Epistemic evaluability · Scientism · Institutional epistemology · Scientific methodology · Social epistemology

1 Introduction

Recently a lively discussion has emerged among proponents and adversaries of scientism (see, e.g., Boudry & Pigliucci, 2017; de Ridder et al., 2018; Mizrahi,

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2022a). Scientism can be seen as a strict form of naturalism where science serves as a guide to ontological commitments and epistemic practices (Ladyman & Ross, 2007; Rosenberg, 2012[2011]).[1] Some champions of scientism are also scientific realists. Among this camp, one can find such thinkers as Mario Bunge, James Ladyman, Don Ross, and Alexander Rosenberg (Bunge, 1986; Ladyman & Ross, 2007; Rosenberg, 2018). Others, however, have argued that science—and thus scientism—does not presuppose or imply realism or other metaphysical doctrines (Fishman, 2009; Boudry, 2011, 15–18, 235–238; Fishman & Boudry, 2013). Probably the best-known example of such an anti-realist and anti-metaphysical variety of scientism comes from the logical empiricists who explicitly endorsed scientism (*Szientismus*) (Neurath, 1981[1936], 696).

Ladyman, in particular, has defended scientism as a philosophical *stance* à la van Fraassen rather than a doctrine (Ross et al., 2007, 60–65; Ladyman, 2011, 2018). According to van Fraassen, stances may contain factual statements, but, more importantly, they also can encompass attitudes, approaches, commitments, values, and goals (van Fraassen, 2002, 47–48). Ladyman takes scientism to be a combination of the stances that van Fraassen has named empiricism and materialism, which have positive and negative doxastic and methodological components (Ladyman, 2018, 111). The positive core component of scientism—also shared with materialism—is that science has no limits. If something can be studied at all, it can be studied via scientific means. The positive element scientism shares with empiricism is that experience is the best way to acquire knowledge about the world. (Id., 113.) Scientism's core negative commitment is that common sense, intuition, religion, or tradition do not have epistemic authority over science. In a nutshell, no source of knowledge trumps science. This combines empiricism's rejection of a priori knowledge and materialism's denial of the supernatural. (Id., 114.) However, we will not explore whether scientism should be taken as a stance since it will not be relevant to the arguments presented herein.

One crucial issue has been overlooked in the debate concerning scientism. Since the proponents of scientism claim that science is superior to other epistemic endeavours, they must be able to distinguish them. We argue that to do this, the champions of scientism must consider factors of scientific methodology or meta-methodology. In other words, they must give some constraints for the proper methods of science. We propose that a suitable meta-methodological principle is *epistemic evaluability*, the idea that our epistemic practices must be intersubjectively evaluable. We outline what endorsing such a principle amounts to through twelve theses related to justification, applicability, and justifications of justifications.

Literature has identified several types of scientism (cf. Mizrahi, 2022b; Peels, 2018; Stenmark, 1997, 2001). However, this paper considers only the

[1] However, see Kojonen (2016) for an argument that also anti-naturalists can be proponents of scientism.

epistemological varieties. This is because epistemic evaluability is most closely connected with epistemic varieties of scientism.[2] But what is epistemological scientism?

2 Epistemological scientism and its critics

Usually, formulations of epistemological scientism consider science to be the *best* or *only* source of knowledge or some similar epistemic good like justification or rational belief[3] (Hietanen et al., 2020; Mizrahi, 2022b; Peels, 2018; Turunen et al., 2022). From this, a distinction can be made between *strong* and *weak* varieties of epistemological scientism (Hietanen et al., 2020, 526). To wit, strong variants of scientism take science as the only game in town epistemologically speaking (see, e.g., Rosenberg, 2012[2011] 6). In contrast, proponents of weak variants are willing to concede that epistemic activities other than science can at least occasionally be successful or as good as science (see, e.g., Mizrahi, 2017, 354).

Epistemological scientism also comes in different varieties, depending on how "science" is understood. First, one can make the distinction between *narrow* and *broad* epistemological scientism. As the concepts already indicate, these types of epistemological scientism have a different understanding of the scope of science. An adherent of *narrow* epistemological scientism says that only the natural sciences are proper sciences. A supporter of *broad* epistemological scientism, in turn, understands science along the lines of the German term *Wissenschaft*, which, besides the natural sciences, also includes the human sciences—such as the social sciences and humanities. (Hietanen et al., 2020, 525–526) These four categories can overlap, as presented in the following two-by-two diagram (see Fig. 1).

From now on, by "scientism", we will refer to epistemological scientism. So, there is narrow-strong scientism, which insists that the natural sciences are the *only* sources of knowledge, justification, rational belief, or the like. And then there is narrow-weak scientism which states that the natural sciences are only *best* at producing such things. Furthermore, on the lower row, there is broad-strong scientism, which says that sciences, in general, are the only sources of knowledge or something similar. Last but not least, we have broad-weak scientism, which claims that sciences, broadly construed, are the best sources of epistemic goods.[4] It is important to emphasise that the proponents of weak scientism do *not* have to claim that science alone can produce knowledge or other epistemic goods. They merely state that scientific research is the *best* way to achieve them. In other words, anyone can know it is raining by looking out the window. However, according to the proponent of weak scientism, comprehension of the weather conditions is better if it is based

[2] Peels (2018) uses the term "epistemological scientism" and Stenmark (1997) "epistemic scientism". We will use both of these terms interchangeably.

[3] Here, we use the terms "knowledge" and "scientific knowledge" in the same way as sociologists of science and many philosophers of science do, including Ladyman and Ross. Accordingly, the reader should take the mentions of "knowledge" in this paper in this spirit as a term of art for any epistemic good science might produce.

[4] For more details on these different types of scientism and their proponents, see Hietanen et al., 2020.

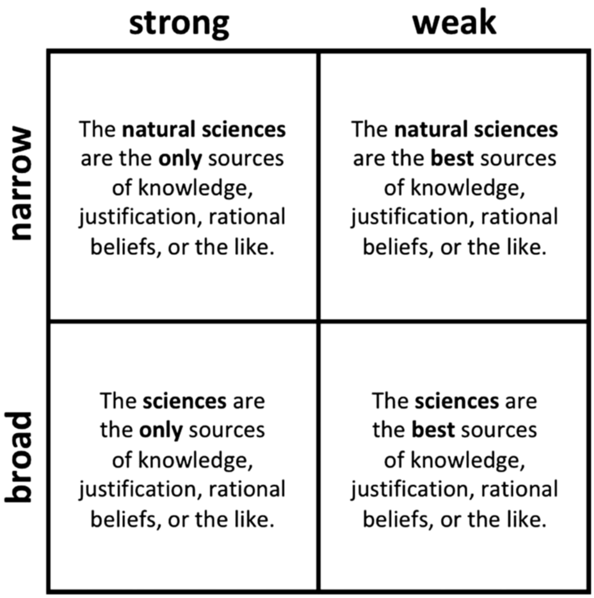

Fig. 1 Four types of epistemological scientism as presented in Hietanen et al. (2020)

on scientific research than what a mere glance outside can provide. Though this, of course, does not mean visual perception is not or could not be part of science.

Ladyman has stated that the four forms of scientism presented here are not exhaustive, and he is not inclined to endorse any of them. Instead, Ladyman accepts a narrow version of scientism even weaker than the weak variety presented here. Ladyman's weaker-than-weak narrow scientism states that if something can be studied at all, it can be studied through natural science. However, natural science does not necessarily offer the best methods for studying the target in question, even if no other source of knowledge can demonstrate scientific results as erroneous. (Ladyman, 2018; Hietanen et al., 2020, 528fn10.)

Scientism is an often criticised stance, to the point that the term is used derogatorily (see, e.g., Haack, 2013, 106). However, it also has its proponents. Some—like James Ladyman, Don Ross, David Spurrett, and Alexander Rosenberg—even take it as a badge of honour (Ross et al., 2007; Rosenberg, 2012[2011]). Apart from this ornamental fencing, there are also well-known arguments against scientism.

The arguments against epistemological scientism are based on the limits and source of scientific knowledge. For instance, is there extra-scientific knowledge, or can knowledge be gained through ways other than science? One such argument is that scientism is self-referentially incoherent. Scientism itself is not arrived at by using scientific means; hence, it undercuts its justification (Peels, 2020; de Ridder, 2014, 27). Another related argument is that science necessarily appeals to some non-scientific, typically metaphysical, background assumptions, such as the uniformity of the world (Midgley, 1992, 108; Peels, 2017; van Woudenberg, 2013, 26). These arguments, however, can be dealt with (see Hietanen et al., 2020).

Scientism has also been criticised by claiming that some areas of study are beyond the reach of science. Such suggested fields include consciousness, human societies, culture, and even life. Here, the burden of proof clearly lies on the side of the opponents of scientism. They must show why a scientific approach to such topics *could not* yield results. A simple assertion will not do. In turn, the proponent of scientism can simply say: “Let’s try and see!” If scientific research can successfully address these questions, then the worries have been demonstrated as misguided. (For a similar argument, see Boudry, 2017.) As an early proponent of such anti-scientistic sentiment, Immanuel Kant claimed that.

> we may boldly state that it is absurd for human beings [...] to hope that perhaps some day another Newton might arise who would explain to us, in terms of natural laws unordered by any intention, how even a mere blade of grass is produced. (Kant, 1987[1790], §75, 400)

Although not yet solved, the problem of abiogenesis seems much more soluble now by scientific means than it did during Kant’s time. The increased scientific understanding of evolution and genetics has paved the way. To sum up, what science is capable of doing, appears to be an empirical question, and one should be careful in making bold statements concerning the final limits of science.

There are also powerful arguments *for* scientism. The most potent is that science has an unmatched track record of epistemic success (Ladyman, 2018; Ross et al., 2007, 7; Rosenberg, 2012[2011], 24). However, the problem is that this track record does not, in itself, tell us the reasons *for* it. In other words, it does not tell *why* science is so successful. To find out why one must look more closely into what science is. This takes us to the problem of demarcation.

3 No escape from demarcation

Recently, there has been a resurgence of interest in the demarcation problem (see, e.g., Pigliucci & Boudry, 2013). Still, perhaps due to Laudan’s (1983) influential criticism, most philosophers of science have not been particularly eager to engage with it again. Nevertheless, the proponents of epistemic scientism are forced to offer an answer to the problem. A simple argument can demonstrate this. As we have seen, epistemological scientism takes science to be epistemologically superior to other epistemic activities. Hence it presupposes that one can separate science from non-science. After all, claiming science to be superior to non-science is ill-conceived if one cannot distinguish between the two. It would be tantamount to claiming that *X* is epistemically superior to non-*X* without explicating what *X* is.

Ladyman agrees that the champions of scientism have to take a stand on the demarcation problem. According to him, the positive core commitment of scientism is “[e]verything real can in principle be investigated by scientific methods and no limits should be placed on what science can study” (Ladyman, 2018, 113). However, he adds that “making this precise requires a specification of what science and scientific methods are” (id., 113n14). Though Ladyman also considers it “unreasonable to demand that the advocate of scientism give a full account of the scientific method but they must have something to say about it" (id., 116).

Merely stating that we already know how to demarcate science in practice will not do since most influential critiques against scientism rely precisely on what science is taken to be (van Woudenberg, 2013, 26; de Ridder, 2014, 27; Peels, 2017, 2018; Hietanen et al., 2020). They, for instance, claim that there are sources of knowledge beyond science. This, of course, presupposes some conception of the extension of science. In addition, there are cases where even the scientific community has no consensus on the scientificity of some research. For instance, some physicists insist that string theory is a physical theory in excellent standing. In contrast, others claim that it is uncomfortably close to pseudoscience (for the former position, see Polchinski, 2019, Kane, 2019, for the latter, see Ellis & Silk, 2014).

We can also be mistaken about where the appropriate perimeters of science lie, as the history of science has demonstrated. For example, several now generally accepted theories, such as Wegener's continental drift hypothesis and Semmelweis' theory on childbed fever, were initially ridiculed (Frankel, 2012, 60, 503; Carter & Carter, 2017[1994], 67–73). Moreover, even the supporters of scientism disagree on where the proper boundaries of science should be drawn. The fact that there is a distinction between narrow and broad conceptions of scientism makes this evident: some champions of scientism, like Rosenberg, take only the natural sciences to be proper sciences, while others, like Jerry Coyne, are ready to endorse a broader conception of science (Rosenberg, 2012[2011], 6, 20; Coyne, 2015, 107).

Another way the proponents of scientism might try to avoid answering the demarcation problem is by referring to clear cases of science, pseudoscience, and other forms of inquiry. If the apparent instances of science enjoy a considerably better track record than the other epistemic endeavours, then one could argue that (epistemic) scientism is justified by that fact alone. Therefore, one would not necessarily need to know by virtue of what science is generally distinct from pseudoscience in order to state that science is the epistemically best game in town. The problem with this suggestion is that it either rests on a very bold inductive generalisation or will only give us information concerning clear cases and thus be useless in the actually interesting cases. Even if certain clear instances of science are epistemically superior to some clear instances of something else, we do not know if this holds in other cases too—like the ones discussed in the previous paragraph. The crux of the issue is that if we do not know *why* certain clearly scientific instances are epistemically successful, we also cannot know whether less clear instances of science should be expected to be similarly successful. Such induction must be justified by showing how the different instances are related to each other in terms of their epistemic success. This is tantamount to answering the demarcation problem.

Not only must the champions of epistemological scientism address the demarcation problem—or rather, because they must address it—they must also have some idea of *how* science is carried out in practice. Recall that epistemic scientism takes science to be epistemologically superior compared to other epistemic *activities*. For such a position to be feasible, there must be a difference between the way science and these other activities are carried out. This, of course, implies that there must be some way science is carried out in practice. In fact, all forms of scientism, even non-epistemological ones, must distinguish science from non-science, regardless of whether they comprehend the scope of science differently.[5]

The traditional way to approach the demarcation problem was to appeal to *the* method of science. Here, the term "method" is used in a singular form on purpose. It is well-known that this way of handling the demarcation problem was fraught with difficulties. After the logical positivists (see, e.g., Ayer, 1959, 31–39; Carnap, 1959[1932], 62–77; Schlick, 1948, 482–505; Karl Popper (2005[1935], 17–20), there have not been practically any appeals to a single method of science as a proper criterion of demarcation—at least not ones that have widely been taken seriously by philosophers of science. Indeed, serious doubts about a singular method of science have been raised, at least starting with the work of Paul Feyerabend (1993), Imre Lakatos (1978), and Thomas Kuhn (1962).[6]

Against this background, it comes as no surprise when Naomi Oreskes writes in her book *Why Trust Science* (2019, 55):

> "There is now broad agreement among historians, philosophers, sociologists, and anthropologists of science that there is no (singular) scientific method, and that scientific practice consists of communities of people, making decisions for reasons that are both empirical and social, using diverse methods."

So, according to our best current understanding, there is no singular method of science. Since there is no such method, it seems like there are two options for the proponents of scientism. One is to make an all-encompassing list of the proper scientific methods and say: "You have to use one of these, or otherwise you are not doing science." Unfortunately, there are at least two problems with this approach.

First, criteria are needed to decide which methods to include and exclude from the list. Then, of course, one would have to *justify* these criteria or risk begging the question. The champions of scientism would have to take up *this* task instead.

Second, one would want to keep the list open-ended unless it is sure that we have discovered all the methods that science will ever need. It seems plausible that we have not yet reached the end of history regarding scientific methodology. Moreover, if this is the case, the whole point of making a list becomes moot. To see this, suppose we have an open-ended list of the methods of science with the caveat: "There might be something else too that could or should be listed here, but we just have not included it yet." However, that would mean that the list on its own would not be able to tell us whether something not mentioned on it should be accepted as science. Of course, if we have unambiguous criteria for selecting methods for the list, we do not

[5] For other varieties of scientism see Peels (2018) and Stenmark (1997; 2001).

[6] The logical empiricists and Popper concentrated on the scientific method and the scientific status of linguistic statements. Kuhn and Lakatos, in turn, were engaged in demarcation on a very different level of analysis. Kuhn was interested in scientific paradigms and their ability to solve puzzles, whereas Lakatos aimed at separating progressive and degenerating research programs. (For more on this, see Hirvonen & Karisto 2022.) Kuhn and Lakatos' approaches have been highly influential and remain relevant to current discussions on demarcation. At the same time, most scholars are critical of Popper's original formulation of the problem.

need to know all its members at the outset. Indeed, we can do away with the list, as these selection *criteria* will be doing all the heavy lifting.

So, a list without criteria for constructing it is a non-starter. The other option is to use something different than methodology as the criterion of science. Currently, the most popular choice is to refer to the social organisation of science (Longino, 1990). This is what, for instance, Oreskes does (2019, 55). However, there is also a problem with this approach.

Let us take a concrete example. Among the proponents of scientism, Ladyman and Ross have used social criteria for demarcating science from non-science. Their project is naturalising metaphysics and, as such, the demarcation of bad (non-naturalistic) metaphysics from good (naturalistic) metaphysics (Ross et al., 2007, 34). Since Ladyman and Ross wish to attain their goal by basing their metaphysics on science, they require a general way of demarcating science from non-science. To wit: "science is, according to us, demarcated from non-science solely by institutional norms" (id., 28).

When they discuss science, they mean *bona fide* institutional science. According to Ladyman and Ross, *bona fide* institutional science is funded by "serious" foundations or funding agencies. (Ross et al., 2007, 33, 36, 37–38) At first blush, this is not a terrible suggestion, given that academic funding bodies appear to be doing something right. However, the proposal raises the question: *Which* funding agencies are the "serious" ones; *which* of them should be taken seriously?

Perhaps, Ladyman and Ross would like to propose something to the effect that serious funding agencies are the ones that fund *bona fide* science. However, this would lead to a vicious circle. After all, if good science is defined through good funding institutions and good funding institutions by the science they fund, we cannot say which institutions and what research is actually good. In addition, what would prevent pseudoscientists from defining their institutions in the same way? They could merely insist that *their* institutions are the good ones. Indeed, for Ladyman and Ross' proposal to have any bite, we need to have some independent criteria for distinguishing the "serious" funding agencies from the "unserious" ones.

As we can see, Ladyman and Ross have merely passed the buck from one demarcation endeavour to another. They exchanged the problem of demarcating science from non-science for the problem of demarcating *bona fide* funding institutions from non-*bona-fide* ones. This new demarcation problem is as much in need of a solution as the original problem, and there is no indication that it will be any easier to solve than the initial one.[7]

Elsewhere Ladyman has proposed an evolutionary approach to the demarcation problem: science can be characterised "as whatever evolves from current science" (Ladyman, 2018, 118). Here again, the question of demarcation is simply swapped to the question of demarcating some *earlier* science. After all, one must have something

[7] Though, it is fair to note that according to Ladyman, appealing to *bona fide* funding institutions is a mere "first approximation" where science and scientific methods are "given by ostension of the actual scientific community" (Ladyman, 2018, 113). Ladyman is, therefore, likely willing to concede that a more thorough analysis of the institutional norms of science would be required for a proper answer to the demarcation problem.

one knows to be science to start evolving further versions. On top of this, it is not clear that everything which evolves from science will be science. Ladyman even explicitly admits this by stating that "the arguments that matter in practice are those concerning extending current science into new domains" (ibid.). However, he also suggests that all sciences share a common core. They, for example, employ logic and mathematics, and in the empirical sciences, "empirical testing is the ultimate source of epistemic authority" (id., 117–118). Now, this is something one can work with and is very close in spirit to what we will be considering in the following section.

4 Epistemic evaluability as the meta-method of science

The proponents of scientism insist that science is superior to other epistemic endeavours by appealing to the unsurpassed success of science. However, in order to see what counts as science, they need a solution to the demarcation problem. But the two proposed avenues for demarcation—methodology and social organisation of science—both seem problematic. However, perhaps there is a third option for tackling the problem of demarcation; an avenue between the two, which encompasses parts of both.

To figure out what might aid us in our quest, it will be helpful to look at the justificatory practises used in science. After all, the social organisation and methodology of science are both central to justification. When Oreskes states that "scientists need to explain not just what they know, but how they know it" (Oreskes, 2019, 57), she is writing precisely about justification. Even though there are significant differences between justificatory practises in the sciences, some of them seem universal. These include, among others, publishing results, peer-review, using similar inferential steps, explicating those steps, and so on.

There is something that should be noted about these seemingly universal practises. They all aim to make scientific claims, justification, and expertise *visible*. We suggest that the rationale behind this visibility is *evaluability* and that it exemplifies a meta-methodological principle of *epistemic evaluability* (Hietanen et al., 2020; Turunen et al., 2022). In other words, scientific research is made public or visible precisely so it can be intersubjectively evaluated. The proponent of epistemological scientism could, therefore, argue that science is epistemically superior because it subscribes to this meta-method of epistemic evaluability.

Furthermore, we argue that the methodology *and* the social organisation of science both presuppose epistemic evaluability. In other words, epistemic evaluability is a general necessary background condition for all science.[8] After all, an important part of the social organisation of science is to evaluate research and facilitate

[8] Although we argue that epistemic evaluability is a necessary condition of science, it does not follow that it must also be a sufficient condition. So, we are not offering a definitive answer to the demarcation problem, nor do we think a satisfactory solution would unavoidably require a set of individually necessary and jointly sufficient conditions of science (Hirvonen & Karisto, 2022). However, for instance, different multicriterial approaches show promise of being practically satisfactory answers to the problem (see, e.g., Mahner, 2013, 38–41; Fernandez-Beanato, 2020). It is also worth mentioning that we are inclined to think that many of the indicators of science in such proposals can be derived from epistemic evaluability.

its evaluation. Social epistemologists typically do not think that mere consensus is enough. They are not saying that because people agree about something, it is correct. They are not saying this even if the people come from diverse backgrounds. However, the fact that many different people agree can certainly be an indication that the object of the agreement is true. Still, this is not what social epistemologists are usually interested in. Instead, they are interested in the *social process* that brought about the agreement. (Fricker et al., 2020, xviii; Kerr, 2020, 200–201; Miller, 2020, 231–235; Goldman, 2001, 98–99; Goldman & O'Connor, 2019, 3.6.)

But what exactly *is* epistemic evaluability? What does it demand? Before we look at these questions, one should note that epistemic evaluability is a *meta-methodological* principle. It can have very general implications, and from the offset we do not know what other methodological principles are consistent or independent of it. For this reason, must to tread carefully and avoid any unnecessary assumptions. We also often must formulate familiar notions in a new way to avoid unintentional interpretations.

5 Evaluability in justification

On a very general level, evaluability has one crucial requirement. Since any analysis hinges on differences, which are distinctions, those distinctions need to be evaluable for an analysis to be evaluable. In other words, one must be able to evaluate what belongs to one side of the distinction and what to the other. This, of course, requires some criteria for such evaluations: one has to say how a distinction distinguishes; how it makes a difference (for a classic take on this, see James, 1907, 45).

The demand for proper distinctions is highly relevant here. This is because a proponent of scientism is basically saying that some conclusions, the ones that science produces, are preferable to others. This requires a distinction between different possible conclusions and, most importantly, some criteria for making such distinctions in practice. Justification can be taken as providing that criterion.[9] This implies that justifications need to be evaluable—at least if the proponent of scientism appeals to epistemic evaluability. We can codify this as our first central thesis on justification:

> **1. Evaluability Thesis:** For there to be a difference between being justified and unjustified, justifications have to be evaluable.

Here a justified conclusion is a conclusion with a justification, and an unjustified conclusion is a conclusion that has not been given a justification. Justifications are how conclusions are justified.

For a justification to be evaluable, it cannot be totally arbitrary in how it justifies something. We need to be able to say how and when a conclusion is justified. For a community of inquirers, as in science, this means that justifications need to be public. Also, it is not enough to simply claim that a given justification can be presented

9 Explicating what justification consists of also helps us to better understand the dynamics of the game of "giving and asking for reasons" (Brandon, 1994, 159; Rorty, 2016, 36).

in some way, since merely claiming that does not yet enable us to evaluate said justification. Due to this, the way a justification is presented needs to be intersubjectively accessible. This gives us a second thesis:

> **1.1 Presentation Thesis:** Justifications must be intersubjectively evaluable through some means of presentation.[10]

This means that one cannot just claim something to be justified if one does not show how it is justified. Without some kind of presentation, it would not be possible for an outsider to evaluate whether a conclusion is justified or not.

It follows from the presentation thesis that conclusions which are simply taken as justified without argument are not justified.[11] This is because their justifications are not presented and thus are not evaluable. After all, merely stating something is justified does not tell *how* it is justified.

Justification depends on the conclusion one is trying to justify. If this was not the case, then the same justification could be used for any conclusion, and all conclusions would be justified. We would not, for example, want to be able to justify all possible conclusions with the fact that ice cream is cold. Otherwise, justifications like *the Eiffel Tower is 324 m tall because ice cream is cold* or *the Earth is flat because ice cream is cold* and the like would be acceptable. If, on the other hand, there would not be any justifications at all, then all possible conclusions would, of course, be unjustified. In either case, there would not be a distinction between justified and unjustified conclusions: all conclusions would always belong to just one of the categories.

Similarly, justification must depend on something we can use to determine when a justification applies and when it does not. For instance, a justification for the conclusion that Earth has tides because the Moon causes them should depend on whether the Moon exists.

Finally, since justifications have to be evaluable in some way, and since a justification is a justification for some conclusion depending on some other initial reasons, like the coldness of ice cream, a justification must also present how the conclusion is supposed to follow from the initial reasons: it has to connect the justifying reasons to the conclusions that are being justified in an evaluable way. This presupposes that the presentation of the justification specifies what conclusions are being justified, which reasons justify them, and how they are connected. From the above, we can infer that an evaluable justification must have at least three components. They are, as is normal for arguments: (1) The *conclusion* that is being justified. (2) The *justification relation*, which is usually presented as the logical form or the link between reasons and a conclusion of an argument. (3) The *reasons* that justify the conclusion. Often, the reasons are presented as the premises of an argument. This gives our next thesis:

[10] Note that the means of presentation do not have to be linguistic. This will be discussed in more detail below.

[11] Note that assumptions are not such conclusions. Assumptions do not need to be justified within a system of justification (see Sect. 6). Rather, their justification pertains to the use of a particular system of justifications to a particular case.

1.2 Structure Thesis: A justification always consists of three parts: reasons, a justification relation, and a conclusion. Justifying is done with reasons through a justification relation to a conclusion.

Justifying a conclusion always starts from a set of reasons and then uses some justification relation(s) to arrive at a conclusion. Here justifying does not refer to the psychological or social processes by which we come to have certain justifications. Instead, justifying refers to the logical process of justifying. The structure thesis merely explicates the formal roles of the reasons, justification relations, and conclusions.

It is important to note that reasons and conclusions do not have to be linguistic sentences. They may be, for instance, images—like photographs or drawings—tables of numbers, models, graphs, charts, mathematical structures, or results of laboratory studies, field practices, and interactions with measurement instruments (see, e.g., Bogen & Woodward, 2005, 226). For example, suppose someone is asked whether they have a copy of Plato's *Republic*. In order to prove (i.e. to give a reason) that they do, they do not necessarily have to say anything. Instead, they may walk to a bookcase, take the book from its shelf, and show it to the person who asked the question. Similarly, in science one may present a picture or a graph to support one's conclusion. Despite this, many reasons and conclusions can be, and often are, expressed in a linguistic form.

The structure thesis does not in itself impose any constraints on what an acceptable justification relation can be. It must, however, be a relation that connects reasons and a conclusion via an evaluable presentation. So, for example, "ice cream" is not an acceptable justification relation, but the sentence "Stinky wants ice cream because he is hungry" includes one. Of course, some conclusions can be more justified than others. The view presented here takes no stand on when this is the case. It only considers whether or not a justification has been given for a conclusion. How good a justification is, depends on the context of use, that is, how the reasons and conclusions are connected in the context in question.

There is yet one final and crucially important constraint on justification that emerges from evaluability. Since, according to the structure thesis, every justification is evaluated with respect to the three aforementioned structural features, all conclusions can be, at most, conditionally justified. This is because we can only evaluate whether a given conclusion is justified if we also have specified reasons and justification relations. Evaluation depends on reasons and justification relations; thus, it does not provide a justification in the absence of them. This leads us to our final thesis regarding justification:

1.3 Conditionality Thesis: Every conclusion is only conditionally justified with respect to its reasons and justification relations.

Every justification of a conclusion is a justification only given the reasons and the justification relations. Therefore, if we want to hold on to the evaluability of justifications, we can only take conclusions being justified with respect to some reasons and justification relations. This means that we always need to assume some reasons and justification relations. Consequently, every justification is only conditional. In mathematics, this could mean that some reasons or premises are taken as axioms and the justification relations are taken as logical deductive rules. In physics, the reasons could be, for instance, Planck satellite data and the justification relations implication of Friedmann equations.

The conditionality thesis also implies that circular justifications add no justification for the conclusion. This is because such justifications are only justified on the condition that the conclusion is given. Yet, the whole point of justifying conclusions is to evaluate whether they should be endorsed or not. If that evaluation depends on the conclusion itself, then the conclusion is justified only if it is already known that the conclusion is correct and, hence, it does not need a justification in the first place.

It is worth noting that since something always must be assumed, for instance, as an axiom, one cannot exclude the possibility that every justification might turn out to be unsound. Thus, there are thus no final or absolute justifications and, in this sense, justification is always fallible.[12]

The theses give us some constraints for what evaluable justification should be like. However, a proponent of scientism is not merely saying that certain conclusions are better justified but also that they are better justified conclusions about the target of inquiry. This means that the conclusions need to satisfy a criterion called *applicability*.

6 Applicability

To be epistemic, the justified conclusions need *to be* about something. Otherwise, they do not provide knowledge *of* something. In addition, we need to be able to evaluate how they are about that something to work out what implications the conclusions have. We call conclusions and reasons that are evaluable in this manner *applicable*. Applicable conclusions do not need to be correct, but they are such that correctness can matter.

In what follows, it will be useful to talk of *Systems of Justification* (SoJ). An SoJ is simply a collection of reasons, justification relations, and conclusions. In practice, an SoJ is what we use to determine which conclusion(s) to endorse with regard to specified cases. An *applicable system of justification* is such that it can be used to work out which applicable conclusions to endorse in specified cases. If we want to uphold a distinction between case-specific endorsed and unendorsed applicable conclusions, we need to be able to evaluate how the SoJ is connected to the cases in question. In other words, applicability requires that the connection(s) between the SoJ and the target(s) it is being used for must be evaluable. We can write this as our next central thesis:

> **2. Applicability Thesis:** For there to be a difference between a system of justification being applicable and not applicable to some case, its connection(s) to that case need to be evaluable.

[12] This relates to *fallibilism*. We will return to this connection in Section 8.

Since applicable SoJ have applicable conclusions, the SoJs in question need to be able to uphold a distinction between applicable and non-applicable conclusions. Evaluability implies that we must have some criteria for distinguishing applicable and non-applicable conclusions. We need to be able to say when and how the conclusions are taken to be correct. This is possible only if we also know what it means for the conclusion to be incorrect. We get the criteria for evaluation and know what to look for through the incompatible alternatives. Therefore, the SoJ must be able to contain both conclusions and their incompatible alternatives. However, this only applies to justified conclusions. Unjustified conclusions, on the other hand, do not need incompatible alternatives for applicability, since they are unjustified and thus are not purported to be correct about any cases in the first place.

Also, since we are concerned with evaluable SoJs, the justifications they contain are always conditional by thesis 1.3. This means that they can only apply when certain conditions are met. So, in order to evaluate whether some conclusion is purported to be correct by some SoJ, we also need to be able to evaluate whether the conditions for its justification are met. In other words, is the case the SoJ is being used for such that the reasons of the SoJ are correct about it? Such evaluations require that we have some criteria for distinguishing such cases and, effectively, that the SoJ also must contain incompatible alternatives for its reasons as well as conclusions. The justification relations, on the other hand, have them by default since the incompatible alternative to, for instance, $A \rightarrow B$ is simply that there is no such relation. These constraints give us a further thesis:

> **2.1. Specification Thesis:** Applicable systems of justification must contain specified incompatible alternatives for its justified conclusions and initial reasons.

Note that the whole point of an SoJ is to have justified conclusions that can be applied to some target. The SoJ, then, should not be *trivial* in the sense that it is either unable to justify any of its conclusions or justifies all of them. Therefore, the SoJ must always include both justified and unjustified conclusions.

Well-known examples of trivial systems are *contradictory* systems. Contradictions allow one to derive any result and, consequently, make all conclusions justified, making the SoJ trivial (Priest, 2002). If such an SoJ has any justified conclusions, their incompatible alternatives will also be justified and thus cannot be well specified anymore within the SoJ. Note, however, that contradictory SoJs are not rejected due to some inherent problem with contradictions but because they make the SoJs trivial. The problem is not contradictions themselves but the fact that contradictions make it impossible for the justified conclusions to be applicable. Moreover, even this only applies to *contradictions*, not *inconsistencies* in general. Some restricted forms of inconsistency can be allowed (ibid.).

It is worth noting that not all trivial SoJs are contradictory. A trivial SoJ can be either *empty* (has no justified conclusions) or contradictory (the incompatible alternatives are not well-specified). This is because non-empty trivial SoJs have a conclusion and its incompatible alternative that are both justified, leading to a contradiction in their specification. Note, however, that it is not contradictory to claim that both a conclusion and its incompatible alternatives are unjustified, since absence of justification for a conclusion is not a justification for its incompatible alternative.

These observations lead to our next thesis:

> **2.2 Triviality Thesis:** Trivial systems are not applicable. Empty and contradictory systems are always trivial.

There is an important caveat here. We know from mathematics that even in a relatively simple system, it might be impossible to show that it is not contradictory (Gödel, 1931; Goodstein, 1963). Demanding for demonstrated consistency would then severely limit the practical applicability of the presented view. It would not be ideal if, in the end, we would need to restrict ourselves to only highly simple SoJs. Yet, triviality is connected to contradictions and must be somehow addressed.

One way out is to note that we are only interested in the distinction between which conclusions are considered to be justified and which ones are not, which conclusions to endorse and which not to. The justification of those conclusions must be evaluable, but this does not mean that it is necessary to know all the conclusions that the SoJ can justify for the SoJ to be applicable in the particular cases we are interested in.

How the distinctions between justified and unjustified conclusions relate to practical situations is a pragmatic matter. This only requires that the justified conclusions of an SoJ can be compared with some evaluable conclusions regarding the target. While it is not entirely clear how and why this is possible, it is nevertheless something we do successfully daily. As long as the SoJ is not known to be trivial, it can still support the relevant distinction between being justified or not, and consequently, it is possible to use it. It may later turn out that the SoJ was contradictory, and so the distinction collapses, but before that happens, it was possible to use the SoJ in some cases. Our interpretation, what we take the SoJ to have justifications for, of the SoJ was consistent, even though that interpretation later turned out to be problematic. This gives us our final thesis on applicability:

> **2.3 Practice Thesis:** For a system to be applicable in practice, its given interpretation has to be such that it is not trivial.

We do not need to show an SoJ to be consistent if our interpretation of it is not contradictory. This means that one can use, say, basic arithmetic to justify claims regarding some target even though one cannot show that the arithmetic system is consistent.

As a simple example, think of balancing a scale. Suppose that there are some coloured weights on the right side of the scale, and your job is to balance the scale by adding coloured weights to the left side. While doing this, you are effectively justifying conclusions regarding how to balance the scale. For instance, a conclusion like this could be: "To balance the scale, put two red weights and one green weight on the left side of the scale." Suppose also that you have a way of calculating which counterweights to put in for which initial weight. These constitute your justification relations. The justifications depend on the initial state of the scale (how many and what weights are on the right side) and on how the initial state is thought to determine the balanced state (whether your way of calculation is correct). The SoJ here is the collection of the possible initial states of the scale (reasons), your rules for working out which counterweights to choose (justification relations) and putting certain weights to the left side of the scale (conclusions).

Suppose you get a scale with, say, three white weights on the right side, and you use the SoJ to work out that you must add two blue weights to the left side to balance the scale. It is not arbitrary which amount and which kind of weights you should put in. We can see that the SoJ conforms to the outlined theses. We know how the conclusions are connected to the activity of placing the weights (2). Each reason and conclusion (having and placing weights) has clear incompatible alternatives (2.1). The SoJ has justified claims and is not contradictory (2.2)—at least not that we know of (2.3). However, unbeknownst to you, the way you work out the balanced state can justify two incompatible ways of balancing the scale for given initial weights. The SoJ is, in fact, contradictory. You are not aware of this, however, so when you do your calculations, you just go with the first solution you get and proceed to balance the scale. The SoJ is still applicable to you since your interpretation of it is such that it only gives one unique result. Once you notice the second result, however, the SoJ is no longer applicable since it cannot determine which solution you should choose. One could, of course, come up with a new evaluable rule for choosing among the two alternatives and make the SoJ applicable again. That would, however, amount to forming a new SoJ since it would have different justification relations.

These theses get us someway in determining what methodological consequences a meta-methodological principle of epistemic evaluability amounts to. However, they are, in an important way, inadequate. The problem is that a justification being evaluable does not in itself make it a good justification. For instance, many pseudo-scientific conclusions are applicable, and their justifications are evaluable. Hence, they fulfil the theses. As an example, homeopathy and astrology are evaluable and not just evaluable. They have been demonstrated not to work (see, e.g., Hartmann et al., 2006; Grimes, 2012). Yet, a proponent of epistemic scientism would definitely not want to take them as epistemically acceptable. What is needed is a way of evaluating which SoJ are epistemically acceptable.

7 Justifying systems of justification

The justification and applicability theses impose some restrictions on the kinds of SoJs we can have, but they are not strong enough to provide us with the "right" ones. Since different SoJs can have different justified conclusions, there must be some way of justifiably choosing between the SoJs, so we can potentially get the desired justified conclusions. We can take this as our third central thesis:

> **3. Epistemic Difference Thesis:** For there to be an epistemic difference between different systems of justification, the systems of justification need to be justified with a system of justification.

Basically, if we want to conclude that some SoJ is epistemically better than another, there needs to be another SoJ, which we can use to justify that conclusion.

For example, we might consider some model epistemically better than another, because it better fits our empirical data.

However, there is a caveat here. If we have two different SoJs, we can always make one of them epistemically superior. This is because we can have an SoJ with a justification relation, "*X* is epistemically superior if it has *Y*", where *X* is the chosen SoJ and *Y* the distinguishing feature. To avoid such trivialities, we need to keep in mind that justifying is a shared endeavour and that we are not generally forced to accept any particular SoJ. This gives us our next thesis:

> **3.1 Sharing Thesis:** A system of justification can only be epistemically superior given some system of justification if that system of justification is shared.

We do not need to accept that a given SoJ is better if we do not share the SoJ where that superiority was justified. For example, we do not need to accept some theory just because a particular group considers it superior.[13]

However, if the epistemic superiority of an SoJ always requires a further SoJ, are we not always in a situation in which we rely on an SoJ that cannot be said to be epistemically superior? Indeed, just as we cannot have evaluable unconditional justifications, we cannot have evaluable unconditionally epistemically superior SoJs. However, this is not a problem since, in practice, we have limited options and are always working with some assumptions in place. The SoJ we rely on for claiming epistemic superiority can be taken as constituting such assumptions. They are not epistemically superior but, instead, tell us about what we *take to be* epistemic success. There are similarities here with Carnap's treatment of "external" and "internal" questions (Carnap, 1950). We have pragmatic constraints for choosing an SoJ (language for Carnap), but once we have chosen one, questions of justification become "internal" questions for that SoJ.

An example of such an SoJ could be one that codifies the idea of empirical testing. It would have justification relations of the form "*X* is epistemically superior to *Y* if *X* better fits our empirical observations than *Y*". Another such SoJ could codify the idea of instrumental rationality with justification relations of the form "If we try to *X* and *Y* results into *X*, we should *Y*". For us here, the most relevant SoJ would, of course, be one that takes epistemic evaluability as a criterion for epistemic superiority.

We can present these considerations as a thesis:

> **3.2 Assumption Thesis:** A system of justification does not need to be epistemically superior if it is taken as an assumption.

It immediately follows that SoJs always either ultimately rely on some assumed SoJ or are themselves assumed. This can also be written as a thesis:

[13] An interesting case here is expert testimony. Clearly we do take expert opinion very seriously, and thus one can sense a certain tension here. This tension is resolvable, as shown in Section 8.

3.3 Reliance Thesis: Every system of justification either relies on some other system of justification or is taken as an assumption. Every system of justification ultimately relies on an assumption or is one itself.

While general in scope, the reliance thesis provides a useful tactic to the proponents of scientism. An epistemically suspect practice can be evaluated in terms of its reliances. If no justifying SoJ can be provided, the practice can be dismissed.[14] If the practice relies on an assumed SoJ, this SoJ can be evaluated. If the assumptions are not shared, the practice can again be dismissed. This means that a proponent of scientism can demarcate desired epistemic practices from undesired ones based on general epistemic assumptions. One such assumption could be epistemic evaluability, as presented here.

8 Discussion and conclusions

We proposed twelve theses for a meta-methodological principle of epistemic evaluability. While many of them are hardly controversial, it is important to recognise their theoretical status: they follow from a general principle and are not independent methodological choices. Abandoning them means abandoning epistemic evaluability, which easily leads to arbitrary epistemologies.

We also noted how evaluable justifications always end up being fallible. This leads to a type of *fallibilism*. According to fallibilism, all or most conclusions are fallible—justified only for the time being. Thus they are possibly subject to revision or even outright refutation as our epistemic situation develops. (Peirce, 1955; Rescher, 2001). However, it is important to note that evaluability is different from *falsificationism*. Evaluability merely requires applicability, not that conclusions can be or are tested. In other words, one only needs to know under what conditions the conclusions would be justified.

The generality of the theses also raises concern for their practical usefulness.[15] After all, evaluating scientific justifications is often very difficult in practice. Indeed, much of science relies on trust, on taking a lot of knowledge for granted. Nevertheless, if the requirements for evaluability are very loose, there is a risk of trivialising epistemic justification, and several false conclusions will be considered justified. Luckily, we do not have to walk this tightrope. The theses merely state that an evaluable justification can be given. If there is an agreement that something is justified, then its justification does not require an additional justification. So, if we share trust in certain medical experts, for example, there is no need to justify their claims. If not, then further justifications should be given, and more thorough evaluations done either for the initial claims or for the reasons to trust the experts. The medical experts should be able to provide justifications for their views, and those justifications can then be further justified until some shared assumptions are found. A conclusion is simply unjustified for the party involved if there are no shared assumptions.

[14] For the time being at least. Just because a SoJ does not yet have epistemic standing, it does not mean that it would not be worth developing further.

[15] We would like to thank Jaana Eigi for bringing this problem to our attention.

Defining science through epistemic evaluability would be particularly appealing for the proponents of broad and weak scientism. However, narrow scientism would not be a good fit since evaluability does not demarcate between natural and other sciences. The human sciences are, in fact, evaluable. Therefore, proponents of narrow scientism must appeal to other principles to elevate natural sciences above others and then face the reliance thesis.

Strong scientism likewise faces troubles. Since it claims that science is the *only* source of knowledge, it needs a strong cutoff between science and non-science. After all, it has a strong cutoff between knowledge and non-knowledge. The theses provide ways for demarcation, but they are not strict enough for the proponent of strong scientism. This is because the theses encompass several pragmatic elements. For example, when is it clear how a reason or a conclusion is taken to be about some case? Or when is a justification presented in an evaluable matter? A proponent of strong scientism will fare poorly with such grey areas. They, too, require further assumptions and then have to face the reliance thesis.

Is what we are proposing here really scientism? Does not *all* good argumentation require evaluability in the sense meant here? Indeed it does, but there is a debate about what counts as good justificatory practices. This is a contribution to that debate. The inspiration for our meta-methodology comes from science—from our attempt to account for the epistemic success of science. We offer this scientific meta-method as a tool for *all* knowledge production: thus, the scientific meta-method becomes a *scientistic* meta-method.

This way of viewing epistemic conclusions and their justification has both benefits and costs. One benefit is the relative simplicity and non-controversial nature of our starting point. If one accepts both that science is successful and that there are (meta)methodological reasons for the success of science, one is easily drawn to this way of thinking. Further, our proposal brings to light the inner workings of scientific argumentation: shareability of premises, justification relations, inferences, and so on. Furthermore, it gives tools for demarcation between science and pseudoscience and, indeed, between science and non-science in general. On the side of costs is the conditional nature of justification, but this is hardly a cost as scientific knowledge is widely recognised as fallible (Rescher, 2001).

Since the theses are very general and explicated without much reference to the details of scientific research, one might worry whether they are, in fact, transcendent extra-scientific epistemic principles. As such, the theses would naturally violate the spirit of scientism since they would not have emerged within science after all. To such concerns, we reply that it is an empirical question of to what extent actual science adheres to epistemic evaluability. Hence, the theses can be taken as empirical hypotheses to be assessed by sociologists of science. It is also good to note that scientific practice is sometimes messy and not always rational on the individual level. Therefore, epistemic evaluability can also be considered a norm recommended for scientific practice. Still, even as such, it should be judged by the empirical success it brings about in research.

Are we here denying the importance of the social organisation of science or social epistemology in studying it? Far from it. We are merely emphasising the more *formal* meta-methodological aspects of the sciences. It is precisely the social organisation of science that helps uphold the theses presented here. That is, the social organisation of science turns the somewhat irrational behaviour of *individuals* into *communities* that employ good epistemic practices. Social epistemologists have argued that the structure of scientific publication and presentation of results (peer review, conferences, etc.) and the incentive structure of science (credit economy, interplay of competition and collaboration) give rise to such communities (see, e.g., Hull, 1988; Longino, 1990; Kitcher, 1993; Oreskes, 2019). Showing that these kinds of practices will lead to adopting something like our theses will, of course, require delving more deeply into the sociology and social epistemology of science. That will, however, remain work for the future.

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