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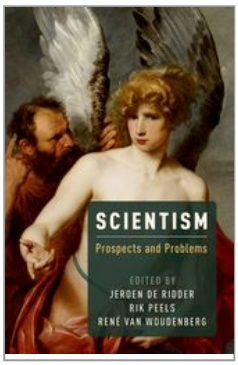

Scientism: Prospects and Problems
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Kinds of Knowledge, Limits of Science

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Abstract and Keywords

Does scientific knowledge have limits? This chapter tries to answer this question by first investigating the distinct nature of scientific knowledge, as contrasted with other kinds of knowledge. Two plausible proposals are considered: scientific knowledge as high-grade knowledge and scientific knowledge as objectifying knowledge. The chapter then investigates what these two proposals entail for the question of whether scientific knowledge is limited. It turns out that, on both proposals, there are in-principle limits to what can be known scientifically. This spells trouble for any forms of scientism denying this.

Keywords: scientific knowledge, limits of science, high-grade knowledge, foundationalism, Ernest Sosa, objectifying inquiry, Bas van Fraassen

8.1 Introduction

Knowledge comes in different kinds. Bertrand Russell famously drew a threefold distinction between propositional knowledge, knowledge how, and knowledge by acquaintance (Russell 1910, 1912). Some philosophers discuss the difference between *knowledge-that* (i.e., propositional knowledge) and *knowledge-wh* (i.e., knowledge what, where, who, when; see Parent 2014 for an overview). Additionally, several philosophers think *first-personal knowledge* is importantly different from third-personal knowledge (Zagzebski 2012). Eleonore Stump has drawn attention to the unique characteristics of *second-personal knowledge* (Stump 2010; cf. also Pinsent 2012).

In this chapter, I want to use the insight that knowledge comes in kinds to explore two thoughts: (1) Scientific knowledge is a special kind of knowledge

with distinctive characteristics, which set it apart from what we might call "everyday knowledge." (2) That scientific knowledge is a particular kind of knowledge has implications for what we can know scientifically. More specifically: It implies that scientific knowledge has limits. I will proceed by laying out two plausible construals of the distinctive nature of scientific knowledge—different, but not incompatible—and exploring what their respective implications are for whether scientific knowledge has limits.[1]

(p.191) If the argument of this chapter is correct, it spells trouble for various versions of scientism. Some versions of scientism hold that science has no limits, or at least none that can be established a priori. James Ladyman (this volume) defends such a claim and Rik Peels's chapter contains references to other examples (cf. also various contributors to Boudry and Pigliucci 2018). If the nature of scientific knowledge imposes constraints on what can be known scientifically, then we have reason to reject such no-limits scientism. *Epistemological scientism* maintains that only the natural sciences provide genuine knowledge (Stenmark 2001; Peels this volume). If I am right that scientific knowledge is inherently limited in certain ways, this constitutes at least part of an argument against epistemological scientism, since it shows that there might be truths outside the reach of scientific knowledge. To turn this into a robust objection against epistemological scientism, we would need a further argument to the effect that some of those truths can be known by other means. While my argument will hint at some possibilities, I cannot provide a full positive defense of them here.

The plan for the rest of the chapter is as follows. In the next section, I will say more about what is behind the distinctions between different kinds of knowledge in order to see how we might go about distinguishing scientific knowledge from other kinds of knowledge. In section 8.3, I will introduce the first way of thinking about the nature of scientific knowledge: scientific knowledge as *high-grade knowledge.* In section 8.4, I explore what limits scientific knowledge has on this proposal. Section 8.5 introduces a second way of thinking about scientific knowledge: scientific knowledge as *objectifying knowledge.* Section 8.6 then discusses what this proposal means for the limits of scientific knowledge. Section 8.7 takes stock and concludes the chapter.

8.2 Kinds of Knowledge

As I observed above, many philosophers hold that there are different kinds of knowledge. The short list I gave can easily be extended with further proposals for distinguishing between different kinds of knowledge. Several virtue epistemologists distinguish between low-gr*ad*e and *hig*h-gr*ade* knowledge (cf. Baehr 2011; Battaly 2008; Greco 2010; Sosa 2007). On one interpretation of contextualism, it entails that knowledge has many different **(p.192)** senses, varying with the strength of your epistemic position (DeRose 2009; Van Woudenberg 2005). In theorizing about justification, many have found it useful

to distinguish *basic* from *non-basic* knowledge or *non-inferential* from *inferential* knowledge (this distinction is key for any foundationalist theory about justification or knowledge; see, e.g., DePaul 2011). Perhaps there is such a thing as *tacit knowledge*, as opposed to explicit (or explicable) knowledge (Polanyi 1958). Maybe *self-knowledge* is interestingly different from knowledge of the rest of the world (cf. Cassam 2014; Fernandez 2013; Moran 2001). Recent epistemology has seen a number of proposals for making sense of *group knowledge* or *social knowledge* (Bird 2010a; Brady and Fricker 2016; De Ridder 2014; Gilbert 1989; Lackey 2014; List and Pettit 2011; Miller 2015; Sirtes, Schmid, and Weber 2011; Tuomela 1992). New work at the intersection of philosophy of mind and epistemology is exploring the notion of *extended knowledge*, that is, knowledge that is extended beyond the mind into the world (Carter et al. 2014; Carter et al. 2018).

I take it that none of these distinctions are set in stone. For instance, Stanley and Williamson (2001) have argued that knowledge how is reducible to propositional knowledge and Matthew Benton (2017) has recently proposed that knowledge of persons is different from both propositional knowledge and knowledge by acquaintance. For present purposes, it is enough if this list makes it plausible that the word “knowledge” in fact masks a diversity of different kinds and sub-kinds of knowledge.

On the basis of what do philosophers distinguish among these different kinds of knowledge? It seems to me that there are a number of differences behind the distinctions. Without pretending to be exhaustive, I want to survey a few of them, in order to draw on them later to elucidate a feasible distinction between scientific knowledge and other kinds of knowledge. The point of this survey is neither to establish the reality of these kinds conclusively, nor to present the distinctions between them in rigorous detail. Obviously, a lot more would be needed to do that.[2]

A. *Object*: One way to distinguish kinds of knowledge is by looking at the objects knowledge takes: differences in the kinds of things that are known. Propositional knowledge, of course, takes propositions as its object, but knowledge-wh has a number of different objects, at least at first **(p.193)** sight: persons (knowledge-who), locations (knowledge-where), times (knowledge-when), objects (knowledge-which), and whether-clauses (knowledge-whether).[3] Different sorts of objects also seem to be what distinguishes knowledge-that (and knowledge-wh) from knowledge-how. The latter has something like skills or ways to do something as its objects and these are arguably different from propositions and wh-objects. Knowledge by acquaintance in turn differs from these other kinds of knowledge because it presumably has real things or persons, rather than propositions about these things or persons, as its objects. This is not to suggest that any difference in the objects of

knowledge gives rise to a different kind of knowledge. Clearly, there is no interesting difference in kind between knowledge of stuff in my office and outside it.

B. *Mode of relating to the object*: Differences between kinds of knowledge can also have be based in how a subject relates to what is known. Some examples can clarify what I have in mind.

i. When you have propositional knowledge, you can be related to the proposition known in a purely theoretical way: knowing that p need not have any practical import. Knowledge-how, however, is practical by definition, in the following sense: You cannot possess knowledge-how without having certain skills and being able to employ those skills in some suitable range of circumstances.[4] You typically cannot know how to ride a bicycle if you have never ridden one. Nor can you know how to do long divisions if you have never been instructed in doing them or practiced doing them.

ii. Some forms of knowledge necessarily involve (the possibility of) mental access to what is known. When you know something, you can bring the object of your knowledge before your mind's eye, so to speak. Propositional knowledge, knowledge-wh, and knowledge by acquaintance are like this. Other kinds of knowledge can remain wholly implicit (although they need not necessarily be so). You can know how to do something without ever mentally representing your skill. Similarly, it is the very definition of tacit knowledge that it isn't explicit; it manifests itself in cognitive and practical behavior, but **(p.194)** isn't accessed and represented consciously. The relation between yourself and the object of your knowledge isn't mediated by mental awareness.

iii. Another difference is whether your access to what is known is mediated by some representational structure. Propositional knowledge, at least as traditionally conceived, is conceptually structured.[5] Relevant facts are captured in the form of propositions that relate various concepts to one another and apply them to the situation at hand. To know that there is a cat on the mat involves some sort of easily accessible internal representation that has as its content a cat on the mat. Knowledge by acquaintance, in contrast, is not supposed to involve such an intermediate vehicle.[6] It is a form of direct awareness of an object or person, unmediated by descriptions, propositions, concepts, or similar representational structures. To know your headache by acquaintance, for instance, is not to know that you have a headache or to apply some concept to it, but to be (or have been) directly aware of it. Something similar goes for knowledge-how. To know how to do something does not require having an internal representation of your doing it (although it of course it doesn't

exclude it either). This is why it is often claimed that knowledge-how can be tacit and need not be explicable in words or imagery.

C. *Strength of epistemic position*: A further basis for distinguishing kinds of knowledge lies in the strength of your epistemic position vis-à-vis the object of your knowledge. There are at least two ways to make sense of this.

i. The first is in a purely *quantitative* manner. A straightforward way to think of this is in terms of how strong your justification (or warrant) is. If it is strong enough, you have knowledge. If it's even higher, you know for sure, or you're certain. Suppose you see a horse **(p.195)** approaching about 2000 feet away. If your eyesight is perfect and you're in clear daylight, you'll be certain that there is a horse there. However, when there's a fog, you can't be very sure, even if you do discern a horse-shaped figure with enough clarity to know that there's a horse approaching. Strength of justification varies with how much justification you have from one source (eyesight under more or less suitable circumstances) and with the number of different sources of justification (perception, testimony, memory, reasoning, etc.). On such a purely quantitative approach, however, there are no non-arbitrary sharp boundaries we can latch onto to distinguish between different *kinds* of knowledge. At best, we could draw up rough categories with context-sensitive and vague boundaries.

ii. The second way to think about the strength of epistemic positions is in a *qualitative* way. Suppose you're an externalist about knowledge and think that reliably produced belief without defeaters suffices for knowledge. Then, your belief that you see a horse in the above example will constitute knowledge. But now suppose you also reflect on your belief and form the meta-belief that it was formed in appropriate circumstances, through your reliable eyesight, on the basis of clear visual appearances. Or suppose that you have considered that your friend, who testified to you that a horse would be coming, is one of the most honest people you know and that she has never lied to you. Arguably, your epistemic position—while continuing to constitute knowledge—is now better. Suppose next that you happen to be an optometrist and know a lot about the physics and neurology of visual perception. Presumably, your epistemic position would be even better still. The general thought is this: There are different kinds of epistemic standing that might constitute your having knowledge and these standings give rise to different kinds of knowledge. Purely externalist standing might suffice for knowledge, but if you also have one or more characteristically internalist standings vis-à-

vis a proposition, your knowledge is of a different, possibly superior, kind (cf. Sosa 2007: 129, 2009: 193).[7] Such internalist standings include reflective meta-beliefs about your first-order **(p. 196)** belief: beliefs about what the grounds for your belief are, about the reliability of your belief-forming mechanisms, about your belief being properly based on your grounds, about how your belief coheres with other things you know, or about your intellectual motivations in forming the belief. Differences like these are behind Sosa's distinction between animal and reflective knowledge (Sosa 2007, 2009), Greco's distinction between knowledge and understanding (Greco 2010: 7–10), and behind virtue epistemologists' distinction between low-grade and high-grade knowledge (Baehr 2011: 40ff.; Battaly 2008: 652–659; Roberts and Wood 2007: 109; Zagzebski 1996: 273–280).

D. *Mode of acquisition*: A final basis for distinguishing different kinds of knowledge involves the way in which knowledge is acquired. Some kinds of knowledge can only be acquired in specific ways. A priori knowledge, if it exists, is only available through something like rational intuition or insight. First-personal knowledge (such as self-knowledge) is only available through methods that are exclusively available from the first-personal perspective through faculties like introspection and proprioception. To the extent that there is first-personal knowledge, it can only be acquired through these methods; there is no other route to it. Something similar goes for second-personal knowledge. Some philosophers have argued that there are truths, particularly about other people, we can only come to know by interacting directly with them. No amount of third-personal descriptions or propositional knowledge about those people can give you such knowledge (and of course it also cannot be obtained from the first-personal perspective). Says Eleonore Stump:

> Second-person experiences cannot be reduced to first-person or third-person experiences without remainder, and so they cannot be captured by first-person or third-person accounts either. . . . Knowledge of persons accessible in second-person experiences is not reducible to knowledge *that.* (Stump 2010: 78)

The kind of knowledge that is most familiar in epistemology, third-personal knowledge, is available through various sorts of familiar methods (perception, testimony, reasoning, etc.) that do not depend crucially on the internal perspective of particular people or on interactions between people.

(p.197) This concludes my overview of possible ways to account for different kinds of knowledge. I will draw on this list as a toolkit to get clearer about what is specific about *scientific* knowledge, as opposed to everyday knowledge.

8.3 Scientific Knowledge: First Pass

One might expect to find building blocks for a general characterization of the nature of scientific knowledge in the debate about a demarcation criterion between science and non-science or in contemporary epistemological reflections on scientific knowledge. It turns out, however, that neither of these places have much to offer on the topic. Reflecting on the demarcation debate in 1983, Larry Laudan concluded that it had come to naught:

> It is probably fair to say that there is no demarcation line between science and non-science, or between science and pseudo-science, which would win assent from a majority of philosophers. Nor is there one which should win acceptance from philosophers or anyone else. (Laudan 1983: 112)

This remains the majority opinion and the demarcation question has largely lost the interest of philosophers of science.[8] Equally remarkably, neither recent mainstream epistemology nor philosophy of science say much about the general nature of scientific knowledge. The former remains mostly focused on more mundane instances of knowledge, while the latter appears to have become wary of general claims about science, focusing instead on narrower philosophical questions pertaining to the special sciences.[9]

This doesn't mean that we're completely at a loss for ideas about the distinctive nature of scientific knowledge. For starters, we can revisit the old philosophical ideal of scientific knowledge: *scientia* or *episteme*. As used by philosophers such as Aristotle and Descartes, this notion of knowledge **(p.198)** entailed a particularly elevated epistemic status, which still seems like a fitting aim for science. In science, we're not content to "just know," we want to understand the reasons why things are as they are and to grasp how everything hangs together. I'll say more about this in the next section. Next, one of the few contemporary philosophers of science whose work does contain suggestions about the nature of scientific knowledge is Bas van Fraassen. According to him, it is "objectifying knowledge." It has a special mode of representing its objects, which we don't find in other forms of rational engagement with the world. I'll elaborate on his thoughts in section 8.6. These two proposals set the agenda for the rest of the chapter: I'll develop them in more detail and explore what they imply for the limits of scientific knowledge.

8.4 Scientific Knowledge as High-Grade Knowledge

If anything, scientific knowledge is supposed to be high-grade knowledge. This has been part and parcel of philosophical reflection on knowledge from the very beginning. Plato already contrasted *episteme*, "genuine" knowledge, with mere *doxa* or opinion. In a similar vein, Descartes distinguished *scientia* from *cognitio*. In contemporary epistemology, this distinction is typically rendered as the contrast between knowledge and mere (true) belief. While there is something to be said for this rendering, it is clear that earlier philosophers had a more

demanding epistemic state in mind than what many contemporary epistemologists—especially those of an externalist bend—call knowledge. For Plato and Descartes, as for many others (BonJour 1985; Fumerton 1995), merely having a reliably produced true belief or a belief that is true because of an exercise of one's intellectual virtues (understood as reliable faculties) wouldn't have sufficed for "genuine" knowledge. Arguably, then, a more fitting contemporary rendering of *episteme* and *scientia* would be *high-grade knowledge, scientific knowledge*, or *understanding* rather than simply knowledge.[10] But regardless of these historical-interpretive questions, there is a distinction between low-grade and high-grade knowledge and it makes sense to think of scientific knowledge as high-grade.

What I said above in section 8.3 (under C) about strength of epistemic position provides two options to think about high-grade knowledge. First, we can construe it as differing *quantitatively* from low-grade knowledge. High-grade knowledge is knowledge that has *a lot of* positive epistemic status. This certainly clarifies part of what is epistemically good about scientific knowledge. **(p.199)** Scientists observe phenomena very carefully and systematically; use the best available observation techniques; repeat their observations where they can with independent methods; and engage in a lot of double-checking, both of their own observations and inferences as well as those of their peers. As a result, at least the low-level observational claims on which scientific models and theories are built typically have a great deal of justification. The results of science, of course, form a complex edifice of claims, models, and techniques with different levels of justification. Even if low-level observational claims are typically highly justified, we cannot assume that such high-grade justification transfers losslessly all the way up to higher-level theoretical claims, since the latter can be quite loosely connected to the former via more or less risky interpretational and ampliative inferential steps. But in so far as high-level claims are based on lower-level ones through valid interpretations and inferences, and have withstood independent critical scrutiny, they, too, can be highly justified.

Do such quantitative differences amount to a difference *in kind* between scientific knowledge and more mundane types of knowledge? Does low-grade knowledge transform into a different kind of knowledge once the amount of justification or warrant reaches a certain level? As I said above, that seems implausible. It is unclear and arbitrary both where the threshold between low-grade and high-grade knowledge should be and why a continuum of levels of justification would divide into just two kinds of knowledge, rather than three, four, or an infinity. There may be significant quantitative differences between the amount of justification for ordinary everyday knowledge claims and low-level scientific knowledge claims, but this doesn't constitute a difference in kind.

The second—and more plausible—way of thinking about high-grade knowledge construes it as qualitatively different from low-grade knowledge. We can take a cue from Ernest Sosa's virtue epistemology (Sosa 2007, 2009, 2011). He distinguishes between *animal* and *reflective* knowledge. Animal knowledge is true belief that is creditable to an agent's exercise of intellectual virtue. As Sosa thinks of it, an intellectual virtue is a reliable belief-forming faculty. A subject thus has animal knowledge when her belief is true *because of* the reliability of the faculty used in forming it. It is important to note that animal knowledge does not require the agent to have any kind of access to, or beliefs about, the reliability of her cognitive faculties, or the fact that her belief is true because of the exercise of reliable faculties. It is an entirely externalistic account of knowledge.[11]

(p.200) Reflective knowledge demands more. It requires "that the knower have an epistemic perspective on his belief, a perspective from which he endorses the source of that belief, from which he can see that source as reliably truth conducive" (Sosa 2009: 135). Hence, to know reflectively, you not only need animal knowledge but also a "perspective" on it, that is, a true meta-belief to the effect that your first-order true belief was indeed formed through reliable faculties. Moreover, this meta-belief, too, should be formed by a reliable cognitive faculty and it should be true because it was formed this way. For you to have reflective knowledge that there is a cat on the mat, you should not only form a true belief through your use of reliable vision, but also reliably form a further true belief that your vision is indeed reliable (in the current circumstances) and that this accounts for why you have a true belief about the cat on the mat. Reflective knowledge thus requires internalist epistemic standing: access to some of the factors that account for the strength of one's epistemic position.

Abstracting away from the particulars of Sosa's position, we can think of high-grade knowledge more generally as knowledge that requires internalist standing. There are several options for such standing (cf. Alston 2005 for an overview): access to (a) the *grounds* for your belief, (b) the *quality of the grounds* for your belief (or the *reliability* of the cognitive process by which your belief has been produced), (c) the fact that your belief has been *properly based* on good grounds (or *non-deviantly produced* by a reliable belief-forming mechanism), (d) the *coherence* of your belief with your further beliefs. These standings can also be combined. The most high-grade (and demanding) type of knowledge would involve all of (a) through (d). In order to have high-grade knowledge, you not only need to find yourself with a reliably (or virtuously) formed true belief, you also need to know something about how or why you find yourself in this epistemic position. On this proposal, the *qualitative* difference between low-grade and high-grade knowledge is that the latter requires not just more of the

same status that low-grade knowledge also has, but another sort of epistemic standing added to it, namely internalist awareness.

This construal of high-grade knowledge fits well with scientific knowledge. What matters in science is not just reliable (virtuous) observation and reasoning (i.e., animal knowledge), but also insight into why your observations and inferences are reliable (i.e., reflective knowledge).[12] This is why scientists **(p.201)** care so much about methodology, research design, proper experimental setup and execution, and correct use of statistics and other inferential methods. Scientific knowledge thus requires internalist epistemic standing: access to the grounds for one's conclusions and to the epistemic goodness of those grounds.[13]

8.5 The Limits of High-Grade Knowledge

Let's now consider what follows from the above proposals for the limits of scientific knowledge, thought of as high-grade knowledge.

Since the first proposal—high-grade knowledge as quantitatively highly justified knowledge—seemed unpromising, I'll be relatively brief about it. Are there limits to how much justification we can acquire for certain propositions? Presumably there are, and they have to do mostly with contextual factors, such as what is known, what methods are available, technological developments, and our spatiotemporal position in the universe. To illustrate, consider Galileo. Given what was known and what could be observed and measured in his time with a reasonable degree of precision, it was very difficult, if not outright impossible, for him to acquire a lot of justification for heliocentrism. Even though later research confirmed that he was right, it is probably false that, at the time, Galileo really *knew* that heliocentrism was true and certainly false that he knew it with a high degree of justification. And so it goes: As our knowledge, methods, and technology develop and become more reliable, we can acquire more justification for extant beliefs and acquire new highly justified beliefs.

There are no in-principle limits to scientific knowledge in sight here, with two important but uncontroversial exceptions. First, given the laws of nature and our position in space-time, parts of the universe must remain unobservable to us, as they are located outside our light cone. It is physically impossible for information from these parts of the universe to reach us (barring sci-fi scenarios). We might still be able to know various things about these parts of the universe by inferring them from our knowledge about the observable part of the universe, but verifying this knowledge through direct observation and measurement is impossible. Hence, there is at least a limit on the kinds of justification we can possess here. Whether this translates into a limit on the **(p.202)** amount of justification we can have is an open question, the answer to which depends on how reliable our inferential methods are.

Second, our natural human cognitive equipment might pose limits to what we can know with high degrees of justification.[14] Given the sorts of brains and perceptual capacities that we have, there must be limits to what we can grasp and know. Of course, our perceptual capacities have been greatly enhanced and extended by all manner of equipment and this process will no doubt continue. But even so, everything we learn must come to us in sensory modalities that we can take in. Our brains, too, might pose limits to what we can know. In the philosophy of mind, so-called mysterians have argued that we will never be able to know the full explanation of consciousness because that is impossible given the abilities of brains like ours (McGinn 1991). Perhaps a similar case could be made for other highly complex phenomena. Less radically, and in line with the present discussion, perhaps there are propositions for which we cannot acquire a very high degree of justification because their complexity outstrips our cognitive abilities. If that is indeed the case, it would follow that we cannot know such propositions scientifically.

Let's consider the second way of thinking about high-grade knowledge: scientific knowledge as knowledge with internalist epistemic standing. As noted above, there are various sorts of internalist standing.

We can get a handle on one class of beliefs for which it is impossible to acquire internalist standing of kind (a)—access to the grounds for your belief—by revisiting popular foundationalist views about epistemic justification. Foundationalists distinguish between *basic* and *non-basic* beliefs (DePaul 2011; Huemer 2010). The latter are beliefs that we accept on an inferential basis, on the basis of other beliefs; and the former are accepted in a direct way, not on the basis of other beliefs. Ultimately, the foundational structure of all our non-basic beliefs must bottom out in basic beliefs. If things go well epistemically, in the sense that our beliefs are justified, this foundational structure also lends justification to our beliefs. At the bottom, justified basic beliefs are justified, not because of their inferential connections with other beliefs, but in virtue of some other epistemically good-making feature (e.g., their indubitability; self-evidence; or their being produced by well-functioning, basic belief-forming mechanisms). Justified non-basic beliefs, in turn, get **(p.203)** their epistemic status from other (justified) non-basic beliefs and ultimately from justified basic beliefs.

From this, we can see that justified basic beliefs, by definition, lack one type of ground: other (justified) beliefs. But some of them will have other grounds that are accessible upon reflection. Consider a lowly perceptual belief: I see that there is a tree in my garden. Assume this belief of mine is justified (there was nothing wrong with my eyes, conditions for tree-perception were suitably normal). Presumably, the ground for this belief is something like my perceptual experience, my being appeared to tree-ly, or it seeming to me that I see a tree.

For such beliefs, internalist standing (a)—access to the grounds for belief—is possible.

But some basic beliefs appear to be groundless. What would constitute the grounds for beliefs that basic logical, mathematical, or conceptual truths hold, or for simple memory beliefs? Such beliefs simply strike us as true, but they aren't based on perceptual or other experiences. At best, they are accompanied by non-experiential seemings, but such seemings are not their *grounds*. The relation between them and the relevant beliefs is quite different from the paradigmatic grounds-for-belief relation as it holds between experiential beliefs and the experiences on which they are based, or between inferential beliefs and the beliefs on which they are based. For one thing, the seemings that accompany these beliefs are much less specific and conceptually structured than the typical grounds for beliefs. Seeing a tree is an experience with rich content, which can only serve as the ground for fairly specific beliefs about that tree. The seeming that accompanies belief that *modus ponens* is valid is just a general sense that it must be so, which is hardly distinguishable from the seeming that accompanies, say, belief that 2 + 2 equals 4. We don't have different seemings for various basic conceptual, logical, and mathematical truths, whereas our experiences for different experiential beliefs are clearly different (no one would confuse seeing a tree with hearing a train approaching). Similarly for memory beliefs: When I remember that I lived in Rotterdam in 1999 this is just how it seems to me. A different memory would elicit roughly the same seeming (although there might be differences in how compelling and strong the seemings feel, but it's not as if each memory belief has its own unique seeming). While this is not a rigorous argument that there are groundless basic beliefs, it at least suggests that some categories of basic beliefs lack grounds.

If this is right and there are indeed groundless but nonetheless epistemically upstanding beliefs, then they constitute a limit for scientific knowledge. Perhaps they can be known (that will depend on your exact theory of **(p.204)** knowledge), but they cannot be known *scientifically*, since it is impossible for these beliefs to have the required internalist standing.

Internalist standing of type (b) requires access to the epistemic goodness of the grounds for your belief (or of your belief-forming process). This gives rise to an in-principle limit for scientific knowledge. To know—or believe with justification—that your grounds are truth-conducive or your belief-forming processes reliable, requires you to form an epistemically justified perspective on the reliability of one or more of your belief-forming practices. There are reasons to doubt that this can be done. As Alston (1993) has argued, arguments for the reliability of our basic belief-forming practices are inevitably epistemically circular: Epistemically justified belief in their premises presupposes the truth of their conclusion. When you want to argue for the reliability of sense perception, you inevitably rely on claims that are only justified if sense perception is in fact

reliable. For instance, to know that my perceiving a tree is an epistemically good ground for my belief that there is a tree, I rely on the claim that my visual perception is reliable (in suitable circumstances). If I were to try to justify that claim, I will end up appealing to past cases of visual perception that turned out to be successful. But to know that these past cases were successful, I must rely on either my own visual perception, that of others, or perhaps on other modes of sensory experience. As Alston argues at length, there is no escaping this circle: To believe the premises of arguments for the reliability of sense perception, you must presuppose their conclusion. And the same goes for other basic practices of belief formation: memory, induction, testimony, rational intuition, etc.

Opinions differ about what general lessons we should draw from this. Whether epistemic circularity really prevents us from knowing that our basic belief-forming practices are reliable, depends on further views about the nature of knowledge and justification.[15] This doesn't matter for my purposes. The conclusion I'm drawing is that there is at least a kind of limit to our access to the quality of our grounds for belief—and thus another limit to what we can know scientifically. Epistemic circularity precludes one thing that seems particularly relevant for high-grade knowledge: to wit the possibility of giving an unproblematic (i.e., non-circular) argument for the conclusion that the grounds for some of your beliefs are epistemically good. Since epistemic circularity affects all of our basic belief-forming practices, **(p.205)** this will be true for the grounds of our perceptual beliefs, memory beliefs, testimonial beliefs, inductive beliefs, etc. That is, pretty much all of our beliefs.[16]

Thinking about internalist standing of type (b)—access the quality of the grounds for our beliefs—yields a further limit for scientific beliefs in particular. This one is not an in-principle limit, but an empirically based and developing one. Start with the platitude that science, like all of our cognitive endeavors, is fallible. We should not think that the mere possibility of error throws the quality of our grounds for belief into doubt. That's a quick route to radical skepticism. We can be more specific about how fallibility creates problems for *scientific knowledge*. First of all, Laudan's (1981) famous pessimistic meta-induction suggests that we should not be too confident about the epistemic status of our current best scientific theories. The history of science shows a steady and sure turnover of theories (explanations, models): Older theories are rejected as mistaken in favor of newer and superior ones. Most past scientific theories have turned out to be false. A simple inductive argument should lead us to conclude that, most likely, our current scientific theories will one day be rejected as false.

Granted, the pessimistic meta-induction is not uncontested. Several philosophers have argued that, once we restrict the inductive basis to developed, mature scientific theories or to the core tenets of scientific theories, the turnover rate becomes much less impressive. Instead, we witness significant amounts of continuity and accumulation in the development of science.[17] Such observations,

however, work with the benefit of hindsight. In retrospect, we can see which elements and aspects of scientific theories are preserved in successor theories, but scientists who were working on those earlier **(p.206)** theories would have had at best a partial and uncertain view of which parts of their theories would stand the test of time. The same will be true of current scientists. While these qualifications give us reason to be more confident about the quality of the grounds for long-established central scientific claims and theories, once we approach the frontiers of science, judgments about the epistemic quality of the grounds for belief ought to become less and less certain.[18]

This conclusion is reinforced by recent work in scientometrics. John Ioannidis (2005) has shown that it is highly likely that most published findings are simply false! He draws this conclusion from a simple but realistic probabilistic simulation model that takes into account several parameters of possible research designs and the current socio-economic organization of scientific fields. His startling conclusion is borne out by empirical scientometric investigations. Samuel Arbesman (2012: Ch. 3) reports that collections of what he—misleadingly—calls scientific facts have a "half-life." For any given body of published scientific literature (journal articles, books), there is a more or less predictable time period within which half of it will have been overturned. For instance, after 45 years, biomedical experts reject half of the published findings on cirrhosis and hepatitis as false or superseded (ibid.: 28–29). In another study, researchers found that after about 10 years, half of the papers in the prominent *Physical Review* journals were never again cited.[19] While this doesn't quite show that these papers are flat-out rejected, it does indicate that they are no longer considered relevant. Surely, for some cases, this will be because they are considered false. The exact numbers for half-lives vary among fields and publication types, but the general picture is clear: For any novel published scientific claim, there is a good (i.e., higher than 50 percent on average) chance that it will be discarded sooner or later.

Compare this with a situation in which you assess your own reasoning powers. When you argue, you try to be as careful as you can, and you thus have a pretty high initial confidence the conclusions you reach. You're aware of the grounds for your conclusions and you think they are pretty solid. But **(p.207)** then a trustworthy expert tells you that you only make sound inferences, say, 50 percent of the time. (Or, if you will, you discover this yourself by revisiting conclusions you drew from your past reasoning.) Surely, this undermines your having high-grade knowledge of the conclusions of your reasoning. Perhaps you can maintain some high-grade knowledge by comparing your reasoning with that of others and by finding out what sort of conclusions you have arrived at time and again through multiple independent strands of reasoning. But even so, you should definitely grant that the amount of high-grade knowledge you have is significantly less than you initially thought.

The above empirical facts thus support the claim that there is a limit to what we can know scientifically. Relatively novel scientific claims cannot be known with robust internalist standing, because we do not have reliable access to the quality of the grounds on which they are based. These grounds may look epistemically superb to us at the time, but sober facts about the development of science demonstrate that there is a big chance that they are really not. At best, we may possess genuine scientific (high-grade) knowledge of mature theories and claims.

The upshot of this section is that construing scientific knowledge as high-grade knowledge gives us four in-principle limits and one developing practical limit to what we can know scientifically: (1) facts and events beyond our light cone, (2) things that are beyond our cognitive capacities, (3) groundless basic propositions, (4) the reliability of our basic belief-forming processes, and (5) the great majority of recent published claims.

8.6 Scientific Knowledge as Objectifying Knowledge

I'll now consider the second proposal for thinking about the distinctive nature of scientific knowledge. This proposal focuses on the object of scientific knowledge or, more precisely, on the specific mode by which scientific knowledge represents or construes its object. It thus takes its inspiration from item B on the list in section 8.2. My primary source here will be the philosopher of science Bas van Fraassen, but the idea that there is something unique about the way in which science engages its objects of knowledge is not unique to him. Different versions of it can be found in Neo-Kantianism (e.g., Rickert 1926) and in the work of the Dutch philosopher Herman Dooyeweerd (1997 [1953]).

To warm up to the idea that there is something distinctive about the way in which scientific knowledge represents its objects, I want to revisit the distinction between *observing something* and *observing that something is the case* **(p.208)** (cf. Van Fraassen 1980: 15).[20] You see something fly by your window and notice that it's a sparrow. Of course, you might have failed to notice that it was a sparrow, or even that it was a bird. But you would nonetheless have seen the sparrow flying by your window—except you wouldn't have recognized it as such. That is, you wouldn't have observed *that* a sparrow flew by your window. A natural way to understand the difference is that, in the former case, you have represented the object of your experience in a specific way or conceptually interpreted it, whereas, in the latter, you just have your "raw" experience without (much) conceptual interpretation and representation.[21] This illustrates the thought that a cognitive relation—in this case, seeing—can take its object in different ways. The object stays the same, but our way of engaging it, representing it, or cognizing about it is different.

Something similar occurs in science, according to Van Fraassen. Scientific inquiry, he contends, is *objectifying inquiry* (Van Fraassen 2002: 156ff). Science

doesn't just observe the world willy-nilly, it *objectifies* the world and the things in it. Van Fraassen never gives a crisp and clear definition of what objectification amounts to, but he does discuss five defining features (ibid.: 160–164). In general terms, the goal of objectifying inquiry is to take ourselves out of the picture: our contingent location in space and time; the contingencies of the senses we happen to have; and our subjective limitations, biases, preconceptions, etc. By doing so, science aims to study the world as it is independently of us and our observations and thinking.[22]

The first thing that is required for scientific inquiry—as opposed to other forms of cognitive engagement—is that a *domain of study* is delimited. Scientists specify in advance what sort of things they will study. This doesn't mean that they say that physics is about physical stuff and biology about living beings; the idea is that scientists specify which specific properties and relations they will study by *stipulating the quantities and parameters* that are allowed to figure in their descriptions of the phenomena. It's not that every scientist does this on her own every time she begins a new project. In everyday science, the quantities and parameters are given by the context of broader lines of inquiry, (sub)disciplines, and scientific fields.

(p.209) Second, not just any specification of parameters will do. Parameters must be *independent* of the scientists carrying out the research. Specifications in terms of how things look or feel *to us* will not do. This constraint was also what inspired the old distinction between primary and secondary qualities. Parameters must capture how the phenomena are in themselves and not how they appear to us.[23] Independence is often interpreted as requiring quantitative measurement. Although quantification can indeed be a good way of making sure that parameters do not depend on us, it does not by itself guarantee the right sort of independence. We could quantify an object's size as a multiple or fraction of our own length with perfect precision, but such a parameter would still improperly depend on us. Similarly, if we could devise reliable ways to quantify our subjective taste, that wouldn't suddenly make it an appropriate parameter in, say, chemistry. If we think of ourselves as a measuring apparatus, the thought behind independence is that acceptable scientific parameters cannot depend on the contingencies of the sort of apparatus that we happen to be.

Third, the set of properly independent parameters and quantities must be restricted to those that are *relevant*: Scientific questions and answers must be expressible in terms of the relevant parameters. Unfortunately, Van Fraassen doesn't tell us in general terms what makes parameters relevant or irrelevant, but he does mention Galileo's list of primary qualities for physics (shape, position, motion, contact, and number) as an example. The point of this feature of objectifying inquiry is to avoid introducing empty terminology: meaningless or uninformative notions that lack genuine empirical content. Using a substance's "dormative virtues" to explain why people get sleepy when they use it

exemplifies such empty verbiage. Even if we had a way of measuring such virtues, they would be unsuitable as a parameter in good science as they don't latch onto a relevant or informative quantity. What counts as relevant can develop over time. Before Newton, a parameter like force, describing mysterious action at a distance, would not have been acceptable, but it is a central element of Newtonian mechanics.

We can think of the previous three features as creating a language for scientific inquiry.[24] The fourth feature involves experimentation and observation: "*nature put to the question*" (ibid.: 163, my italics). Scientists observe **(p.210)** the phenomena carefully, often under meticulously prepared (experimental) conditions, and record their observations in the previously accepted language of science, that is, in terms of the relevant parameters and quantities. The result is a systematic set of data, or a "data model," which can subsequently be studied to find patterns.

The fifth feature characterizes scientific *theorizing*. Hypotheses, models, and theories are introduced to account for the data. They, too, must respect the language of science and must therefore be formulated in terms of the relevant parameters and quantities. But sometimes it will be necessary to introduce novel quantities at the level of theory construction. What quantities are permissible? Van Fraassen says that it is impossible to give a general and informative answer to this, but it depends on the preceding theoretical development of a scientific (sub)discipline, on the relation of the proposed novel quantities to older models and theories, and, for the special sciences, on the parameters and quantities already in use in more fundamental sciences. Theoretical quantities can only be observed indirectly, by observing the quantities with which they are connected in the model or theory. The quality of hypotheses, models, and theories is ultimately decided by how well they fit the data.[25]

Now we can see more clearly what it might mean to say that scientific knowledge involves a special mode of representing of its object. Objectifying inquiry requires the deliberate construction of a system of representing the world—a scientific language of sorts—consisting of measurable parameters and quantities that are properly independent of us and that delimit the possible domain of inquiry. This language constrains what can be expressed and observed, and what can make it into scientific theories, because every observation must be put in terms of the relevant parameters and quantities, with possible additions of theoretical quantities.

Let me guard against two potential misunderstandings. First, to draw attention to the objectifying character of scientific knowledge is not in any way to suggest that science *distorts* reality. On the contrary, the fit between a scientific account of a phenomenon and that phenomenon itself might be excellent. But even in cases of optimal alignment, science homes in on its objects in a specific way, by

conceiving of them in pre-specified **(p.211)** relevant terms that we ourselves have authored. The actual phenomena always outstrip their scientific descriptions, in the sense that they have more properties—perhaps even radically different properties—than any particular scientific description captures.

Second, none of this should be read as implying that scientific knowledge is a social construction, or that the acceptability of hypotheses and theories is largely determined by social processes. According to Van Fraassen, the ultimate arbiter for the success of scientific theories—and thus for the parameters and quantities in which they are formulated—is their fit with the data. While he recognizes that observation is theory-laden and that the interplay between theory and observation is often subtle and complex, he remains firmly committed to empiricism. In the long run, empirical observations determine what goes in science.

8.7 The Limits of Objectifying Knowledge

Let’s now consider whether this conception of scientific knowledge poses limits on what we can know scientifically.

The first three features of objectifying inquiry discussed above impose one limit on science. Something that cannot be described in the language of science—that is, in terms of items on the list of relevant parameters and quantities that define the domain of inquiry and that are allowed to figure in the descriptions of measurements and in theories—cannot figure in science. This is not to say that the sciences all have their own sets of parameters and quantities that are fixed once and for all, so that some phenomena can never become objects of scientific study. This would make radical innovation and novelty—genuine scientific development and progress—impossible. Without the introduction of new vocabulary to describe, say, electromagnetic forces, physics would have remained stuck in the first half of the 19th century. So limits imposed by the set of accepted parameters and quantities are relative to time and the state of scientific development. They can be overcome when inexplicable phenomena, puzzling experiments, or theoretical difficulties force scientists to introduce novel parameters or to develop new theories. Giving specific examples of this requires in-depth knowledge of cutting-edge scientific developments, but perhaps the well-known incompatibility of general relativity and quantum mechanics in physics can serve as a toy illustration. Both theories are extremely well established, but also known to be mutually incompatible. Hence, they cannot both be correct as they stand. Recasting this in terms of Van Fraassen’s notions, we might say that the currently accepted **(p.212)** language of physics precludes us from finding a solution to the recognized inconsistency. To unify both theories, novel parameters and quantities might be needed.[26] At any given time, the currently accepted language of science puts limits to what science can

study. This isn't a limit that is fixed once and for all, as science may progress by revising and extending its vocabulary, but it is a limit nonetheless.

Another limit of scientific knowledge derives from the fact that parameters used in science must be *independent* of observers. This requirement excludes the use of parameters that depend on how things *seem to us* or on *what it is like for us* to experience something. In other words, it precludes what are often called *phenomenal qualities* from playing any role in scientific knowledge. This is reasonable enough for most parts of the study of the natural world,[27] but significantly less so for scientific and humanistic disciplines that involve individual and collective human experience in its many varieties: psychology; anthropology; sociology; economics; the full range of the humanities; and many interdisciplinary fields like communication science, business administration, political science, etc. Of course, not every inquiry in these disciplines *must* involve parameters tied to the subjective characteristics of human experience, but many of them in fact do. That is because people's subjective takes on the world will often be an important factor in explanations of behavior. This holds for the study of history, for studying works of art or other cultural achievements, and for many other social-scientific and humanistic inquiries. It would thus seem that Van Fraassen's conception of science poses severe limitations on what can be known scientifically. So much so, in fact, that Van Fraassen's suggestion that the objectifying style of inquiry is characteristic for all intellectual pursuits carried out with academic rigor loses much of its plausibility.

Perhaps, however, these observations bring out an ambiguity in Van Fraassen's characterization of the independence requirement. It might be interpreted (a) as banning parameters and quantities that are essentially tied to the subjective character of human experiences from science altogether; *or* (b) as banning only those parameters and quantities that are essentially tied **(p.213)** to the subjective character of human experience *that are not intersubjectively accessible or verifiable*, that is, that are not independent of the particularities of the individuals conducting the inquiry. The worries I raised in the previous paragraph primarily affect the first interpretation. On this interpretation, much of what goes in psychology, the social sciences, and the humanities would fall short of proper objectifying inquiry. This is probably not what Van Fraassen had in mind.

On the second interpretation, there is no problem with using subjective human experience in scientific inquiry as such, but it must be made to satisfy the demands of objectifying inquiry. Experiences must be codified in terms of parameters and qualities that are independent of any *particular* inquirers. And this is precisely what the methods of the human sciences and the humanities are for. Variables involving subjective experiences are operationalized so that they can be measured on a suitably independent scale, for instance, by means of surveys. Qualitative information obtained in interviews has to be coded in terms

of intersubjectively verifiable variables. Historians have to account for their interpretations of the sources in such a way that others can follow and evaluate their reasoning. And so on. Since Van Fraassen intended his notion of objectifying inquiry to capture every sort of investigation pursued with academic discipline, adopting this second interpretation is the charitable thing to do. The central question about the limits of scientific knowledge thus transforms into the question whether there are limits to what can be captured about subjective experience in intersubjective terms.

There is good reason to answer affirmatively. First, there is a large intuitive difference between experiencing, say, disgust from the first-personal perspective and filling out a survey about what sort of things you find disgusting or scoring the intensity of your disgust on a five-point scale. Similarly, how satisfied you are at your job, when experienced from within your own subjective point of view is very different from the list of answers you might give in a work satisfaction survey or in a face-to-face interview, even if you're completely forthcoming. The same holds, a fortiori, for more complex experiences such as appreciating Edward Hopper's *Nighthawks*, being engrossed in Marilynne Robinson's *Lila*, playing volleyball, or maintaining a romantic relationship with your significant other. These and many other subjective experiences are so richly textured, multifaceted, and holistic that they resist description by standardized quantities. This is not to deny that parts and aspects of such experiences can be described by means of intersubjectively accessible variables, but much in them eludes such description. To the extent that first-personal perspectives can be conveyed to others, novels and other **(p.214)** works of art are arguably more suitable to the task. This is precisely because they don't adhere to the rules of objectifying inquiry.

In addition to these intuitive considerations, further support comes from well-known arguments for the inherent subjectivity of conscious mental states (Nagel 1974, 1986) and the irreducibility of phenomenal consciousness (Chalmers 1996). If Nagel and Chalmers are right that consciousness cannot be accounted for in purely physical terms, then its contents are not fully explicable through objectifying inquiry either. This is because phenomenal consciousness and the subjective, first-personal point of view are essentially tied to experiencing "what things are like" for me as an individual. Phenomenal consciousness is *defined* in terms of how things look and feel from the perspective of particular individuals. Hence, attempts to describe it will violate Van Fraassen's independence requirement by definition, since that requirement explicitly excludes the use of variables and quantities that are tied to a subjective point of view. Some aspects of phenomenal consciousness might be shared by many individuals so that they are susceptible to intersubjective access, but not everything is. What it is like for me to experience things has an essentially private quality. This, then, constitutes another in-principle limit for scientific knowledge.[28]

This section has shown that, when scientific knowledge is construed as objectifying knowledge, we find two more limits to what can be known scientifically. (6) One constituted by what Van Fraassen calls the “relevant parameters and quantities” for a field of scientific inquiry. Whatever cannot be described in terms of them, cannot be studied scientifically (or at least not until science has progressed to revise its vocabulary). (7) Subjective, first-personal experiences: “what it is like” to experience various kinds of things.

8.8 Conclusion

Thinking about the nature of scientific knowledge throws cold water on scientistic assertions to the effect that scientific knowledge knows no limits. I hope to have shown that there are at least seven different kinds of limits to scientific knowledge. We cannot obtain scientific knowledge of: (1) facts **(p.215)** and events beyond our light cone; (2) things that are beyond our cognitive capacities; (3) groundless basic propositions; (4) the reliability of our basic belief-forming processes; (5) the great majority of recently published scientific claims; (6) things that cannot be described in terms of currently accepted “relevant parameters and qualities” for various fields of inquiry; (7) subjective, first-personal experiences: “what it is like” to experience various kinds of things.

As I said at the beginning of this chapter, establishing that scientific knowledge has limits, isn’t a full-fledged objection to epistemological scientism, for it might be that the limits of scientific knowledge simply coincide with those of knowledge in general: What cannot be known scientifically cannot be known at all. Defending the claim that there is knowledge to be had outside science falls beyond the scope of this chapter, although I do think that for at least a few of the items on the above list, for instance 3, 4, 6, and 7, it is at least intuitively plausible that we can know them by other means than scientific inquiry.

I want to close with two final remarks to put the project of this chapter in perspective. First, the above list of limits isn’t radically novel or different than what other authors have proposed (e.g., Rescher 1999). While this may seem disappointing initially, I actually think it’s a good thing. It would be rather surprising if careful thinking about the nature of scientific knowledge would suddenly entail that the limits of science are very different from what has been proposed on other grounds.

Where I nonetheless see my project making progress, is in proposing and exploring a different and promising approach to thinking about the limits of science. Rather than relying on more or less intuitive judgments about what science can and cannot do or what sorts of questions it can and cannot address, my approach is more systematic. Hopefully, thinking about the *nature* of scientific knowledge isn’t as divisive as thinking about the limits of science directly. If I’m right about this, my approach gives both proponents and

detractors of the idea that scientific knowledge has its limits common ground for further debate.[29]

(p.216) References

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Notes:

(1.) One caveat about the project of this chapter. Popular discussions about the limits of science often mention knowledge of normative facts—moral knowledge in particular—and knowledge of supernatural facts as intrinsic limitations to scientific knowledge (Hughes 2012; Wieseltier 2013; cf. also Rescher 1999: 243–248; and René van Woudenberg's contribution to this volume). While I have considerable sympathy for these ideas, I will leave them to one side here, because the existence of both robust normative facts and supernatural facts is controversial.

(2.) For the sake of brevity, I will not discuss those kinds of knowledge that wear their differences on their sleeves, such as non-inferential vs. inferential knowledge, individual vs. group knowledge, and extended vs. non-extended knowledge.

(3.) But perhaps all such wh-clauses are ultimately reducible to one or more propositional clauses, so that knowledge-wh is a subclass of propositional knowledge (cf. Parent 2014 for discussion).

(4.) Or perhaps having been able to employ your skills; someone who's recently lost a hand might still know how to play the piano even if she cannot do so any longer.

(5.) Although recent work in enactivism and embodied cognition conceives of belief without any internal representations whatsoever (cf. Hutto and Myin 2013).

(6.) Cf. Bertrand Russell's characterization of it: "I say that I am acquainted with an object when I have a direct cognitive relation to that object, i.e., when I am directly aware of the object itself. When I speak of a cognitive relation here, I do not mean the sort of relation which constitutes judgment, but the sort which constitutes presentation. In fact, I think the relation of subject and object which I call acquaintance is simply the converse of the relation of object and subject which constitutes presentation. That is, to say that S has acquaintance with O is essentially the same thing as to say that O is presented to S" (Russell 1910: 108 (although Russell didn't think we have knowledge by acquaintance of *persons*); cf. Gertler 2012 for recent accounts).

(7.) But cf. Grimm (2016) who argues that the value of such reflective states is not epistemic but moral.

(8.) However, there are a few recent dissenters, who think a feasible demarcation criterion can be provided after all. See Ladyman et al. (2012) and several contributors to Pigliucci and Boudry (2013).

(9.) Some notable exceptions are Alexander Bird's work on the epistemology of science (Bird 2010b), John Hardwig's (1985, 1991) papers on trust and epistemic dependence in science, Elizabeth Fricker's (2002) paper on testimony in science, and Philip Kitcher's (2002) epistemology-oriented overview of prominent debates in the philosophy of science. Even though these authors make insightful points about the goal of science, scientific progress, scientific inference, and the social character of scientific knowledge, none of them attempt to characterize what's special about scientific knowledge when compared to nonscientific knowledge.

(10.) See Grimm (2001) and Greco (2014) for arguments to this effect.

(11.) There are different construals of the internalism/externalism distinction in epistemology. Here I am working with one default understanding of it in terms of whether or not knowledge requires cognitive access to the factors that confer the knowledge status on one's belief (BonJour 2010).

(12.) To be clear: The proposal is to think of internalist standing as a *necessary* condition on scientific knowledge and not a sufficient one. There may be nonscientific knowledge with internalist standing too.

(13.) Elsewhere, I have argued that this feature makes much scientific knowledge *collective* knowledge (De Ridder 2014). Often, no single individual is cognitively able to bear the burden of having such internalistic epistemic standing.

(14.) I'm disregarding the possibility of radical changes in our cognitive make-up as a result of evolutionary or (bio)technological developments.

(15.) See Stroud (1989, 1994, 2004) and Sosa (2004: 315–317, 2009: 166–177, 204–120) for discussion.

(16.) Perhaps some readers are tempted to take this argument as showing that high-grade knowledge of anything is impossible. Since (a) epistemic circularity besets all our basic belief-forming practices and (b) all our beliefs ultimately spring from one or more of these practices, it might seem that we lack the right sort of access to the quality of the grounds for all of our beliefs. I don't think this follows, though. The thing to see is that there are other considerations bearing on the quality of the grounds for a belief, besides a non-circular argument for the reliability of the relevant belief-forming practice. If I see someone familiar approaching nearby, I can notice that my observation takes place in broad daylight, that I often see this person around here, that I'm not under the influence of any substances, etc. All these things tell me something about the quality of the grounds for my belief, even if I don't have an unproblematic argument for the reliability of visual perception in general. Hence, the argument doesn't automatically generalize into a skeptical one.

(17.) See Leplin (1981) and Chakravartty (2007: Ch. 2) for discussion.

(18.) My impression is that most practicing scientists are keenly aware of this, which is why, when pressed, they are usually careful in making explicit knowledge claims. Most will talk of plausibility and probability when characterizing the confidence they have in the latest findings.

(19.) That is, as far as the researchers could tell at the time. It remains a (distant) possibility that some papers will be resuscitated at some point in the future. This is unlikely to happen to a great many papers, however, so the general conclusion stands.

(20.) Or, alternatively, between object perception and fact perception (Dretske 1969).

(21.) I don’t mean to take a stand on whether experience is always and necessarily conceptually interpreted. If that’s the case, then the difference between the former and the latter case should be cashed out in terms of interpretation with more and less *specific* concepts.

(22.) In characterizing science as objectifying inquiry, Van Fraassen is laying out an ideal of science. He is not committed to the claim that actual science always fits this ideal perfectly.

(23.) Of course, when humans are the object of study, parameters will depend on us in the sense that they will describe qualities of human beings. But they must not do so in a way that depends on how these qualities look, feel, or seem to us.

(24.) Note that the metaphor of language is mine. Van Fraassen might not approve of it. It should certainly not be associated with the positivists’ ideal of an observational and theoretical language.

(25.) Van Fraassen is optimistic about how widely his notion of objectifying inquiry applies: “With some minor adjustments the pattern of objectifying inquiry is certainly general enough to include the sciences and *in fact all disciplines pursued with academic discipline*” (ibid.: 165, my italics). This strikes me as a bold claim. I do not find it easy to see how various forms of humanistic inquiry could fit the mold of objectifying inquiry. But it is sufficient for my purposes if objectifying inquiry at least captures what is special about *scientific* inquiry.

(26.) I’ll readily admit that what I say here is speculative and vague. I nonetheless hope that it conveys the thrust of how we could think about one kind of limit of science with the help of Van Fraassen’s picture of science.

(27.) Although it would seem that scientific subdisciplines that are essentially concerned with things that affect human experience—such as nutrition science, pharmaceutical science, medicine, and many engineering disciplines—cannot avoid taking onboard such parameters.

(28.) If there indeed is distinctly second-personal knowledge (cf. Pinsent 2012; Stump 2010), that is, knowledge that is only available through interpersonal interactions with another person, then a parallel argument could be constructed that this constitutes a similar limit for scientific knowledge.

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