justified in believing a contradiction, you now reject (Perception). Thus, contrary to Boghossian's implication, experimental evidence could rationally destroy your credence in (Perception).

In short, Boghossian's chosen examples of principles to which a posteriori evidence is irrelevant show just the opposite. They support something much closer to Quinean holism. Boghossian may be the victim of his own purism about the a priori. A loose, superficial category of the a priori might well have room for his examples. Positing a realm of perfect evidential independence from sense perception sets the bar too high for any human cognition to meet.

D. Reply to Williamson

Paul Boghossian

D.1 Williamson's Argumentative Strategy

In "Is the A Priori/A Posteriori Distinction Superficial?" Timothy Williamson takes another crack at defending his well-known and distinctive view that the aforementioned distinction is real but shallow, of no great significance for epistemology. His strategy is similar to the one he pursued in Williamson (2013), although the type of example he focuses on here is very different. In the earlier work, he focused on *All crimson things are red*, purporting to show that our typical way of knowing this paradigm of a priori knowledge is very similar to the way in which we typically know *All recent volumes of Who's Who are red*, a paradigm case of a posteriori knowledge. In the present essay he zeros in on a different type of example as paradigmatic of apriority: the case of knowledge by mathematical proof.

Actually, there is a certain unclarity about exactly what Williamson's target is in Section A in this chapter. Often, he sounds like he's talking about the very general phenomenon of knowledge by proof. Thus, he says:

> A central kind of a priori knowledge is knowledge based on logical or mathematical proof. Unless such cases normally fall under the category of a priori knowledge, it risks being too sparsely inhabited, too marginal, to matter much. The Distinction's true friends should be willing to maintain that proof-based knowledge is normally a priori. (Section A.2)

However, his actual example involves not merely the phenomenon of knowing M via a proof Π, but the much more specialized case in which, in order to know M via Π, you need to know the second-order claim that Π is a valid proof of M. As Williamson himself acknowledges, it's one thing

> (Simple) To know M by virtue of proving M via Π; and quite another thing
> (Meta) To know M by virtue of (a) proving M via Π *and* (b) knowing that Π is a valid proof of M.

The second type of case will usually involve proofs that are so complex that one can only be confident of M if one is *antecedently* confident that Π is a valid proof of M. By contrast, in many cases of knowledge by proof, one can know M a priori by proving it from a priori premises, without any need for reflection on the proof itself. Although Williamson often talks as though he has shown that Simple cases are akin to a posteriori knowledge, his official target is a Meta case.

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c08.indd 194 10-30-2023 17:43:18

Now, one problem with Williamson's argumentative strategy has to do with the relative specialness of Meta cases. *Even* if he were right about them (and I'm not at all persuaded that he is), that would tend to show very little about the Distinction's significance, or lack thereof. Many of the Distinction's best friends and theorists were willing to cede not only Meta cases to the category of the a posteriori, but even many Simple cases as well. Roderick Chisholm, for example, wrote:

> What if S derives a proposition from a set of axioms, not by means of one or two simple steps, but as a result of a complex proof, involving a series of interrelated steps? If the proof is formally valid, then shouldn't we say that S knows the proposition a priori? I think that the answer is no. Complex proofs or demonstrations, as John Locke pointed out, have a certain limitation. They take time. The result is that the "evident lustre" of the early steps may be lost by the time we reach the conclusion: "In long deductions, and the use of many proofs, the memory does not always so readily retain."
>
> (Chisholm 1989: 29–30)

Here Chisholm is not only rejecting Meta cases as a priori, but also lengthy Simple cases as well. And as he notes, Locke and Descartes held a similar view. How, then, can Meta cases be said to be *central* cases of the a priori, when even Simple cases were held to be a posteriori by some of the Distinction's most important friends and theorists?

I'm not at all in agreement with Chisholm that Simple cases should be regarded as a posteriori – indeed, I don't think they should be. In making this concession, Chisholm is not taking advantage of important distinctions – for example, between preservative memory and substantive memory (see my essay in Section B) – which are crucial to the correct theory of the a priori. I'm merely pointing out that it is dialectically odd for Williamson to try to undermine the significance of the a priori by arguing that a special instance of the broad category of knowledge by proof is akin to a posteriori knowledge, when there is a robust tradition among the friends of the a priori of insisting that many of the instances of even that broader category are a posteriori.

I suppose what Williamson must be thinking is that Chisholm and others radically *underestimated* the damage that would be caused by their concession. As he puts it, if knowledge by proof were excluded, the notion of the a priori would be "too sparsely inhabited, too marginal, to matter much."

There are several problems with his claim. To begin with, the claim that the category of the a priori would be too "sparsely inhabited" if the concession is made is clearly not true if what is meant by "knowledge by proof" is a Meta case. Indeed, it wouldn't even be true if "knowledge by proof" covered Simple cases as well, since there are *countless* propositions that are known in a way that doesn't rely on sense experience, but which have *nothing* to do with the issues raised by knowledge of proof. For example: "A square has four sides," "Red is a color," "If Mary ate the apple and the pear, then she ate the apple," and so on.

A much worse problem, though, is that the implied inference from "too sparsely inhabited" to "too marginal" simply doesn't hold. The philosophical significance of a category cannot be measured by how many instances it has. For example, the notion of a proposition of which I can be *completely certain* has very few instances: perhaps little more than "I exist," "I am conscious," and "I am here now." But that doesn't make it an insignificant category for epistemology: it is a hugely important lesson of the theory of knowledge that certainty is exceedingly hard to come by.

Similarly, for the a priori. If there are even a very *few* propositions that are knowable without appeal to sense experience, that would be enough to make the a priori an important category for epistemology, since we would have to explain how *any* such proposition is so much as possible.

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c08.indd 195 10-30-2023 17:43:18

Hence, even if Williamson were right about everything he claims about Meta cases, he would fall well short of having undermined the significance that the a priori has traditionally had for epistemology.

D.2 Meta Cases of Knowledge by Proof

Let us turn, though, to the question whether he is right about Meta cases. You get a Meta case when a proof Π of M is so complex that in order to know M via Π you need to know the second-order claim that Π is a valid proof of M. That complexity implies, at least for creatures like us, that Π needs to be constructed and preserved in a *language* – a public medium with a syntactic structure. This is so both because of human memory limitations, and because proofs need to be surveyable and checkable by others, including by machines employing mechanical procedures.

In such a situation, it might appear that knowing that Π is a proof of M will require using one's visual capacities, especially one's capacity for pattern recognition: you would need to use your vision to figure out the logical form of the various inferential steps – for example, to recognize that a particular step in the proof is an instance of modus ponens.

Surprisingly, Williamson doesn't address the question how you know that *modus ponens itself* is a truth-preserving inferential rule or pattern, a type of knowledge that *is* a genuine paradigm of apriority; he focuses solely on how you would recognize that some step in a written proof is an instance of modus ponens. Absent an alternative account of our knowledge of the validity of modus ponens itself, and even granting Williamson his thesis about Meta cases, a reader might be left with the impression that a priori knowledge (of the validity of MP) lives strong, even in Williamson's own treatment of knowledge by proof.

Setting this point to one side, though, let's look at whether anything of consequence follows about the a posteriori character of Meta cases. We can start with agreeing with Williamson that, given normal human limitations, complex proofs will need to be written down in a public language, if they are to be checked and surveyed for validity. However, a friend of the a priori may well want to argue, this is just an accident of human circumstance; *in principle*, the proof could be verified by the mathematician in their head. Williamson replies:

> The qualifier "in principle" is a euphemism for a huge idealization … Even if each individual step can be entertained in the head, verifying a proof also involves checking that the individual steps all fit together … It is unclear how one could do that without some lasting record of the proof's individual components, such as writing makes available. (Section A.2)

I fail to see the force of this reply. Once it is conceded that an individual step (basically, a short proof) can be entertained and verified in the head, and so as a result deemed a priori, it becomes a hugely interesting question for epistemology whether, *if only our memory, in its purely preservative role, had been more capacious,* the whole proof could have been verified in the same way. And the answer to that question, at least as far as anything we have seen so far is concerned, would appear to be "yes."

Perhaps mindful of this threat to his argument, Williamson also argues that the initial concession, that each individual step might be verifiable a priori, should not be made:

> a proof in the head is quite similar to one on paper. Visualizing an instance of modus ponens is the offline version of the online process of seeing the instance; they differ primarily in the source of the inputs. One uses a visual recognitional capacity online to recognize … what one sees; one can reuse the same recognitional capacity … to recognize instances of modus ponens in what one imagines. (Section A.2)

UNCORRECTED PROOFS

c08.indd 196 10-30-2023 17:43:18

This idea, that certain capacities can be used both "online" and "offline," has become quite a common way of speaking, despite not being very clear. In any case, I'm not seeing the merits of the claim that in order to recognize the logical form of some thought that I'm having I must be using my *visual capacity* offline.

Do we really want to claim that, if I think to myself the *thought* that I would express in language with the English sentence "If *p*, then *p*," I must be using my visual capacity offline in order to know that I thought that *If* p *then* p? Although knowledge of the contents of one's own thoughts is a tricky and unsettled matter, that seems like a completely unmotivated view of how it is accomplished.

Perhaps Williamson will complain that, in stating the objection in the way that I have, I have snuck in the assumption that proofs are relations between *contents*, rather than relations between *sentences*, a possibility he is eager to refute.

> Friends of the Distinction may try a different line, objecting that such arguments go wrong because they focus on the perceived *forms* of representation instead of the contents which they represent: a proof's written form merely enables access to the *real* proof, a more abstract intellectual structure. (Section A.2)

I have to confess to being guilty as charged. From where I'm sitting, it is obvious that a proof is a relation between contents and not sentences, since *in general* what the sentences of a language do is express contents. On such a view, when I say "Snow is white," I am expressing the proposition that *snow is white*, a proposition which in French I would express by saying "*La neige est blanche*." Given that plausible picture, M would express a particular mathematical proposition, and a written proof of M, Π, would represent a certain structure of propositions, one that shows how M follows by familiar deductive steps from premises P1 to P*n*. Does Williamson's argument about proof by knowledge depend on the unavailability of this familiar and plausible picture of content?

Williamson claim seems to be that, regardless of what we say about sentence content in general, *proofs* cannot be conceived of on the Content view since that would end up neglecting what formal proofs are *for* –

> A generally accepted requirement for a proof system to count as *formal* is that there should in principle be a mechanical procedure for determining of any given proof-candidate (a sequence or array of representations) whether it constitutes a correct proof in that system. (Section A.2)

Yet –

> if the premises and conclusions of arguments were pure abstract contents, with no such quasi-syntactic structure, it is quite unclear what could be meant by a "mechanical test" for instances of modus ponens. Thus treating perceptual form as epistemologically irrelevant risks throwing away the very feature on which the epistemic value of formal proof depends. (Section A.2)

Why, though, should a commitment to the Content view carry a commitment to the view that *the premises and conclusions have no quasi-syntactic structure*? As Williamson knows, many theorists of content (if not most) treat contents as structured entities; and they do so for a variety of powerful reasons of a familiar kind – namely, because our capacity for thought, just like our capacity for language, is *generative* and *systematic*. If the invocation of syntactic structure is the best explanation for these features in the linguistic case, might not the invocation of structured contents also be the best explanation of those features in the case of thought?

UNCORRECTED PROOFS

c08.indd 197 10-30-2023 17:43:18

Since the syntactic structure of sentences can, broadly speaking, be thought to recapitulate the structure of the contents they express, I see no difficulty in explaining how there could be a mechanical test for instances of modus ponens in proof, on a Content view.

To sum up the discussion up to this point: Williamson does not succeed in showing that even the very specialized Meta case of knowledge by proof cannot be thought of as a priori. Proofs are certain kinds of structures on contents.

We write those proofs down on paper both because we can't hold them in mind all at once in order to survey and check them, and because we want to communicate them to others and have them checked by others. The paper on which they're written preserves those contents for reasoning and checking; our vision gives us access to what those contents are. Once proofs are conceived in this way, it becomes easy to show that familiar resources enable us to explain how even a Meta case of knowledge by complex proof can be a priori.

As I emphasized in Section B, the strongest argument for the Distinction is the very strong implausibility of accounting for the justification for all our beliefs, or rules of inference, in empirical terms, as Quine (1951) tried to do (again, see Boghossian and Wright, forthcoming). Williamson is adamant that he does not face this embarrassment, since, apparently, he cannot be claimed to have argued that all knowledge is a posteriori:

> I have *not* argued that logical and mathematical proofs yield only a posteriori knowledge. If some philosophers want to stipulate that knowledge based on logical or mathematical proof is simply paradigmatic of a priori knowledge, let them. For all that, the knowledge was obtained through exercise of the same recognitional capacities through which we can also obtain paradigmatically a posteriori knowledge. (Section A.2)
>
> Does this amount to some form of *empiricism*, on which all knowledge is a posteriori, perhaps on the lines sketched by Quine (1951)? Not at all. For empiricism characteristically minimizes the role of innate structure. If it cannot quite get away with positing an initial *tabula rasa*, it comes as close to that as it can … By contrast, the present picture is consistent with richly differentiated innate structure, such as a Chomsky-style language module. (Section A.4)

Williamson is right to want to distance himself from Quine's unpalatable view that all justification is at bottom empirical. But there is really nowhere to hide. If the correct explanation for the justificational status of any belief (or inference) involves sense perception (or something sense perception-like), then it is, *by definition*, a posteriori. Since the whole point of Williamson's examples is that sense perception is always epistemically involved, that implies that his view is indistinguishable from Quine's.

While it may be generous of Williamson to allow the friend of the Distinction to stipulate that knowledge by proof is to be called "a priori," that would be a *mislabeling* on his view. And the attempt to put daylight between his views and Quine's by emphasizing his openness to innate structures also doesn't cut much ice: the descriptive and empirical question *how much* innate structure we are born with is thoroughly orthogonal to the question whether any justification is a priori. Innateness is a descriptive property; justification (a priori or a posteriori) a normative one. The two properties are entirely independent of one another.

Williamson's argument would not have succeeded in undermining the interest of the a priori even if his treatment of Meta cases of knowledge by proof had been plausible. But, in addition, there is every reason to think that his treatment is not plausible.[15]

[15] I am very grateful to Kit Fine, Yu Guo, Declan Smithies, and Crispin Wright for valuable comments on an earlier draft.

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c08.indd 198 10-30-2023 17:43:18

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