Review of Zalabardo’s *Scepticism and Reliable Belief*

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Zalabardo develops a broadly Nozickian, tracking theory of knowledge, argues that this theory allows for a satisfactory solution to all of the standard skeptical arguments, but then argues that there is one last form of skeptical argument that this theory of knowledge does not protect against. The proper response to this last form of skepticism, he argues, is to reject a realist theory of truth (or of the goal of cognition) in favor of a quasi-realist one.

 The majority of the book is concerned with developing and defending the theory of knowledge. The theory is sophisticated, nuanced, and detailed. There is an appendix consisting of thirty-four theorems derived from the Kolmogorov axioms of probability, and he uses these theorems to apply his theory to important cases. I will have to ignore many of the subtleties here and focus on the overarching view.

 Like Roush (2005), Zalabardo reformulates Nozick’s (1981) counterfactual theory of truth tracking into a probabilistic idiom, and also like Roush, he departs significantly from Nozick in denying that tracking is necessary for knowledge. Zalabardo also differs from Roush in several ways, the most important being (a) that he denies the closure of knowledge under known entailment, which Roush explicitly embraces, and (b) he avows a kind of knowledge that involves neither truth tracking nor inferential support from any such knowledge. His final view is this:

“A true belief is knowledge if [and only if] it tracks the truth, or if the subject is in possession of adequate evidence in its support, or if it is a standing belief (139)”.

Each of these three routes to knowledge merits further explanation.

 Zalabardo replaces Nozick’s counterfactual “sensitivity” condition (if *p* were false, *S* wouldn’t believe that *p*) with a probabilistic one (the conditional probability prob(*S* believes that *p* | ~*p*) is quite low). Probabilities are taken to be contingent, objective, mind-independent facts. Zalabardo rejects Nozick’s “adherence” condition (if *p* were true, *S* would believe that *p*) and also rejects Nozick’s relativization of sensitivity and adherence to methods (if *S* believes that *p* via method *M*, then the relativized sensitivity requirement is that *S* wouldn’t believe that *p*—via *M*—if *p* were false). Tracking, thus understood, yields noninferential knowledge.

 Inferential knowledge of *h* on the basis of *e* requires *S* to know *e* and also to know that *e* evidentially supports h (88). The support relation is understood in terms of conditional objective probabilities: *e* is evidence for *h* only if, first, the likelihood ratio prob(*e* | *h*)/prob(*e* | ~*h*) is high, and second, prob(*h* | *e*) is high. Zalabardo also imposes an important third constraint on evidential support and thus inferential knowledge:

 (PI) *S* can inferentially know *h* on the basis of *e* only if *h* is confirmed by *S*’s believing that *e*

(that is, only if *S*’s believing *e* satisfies the first two constraints on evidence, vis-a-vis *h*) (98). This is a probabilized version of Nozick’s requirement that *S* wouldn’t believe the premise if the conclusion weren’t true. (This is in turn quite similar to Dretske’s (1971) more familiar notion of a conclusive reason, though Zalabardo doesn’t mention it, perhaps because PI is in a probabilistic, inconclusive, form.)

 A “standing belief” is one such that “we have an innate predisposition to form it that is largely independent of input, but we are not equipped to detect its truth value. (137)” Standing beliefs, Zalabardo claims, are known if and only if true.

 So the view is that tracking truth is sufficient but not necessary for knowledge. Inferential beliefs are sometimes known, even when they don’t track truth, and standing beliefs are known when true, even though they never track truth.

 Zalabardo thus holds there are two kinds of knowledge that don’t require evidence: noninferential knowledge, and what I’ll call “standing knowledge”. This would undermine skeptical arguments that invoke a regress of reasons (e.g., the problem of the criterion), because such arguments premise falsely evidential requirements on all knowledge. Of course, this response requires there to really be forms of knowledge that don’t depend on evidence.

 Zalabardo doesn’t say much in defense of standing knowledge. This separate and lower standard for standing beliefs seems somewhat ad hoc and perhaps not very plausible anyway. Everyone in my tribe could be designed to believe that he or she is the tallest in the tribe. It doesn’t seem plausible that the one who happens to be right thereby knows.

 Perhaps more central to his account is noninferential knowledge is. There is a lot one could discuss here, but I want to highlight the difference between the kind of reliability theory we get here and a process reliabilism (e.g., Goldman 1979). The Nozick-Zalabardo view (especially when probabilized and purged of methods) is a kind of *indicator* reliabilism: it holds that the epistemic status of a belief is determined by the objective probability of that particular belief being false; a *process* reliabilist, in contrast, holds that the epistemic status of a given belief is determined by the objective probability that *other beliefs produced in the same way* would be false. One well known difference between the two types of reliabilism is that process reliabilism does a much better with necessary truths. If I believe that 3x3=9 only because dial-a-psychic told me that believing it today would bring me fame and fortune tomorrow, then I don’t know it, even though the belief tracks the truth perfectly. (Zalabardo is well aware that his theory doesn’t handle belief in necessary truths.) Even restricting the theory to contingent truths, indicator reliabilism seems to get many of the cases wrong. One example (see Lyons forthcoming for more): some brilliant neurosurgeons want you to know more neuroscience, so while you’re sleeping, they surgically implant in you the belief that dopamine is released in the nucleus accumbens. They wouldn’t have implanted this if it weren’t true, so your belief is sensitive. But it isn’t something you know. The process reliabilist can say that it’s not knowledge because you didn’t come about the belief in the right way. The indicator reliabilist can’t say this. For these sorts of reasons, I think that process reliabilism is more promising than indicator reliabilism.

 There is, however, one area where indicator reliabilism has a prima facie advantage over process reliabilism, and Zalabardo presses this to some advantage. This area is inferential knowledge, in particular, a broad class of inferences that I’ll call (in deference to Cohen 2002) “easy” inferences. These will include Moorean inferences, e.g., from ‘I have hands’ to ‘I’m not a brain in a vat’; Dretskean inferences, e.g., from ‘those are zebras’ to ‘those are not cleverly painted mules’; and bootstrapping inferences, e.g., from ‘the gauge read full on these occasions’ and ‘the tank was full on these occasions’ to ‘the gauge is reliable’. (I’m lumping these all together for the sake of brevity; Zalabardo carefully distinguishes and separately treats each of the subtypes.) These are a mixed bag in epistemology; some epistemologists (e.g., Pryor 2004) think that Moorean inferences are legitimate, while virtually no one thinks that the bootstrapping inferences are. Zalabardo’s view entails that all of these inferences are bad, because they all violate PI: we would (or might well) believe the premises even if the conclusions were false; thus our premise beliefs do not constitute adequate evidence for these conclusions.

 Intuitions differ here, but mine agree with Zalabardo’s. Furthermore, PI fits naturally into an indicator reliabilism, though it would seem an ad hoc addition to process reliabilism. And without PI or something like it, some of these—especially bootstrapping (Vogel 2000)—pose problems for process reliabilism. It is a strong selling point of this view that it handles bootstrapping so nicely.

 Zalabardo does not, however, take these cases to be counterexamples to the closure principle, which holds that if *S* knows that *p* and knows that *p* entails *q*, then *S* knows that *q*. Rather, they are counterexamples to a *transmission* principle: if *S* knows that *p* and knows that *p* entails *q*, then *S* knows that *q* *on the basis of p*. Zalabardo holds that in many of these cases we do know the conclusions, we just don’t know them on the basis of these inferences. We have standing knowledge that we’re not brains in vats, and the Moorean inference neither adds to nor detracts from that knowledge. We have inferential knowledge, on the basis of our general background information about zoos, that the animals are not painted mules. He even allows (110) that one might need to know the gauge is reliable in order to know that the tank is full, even in a context where one can’t know the former on the basis of the latter.

 He will go on to offer counterexamples to closure, but it is surprising that he bends over backward to refrain from doing so when confronted with these classic putative examples of nonclosure. His thinking appears to be that the abstract, general principle of closure is sufficiently intuitively compelling that we should maintain as many instances of it as possible. But this seems wrong: the general closure principle is intuitively plausible, but no more than PI, and it turns out to be false. Nonclosure is *consistent* with some “abominable conjunctions” (DeRose 1995), like ‘*S* knows that there’s a red barn but doesn’t know there’s a barn’. But it doesn’t imply anything about particular cases; it’s only the overarching theory of knowledge that can do that. The closure denier merely has to show that the overarching theory doesn’t license the genuinely abominable instances. There’s no apparent benefit to denying nonclosure in cases where closure intuitively fails.

 Thus, I think Zalabardo’s attempt to distance himself from his own rejection of closure is relatively undermotivated. I also suspect that it is unsuccessful. Surely we often *do* have knowledge about how zoos work, but presumably such knowledge isn’t necessary for us to track zebras; for this we only need adequate visual match templates and a cooperative environment. Similarly with the fuel gauges: it’s possible to track the level of fuel in the tank without knowing that the gauge is reliable.

 This diffidence about closure is reflected in the treatment of the relevant skeptical arguments. For example, I know that having hands entails that I’m not a brain in a vat. From closure and the plausible claim that I don’t know I’m not a brain in a vat, it would follow that I don’t know I have hands. Dretske and Nozick, of course, deny the conclusion by denying closure. Zalabardo denies it by insisting that I have standing knowledge that I’m not a brain in a vat. Maybe it’s a virtue of Zalabardo’s account that it’s different from, and maybe more internalist-friendly than, Nozick’s and Dretske’s. But it remains to be seen how many internalists appreciate the overture, especially when the only thing standing beliefs have going for them, epistemically, is that they happen to be true.

 The book ends with a kind of skeptical argument that Zalabardo thinks even his theory of knowledge can’t adequately answer. Another form of easy inference is the “cognitive self appraisal” (CSA): when I believe that *p*, I generally also believe that my belief that *p* is true. The higher order belief—the CSA—is entailed by the first order belief, but PI prevents the latter from serving as evidence for it. CSAs don’t generally track truth, and they aren’t standing beliefs or derivable from them. Thus, most of our CSAs are not knowledge. Because this doesn’t impugn our first order beliefs (because of nonclosure), one might find this to be a fairly innocuous result. Zalabardo, however, takes it to be a form of skepticism serious enough that we need to reject realism to avoid it. The idea (which I’m treating far too briefly!) is that if we abandon the realist notion that successful cognition is cognition that hits the target of truth, and replace the goal conception with a specification of the principles we use in attributing success and failure, the distinction between the first order beliefs and CSAs of them will disappear; the epistemic gap and consequent skepticism will disappear along with it. Apparently, if epistemology can’t answer this form of skepticism, we’ll need a metaphysics of truth that can.

 This quasi-realist proposal is, he admits, a sketch, and much more would be needed for a full assessment of it. I worry that it might undo some of the impressive work of earlier chapters, especially concerning bootstrapping, by effectively weakening PI. If my belief that the tank is full *just is* the belief that my first order belief (that the tank is full) is true, then it is unclear what might block the induction that I have a large number of true beliefs—produced by the gauge—and thus that the gauge is reliable. But this is the problematic bootstrapping inference we wanted to avoid. We can’t be sure, however, until this interesting idea is worked out with the same, admirable level of detail as the other parts of this book.

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