teorema
Vol. XXXIII/3, 2014, pp. 00-00
ISSN: 0210-1602
[BIBLID 0210-1602 (2014) 33:3; pp. 00-00]

Repuestas a mis críticos/Replies to My Critics

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I feel immense gratitude to the nine philosophers who have taken the time to consider my ideas and share their reactions with me. I've learnt a great deal thinking through their objections. Addressing them adequately would require rewriting the book. All I can offer here is a few hints of where I might hope to find satisfactory answers to the questions that they raise.

REPLY TO MURRAY CLARKE

Murray Clarke takes issue with my discussion, in chapter 2, of BonJour's attack on reliabilism. Clarke appears to accept my construal of BonJour's main argument as trying to derive the conclusion that reliabilism is wrong from the following two premises:

1. Reliabilism entails that a belief can be knowledge even if the subject is epistemically irrational and irresponsible in holding it (relative to his own subjective conception of the situation).
2. A belief can't be knowledge if the subject is epistemically irrational and irresponsible in holding it (relative to his own subjective conception of the situation).

On my construal, BonJour supports premise 1 with the contention that Norman's belief that the president is in New York City is the kind of belief that, according to premise 1, reliabilism treats as possible: reliabilism ascribes to this belief the status of knowledge but Norman is epistemically irrational and irresponsible in holding it (relative to his own subjective conception of the situation).

I argue, to the contrary, that on the most plausible construal of the notion of epistemic rationality and responsibility, Norman's belief doesn't exhibit those shortcomings. On my construal of the notion, the following principle is correct:

ER2* If a subject has done her best by her lights to determine the truth value of a proposition *p*, then from the point of view of her conception of her epistemic situation, it is epistemically rational and responsible to believe that *p* just in case she believes that *p*.

Clarke appears to concede that, on this conception of epistemic rationality and responsibility, Norman's belief has these features. However, he complains that BonJour should be read as invoking a different conception of epistemic rationality and responsibility — one according to which, in order to be epistemically rational and responsible one would need to be:

> sufficiently objective to carefully look for possible evidence against what one believes and only believe that *p* once one had objectively good reasons in hand.

Call this *Clarke's constraint*. I think it's open to question whether Norman's belief satisfies Clarke's constraint. One could argue that Norman has objectively good reasons for believing that the president is in New York City, since the clairvoyant power on which his belief is based is objectively reliable. But let me concede that Norman's belief doesn't satisfy Clarke's constraint — that, on this construal of epistemic rationality and responsibility, Norman's belief doesn't have these features. Then someone who accepts Clarke's constraint will have to accept premise 1 of BonJour's argument.

My problem with this reading is that it results in a significant reduction of the dialectical appeal of the argument. Our goal, remember, is to consider whether BonJour has provided a cogent argument for the evidential constraint on knowledge. On Clarke's reading, BonJour has provided an argument for this conclusion that will have to be accepted only by those who subscribe to Clarke's constraint. But someone who doesn't accept the evidential constraint on knowledge is unlikely to accept Clarke's constraint: if you think that it is possible to have knowledge in the absence of objective reasons or evidence, then you are likely to think that it is possible to be epistemically rational and responsible in the absence of reasons and evidence.[1] If this is right, then BonJour's argument, on Clarke's reading, would be of very little use in the dialectical battle between supporters of the evidential constraint and their opponents. Clarke thinks that my reading of BonJour violates the principle of charity, on the grounds that his alternative reading is more favourable to BonJour. I don't think this is right. On Clarke's reading, BonJour's argument has no power to compel opponents of the evidential constraint to change their minds. Charity doesn't favour Clarke's reading over mine.

Clarke then gives an accurate characterisation of my methodology in the analysis of knowledge. I am assuming that an analysis is correct just in case it provides the most charitable and illuminating systematization of our intuitions concerning the circumstances under which people know things. Clarke is right to point out that this raises difficult issues concerning, among other things, whose understanding of the concept of knowledge I take my intuitions to represent. There is a significant body of recent literature on these issues that SRB doesn't engage with. Clarke is right to point out that this is a weakness of the book, and that more work is required in order to assess my proposals in light of these methodological issues.

REPLY TO TIM BLACK

Tim Black's paper focuses on my rejection, in Chapter 3 of SRB, of Nozick's idea that the sensitivity condition should be relativized to the method employed in forming the belief. According to Nozick, sensitivity should be formulated along the following lines, where *M* is the method actually employed by *S* in forming her belief that *p*:

> *Methods-1*: If *p* weren't true and *S* were to use *M* to arrive at a belief as to whether *p*, then *S* wouldn't believe, via *M*, that *p*.

My complaint about Methods-1 concerns its application to cases in which *M* is a one-sided method — i.e. one that can recommend belief in *p* but cannot recommend belief in not-*p*. The problem is that in these cases Methods-1 may force us to consider worlds that are very remote. If *M* is one-sided, then it won't produce belief in not-*p* in any nearby world. If in addition, *M* won't produce belief in *p* in any nearby not-*p* world, it follows that the antecedent of Methods-1 won't be true in any nearby world. Hence the epistemic status of *S*'s actual belief that *p* will depend on how things stand in very remote worlds.

Black accepts my objection to Methods-1, but hopes that it can be overcome by a different formulation of method-relative sensitivity. His proposal is to formulate the notion in the following terms:

> *Methods-2*: If *p* weren't true, then if *S* were to use *M* to arrive at a belief as to whether *p*, then *S* wouldn't believe, via *M*, that *p*.

Black claims that his proposal yields the right result in the example I discuss in SRB [pp. 58-60], of a doctor who believes that a patient has a condition on the basis of a positive result in a clinical test with virtually no false positives but lots of false negatives. Intuitively we want to say that the doctor knows that the patient has the condition. Black claims that Methods-2 delivers this result.

I'm not sure I accept Black's reasoning for the conclusion that the doctor's belief satisfies Methods-2. Here is the argument:

> But what about *Methods-2*? It demands in the first place that we examine the nearest worlds in which the condition is not present in the patient. It then demands that we look among all and only those worlds in which the test produces a belief as to whether the condition is present. But there are no such worlds: the nearest worlds in which the condition is absent are worlds in which the test comes back negative and therefore recommends no belief. [...] But this means that the doctor's (actual-world) belief that the condition is present is sensitive: the nearest world in which the condition is absent are worlds in which the test fails to produce a belief as to whether or not the condition is present and therefore fails to produce the belief that the condition is present.

If I understand this correctly, Black must be invoking a principle along these lines (with $\rightarrow$ representing the subjunctive conditional):

> B: If in the nearest worlds in which p is true we have that q is false but r is true, then $p \rightarrow (q \rightarrow r)$ is true.

Black seems to be invoking this principle when he argues that the doctor's belief is sensitive: her belief satisfies Methods-2 because in the nearest worlds in which p is false, we have that S doesn't use M to arrive at a belief as to whether p and S doesn't believe, via M, that p.

But it seems to me that B is false. Take the following proposition:

> If I went to see a good movie, then if a fire started in the theatre after the opening credits, I would stay until the end of the movie.

It seems obvious to me that this proposition is false. However, B would make it true: in the nearest worlds in which I go to see a good movie, there is no fire and I stay until the end of the movie. B has to be rejected, but in its absence I can't see how Black could argue that the doctor's belief satisfies Methods-2.

In fact, it seems to me that Methods-2 faces exactly the same problem as Methods-1. To determine its truth value in a given case, we need to look at the nearest worlds in which p is false, and then look at the nearest worlds to these in which p is false *and S* uses M to arrive at a belief as to whether p, *no matter how distant they are*, and check whether in these worlds S believes p using method M. The problem is, as with Methods-1, that if M is a one-sided method, and if M won't produce belief in p in any nearby not-p world, the worlds we need to look at in order to determine the truth-value of an instance of Methods-2 will be very remote. If we accept, as Black does, that this is a problem for Methods-1, then Methods-2 also has to be rejected. Black's proposal doesn't overcome the difficulties that I raised for Nozick's method-relative notion of sensitivity.

Black then goes on to discuss whether Methods-2 yields the right results for my modification of Nozick's grandma case: a grandmother forms the belief that her grandson is well by consulting her crystal ball, in a way that would produce the belief that he is well in any nearby world, but if the grandson weren't well, others would make sure that she doesn't find out and, among other things, would destroy her crystal ball [SRB, p. 61]. I claim, and Black agrees, that grandma doesn't know that her grandson is well. However, according to Black, my own account of truth tracking doesn't deliver this result. Black makes the point with the notion that he labels 'Zalabardo-sensitivity':

> Your belief that *A* is Zalabardo-sensitive just in case "you are unlikely to believe *A* if *A* is false".

According to Black, "Grandma's belief that her grandson is well is Zalabardo-sensitive: Zalabardo himself maintains 'that if the grandson were unwell or dead, others would make sure that she doesn't find out' [SRB, p. 61]".

I can't see how it follows from this that Grandma's belief is Zalabardo-sensitive. This would require that Grandma is unlikely to believe that her grandson is well if her grandson is not well. But the stipulation quoted by Black ensures precisely that the condition is *not* satisfied. If the grandson is not well, then others will make sure that she is still *likely* to believe that he is well. Black has not shown that my account of truth tracking yields the wrong result in this case.

REPLY TO LARS BO GUNDERSEN AND JESPER KALLESTRUP

Lars Bo Gundersen and Jesper Kallestrup provide a very illuminating account of how my proposal of treating inferential knowledge as a separate sufficient condition for knowledge can deal with some of the problems that Nozick sought to address by relativizing truth tracking to methods. They then claim that if my proposal is adopted, relativizing truth tracking to methods no longer faces the problems that I presented as recommending the rejection of this move: "these problems seem to evaporate once we follow Zalabardo in paying attention to the crucial role played by inference in knowledge acquisition". Their proposal seems to be that in the context of my analysis of knowledge, relativizing truth tracking to methods would not face the problems that it faces as part of Nozick's account.

If I understand Gundersen and Kallestrup's point, I can't see how it undermines my position. They observe, rightly, that Nozick imposes on inferential knowledge the condition that the subject wouldn't believe the premise if the conclusion were false. A probabilistic version of this constraint (principle PI) is part of my own account of inferential knowledge [SRB, p. 98]. I agree that inferential knowledge, construed along these lines, doesn't face the problem that I raised for one-sided methods in section 3.4. But this is not to say that we wouldn't face the problem if we construed knowledge of *H* as truth tracking relativized to the method of inferring *H* from *E*. In the one-sided case, i.e. when *E* supports *H* but $\neg E$ would provide only very weak support for $\neg H$, the problem would still be present: the nearest world in which I form a belief as to whether *H* with this method will be a remote world. In sum, allowing knowledge to result from inference subject to a constraint along the lines of Nozick's I or my PI is not the same as construing knowledge of the conclusion in terms of truth tracking relativized to the method consisting in inferring it from the premise. Gundersen and Kallestrup are right that the former approach doesn't face problems with one-sidedness, but the latter still does. Treating inferential knowledge as a separate sufficient condition for knowledge doesn't remove the problems that I raised for the method-relative version of truth tracking.

Gundersen and Kallestrup then go on to raise some questions for my proposals as to how to deal with cases in which closure appears to be in conflict with other intuitions.

First they highlight the contrast between my approach to the BIV scenario and Hawthorne's safari case [SRB, pp. 142-43]. In the former case my verdict is that there is no failure of closure, since I know both that I have hands (by truth tracking) and that I am not a brain in a vat (by default). In the latter case I claim that we need to accept that we face a counterexample to closure: I know that I won't go on a safari, but I don't know that my lottery ticket won't win, even though I know that the former entails the latter. Gundersen and Kallestrup claim that my verdict on the safari case is based on the "standard tracking theory". This gives rise to their challenge: "The pressing question is: given that such theory yields identical predictions about the two cases, what's the more principled reason for treating them differently?" The answer is that my verdicts are not based on the standard tracking theory, but on the account of knowledge that I defend in SRB. This account contemplates truth tracking as one of the ways in which a true belief can achieve the status of knowledge, but there are others. The reason why I know that I'm not a brain in a vat is that I satisfy one of the sufficient conditions for knowledge contemplated by the account (default knowledge). The reason why I don't know that my lottery ticket won't win is that my belief doesn't satisfy any of the sufficient conditions for knowledge contemplated by my account (it can't be default knowledge because it's not a standing belief). This is what I would offer as my principled reason for treating the two cases differently.

Second, Gundersen and Kallestrup take issue with my treatment of the case in which Roxanne infers that her petrol gauge is working reliably with an inductive argument from premises concerning readings of the gauge and the contents of the petrol tank at the time, with her beliefs concerning the contents of the tank based solely on gauge readings [SRB, pp. 104-07]. I claim that Roxanne doesn't know that the gauge is reliable. Gundersen and Kallestrup protest: "We need an explanation of why you couldn't have inferential knowledge or perhaps default knowledge of the consequent". The reason why Roxanne can't know by default that the gauge is reliable is fairly clear: her belief to this effect is not a standing belief. And the reason why she doesn't have inferential knowledge is that she violates principle PI: given her state of information, she would be as likely to believe the premises of her bootstrapping argument if the conclusion were false as if it were true.

Third, Gundersen and Kallestrup target my contention that cognitive self-approvals can't have the status of knowledge. They claim: "We need to know more about why beliefs that are so-called cognitive self-approvals cannot have the status of — inferential or default — knowledge." The reason why CSAs can't be known inferentially is provided by my argument in sections 7.5-7.7 of SRB. This is an intricate argument to which I have nothing to add. If the argument goes through, then on my account of inferential knowledge, CSAs can't have the status of knowledge in this way, at least after we are exposed to sceptical reasoning. Concerning knowledge by default, my answer would be, in the first instance, that CSAs can't have the status of knowledge by default because, for any *p* in which I don't have a standing belief, my belief that *p* is true is also non-standing, and hence not a candidate for default knowledge. However, this answer is challenged in Adam Leite's paper. I will come back to this issue in my reply to Leite.

Gundersen and Kallestrup close by challenging the disparity between my treatment of Saul Kripke's version of the fake-barn case [SRB, pp. 122-23] and my treatment of Alvin Goldman's Dack the dachshund case [SRB, pp. 127-28], since, as they point out, the two cases are structurally similar. Gundersen and Kallestrup are right that, on a certain understanding of the fake-barn scenario, this case should receive the same verdict as the Dack the dachshund case. I will expand on this point in my reply to Fred Adams, who has raised a similar issue.

REPLY TO VALERIANO IRANZO

Chapter 4 of SRB puts forward an account of evidence — of when a state of affairs *E* provides adequate evidence for a state of affairs *H*. This is the aspect of the book that Valeriano Iranzo focuses on. He provides a very insightful account of the motivation for the view that I defend on this point, but he then raises several issues for my proposal. He focuses, in particular, on my defence of the claim that a high value for $LR(H, E)$ (i.e. $P(E|H)/P(E|\neg H)$) should be treated as a necessary condition for evidential support. Iranzo doesn't take issue here with my main argument for this claim, as he has discussed it elsewhere in joint work.[2] He concentrates instead on some additional advantages that I claim for my proposal.

First, he considers my claim that likelihood measures of incremental confirmation are preferable to probability measures because the former, unlike the latter, use only conditional probabilities, and conditional probabilities are often well defined while the corresponding unconditional probabilities are not. Iranzo argues, to the contrary, that "Probabilistic assignments are always relative to the agent's set of degrees of belief. So, strictly speaking, all probabilities which take part in Bayes' Theorem are conditional ones". On Iranzo's proposal, then, $P(H)$ and $P(E)$ are to be understood as shorthand for the probability of H and E conditional on our background beliefs.

Iranzo's proposal may well work for a Bayesian who is keen to avoid commitment to unconditional probabilities. Unfortunately, however, it wouldn't work for me. Unlike Bayesians, I am not thinking of probabilities along subjective, doxastic, lines, as degrees of credence. The probabilities that I invoke in SRB arise from objective features of the nomological order, independently of anyone's beliefs. Making unconditional probabilities implicitly conditional on our background beliefs would go against the spirit of the conception of probability that I want to use.

Iranzo adds that in the case of clinical testing, he "cannot discern a substantial difference between likelihoods and priors":

> After all, what is required to ascertain those values are empirical data about frequencies. And obtaining the relevant information for the priors — the rates of asthma and lung cancer in the population — does not involve radically different procedures to those developed concerning likelihoods. So there is no reason why priors cannot be as well defined as likelihoods in contexts like these.

Once more, Iranzo's proposal doesn't seem to me to be compatible with the account of probability that I want to use. If probabilities are defined as frequencies, then the probability, say, that patient *A* has asthma will be implicitly relative to a reference class. Then, as Iranzo suggests, unconditional probabilities won't be more problematic than conditional probabilities. However, on the account of probability that I want to use, relativity to a reference class is not built into the notion. The probability that *A* has asthma will have to be treated without reference to a specific population of which *A* is a member. And in these circumstances, I claim, the notion is not well defined.

Second, Iranzo takes issue with my contention that the fact that the value of LR(H, E) can be high even if H or E is very probable should be treated as an advantage of my proposal.

Concerning highly probable hypotheses, Iranzo claims that it's plausible that they cannot be confirmed to a high degree, since when P(H) is high, "the distance to the maximum value for P($H|E$) is shorter", and "it is precisely this distance that *incremental* confirmation is concerned about". I'm not sure that the intuition that Iranzo invokes should carry the weight that he attributes to it. Perhaps *in some sense* incremental confirmation measures the distance between P($H|E$) and P(H). However, all the measures of confirmation other than PD make room for cases in which P($H|E$) – P(H) < P($H^*|E^*$) – P(H^*) but E confirms H to a greater degree than E^* confirms H^*. Given this, there is no reason to assume that when the distance between P($H|E$) and P(H) is small E can only confirm H to a small degree.

Concerning highly probable evidence, Iranzo argues that you would expect it to provide only limited support for a hypothesis, in light of the intuitive evidential bonus of unexpected events. I don't think Iranzo's position accords with intuition. Suppose I've bought a lottery ticket, and I've decided to buy a car — a Ford if I don't win the lottery and a Porsche if I win. Then it seems to me that my not winning the lottery should in principle support the hypothesis that I'll buy a Ford to the same degree to which my winning the lottery supports the hypothesis that I'll buy a Porsche, even if the probability of my not winning is assumed to be arbitrarily high.

Third, Iranzo takes issue with my treatment of deductive evidence. If E deductively entails H, then the denominator of LR(H, E) is 0, and hence its value is undefined. In SRB I propose to stipulate that in these cases LR(H, E) has maximal (infinite) value. Iranzo's main concern here is that it might lead to infinite degrees of confirmation in empirical, non-deductive situations. As an example of this, he considers a clinical test with no false positives. Once again, it seems to me that our disagreement comes down to the conceptions of probability with which we are operating. On a frequency interpretation of probability, it might be that if we get no false positives with a sufficiently high number of trials we have no option but to ascribe to false positives a 0 probability. But on the conception of probability that I am presupposing, probabilities won't have to coincide with actual frequencies, and the nomological facts from which probabilities arise can be expected to always yield non-zero values for P($E|\neg H$) when E doesn't logically entail H.

Iranzo closes with an alternative proposal for analysing evidential support. I want to focus on one aspect of his proposal. He agrees with me that a value for P($H|E$) above some threshold higher than 0.5 but lower than 1 is a necessary condition for evidential support, but he adds, as another necessary condition, that P(H) has to be below this threshold. I understand his motivation for introducing this condition, and I deal with the issue that he is seeking to address in my reply to Miguel Ángel Fernández. However, it should be clear that on my objective construal of probability the proposal can't possibly work, as it entails that we can't have evidential support for very probable propositions, and hence that these propositions can't be known inferentially.

REPLY TO MIGUEL ÁNGEL FERNÁNDEZ

Miguel Ángel Fernández focuses on how SRB draws the distinction between inferential and non-inferential knowledge. According to the account of knowledge defended in SRB, a belief can acquire the status of knowledge by tracking the truth. Since a belief can track the truth even if the subject has no adequate evidence in its support, this feature of the account licenses counterexamples to the evidential constraint. But according to SRB a belief can also acquire the status of knowledge as a result of the subject being in possession of adequate evidence in its support. In Nozick's original truth-tracking account of knowledge, truth tracking was not only sufficient but also necessary for knowledge. Hence evidence could produce knowledge only if as a result of its acquisition your belief came to track the truth. The account of knowledge advanced in SRB differs from Nozick's account in this respect: a belief can have the status of knowledge if the subject has adequate evidence in its support even if the belief doesn't track the truth. As a result, inferential and non-inferential knowledge are independent of one another in both directions: non-inferential knowledge is possible in cases in which the conditions for inferential knowledge are not satisfied and inferential knowledge is possible in cases in which the conditions for non-inferential knowledge are not satisfied. In SRB I express this mutual independence by saying that the distinction between inferential and non-inferential knowledge is fundamental.

However, as Fernández nicely spells out in detail, on the accounts that I offer in SRB, inferential and non-inferential knowledge exhibit important parallels. In a nutshell, in order for my belief that *p* to track the truth, the state of affairs of my believing *p* has to be related to *p* in such a way as to constitute adequate evidence for *p*. Tracking the truth doesn't require being in possession of adequate evidence, but it amounts to 'embodying' adequate evidence. Fernández complains that this fact is incompatible with my claim that the distinction between inferential and non-inferential knowledge is fundamental. He might be right that the similarities between the two types of knowledge render the term 'fundamental' inappropriate. However, the important question is, I think, whether my account brings inferential and non-inferential knowledge closer together than they should really be. I don't think Fernández offers support for this claim. He claims that if knowledge is construed as I propose, then "we should conclude that NIK [non-inferential knowledge] and IK [inferential knowledge] are two manifestations of the same fundamental epistemic phenomenon". This strikes me as a welcome result. This fundamental epistemic phenomenon is what we call knowledge.

Fernández then complains that my account misclassifies some intuitive instances of non-inferential knowledge as inferential. He illustrates his point with Nozick's grandmother case and Goldman's Dack the dachshund case. In these cases, Fernández claims, the subjects "*see* to be the case *exactly the same thing* that they thereby come to know to be the case". On these grounds, they should be treated as cases of non-inferential knowledge. I want to argue, to the contrary, that there are good reasons for treating these as cases of inferential knowledge. Let me focus on the grandmother case. My proposal is to take her belief that her grandson is well as inferential knowledge, arising from the evidence provided by a proposition *E* describing his appearance on the occasion of his visit to his grandmother [SRB, p. 126]. For Fernández, *E* doesn't play any role in the epistemic status of the grandmother's belief that her grandson is well. If he were right, then the epistemic status of the grandmother's belief would not be contingent on the relationship between *E* and the proposition that the grandson is well (WELL). However, it seems to me that this contingency does obtain — the belief would not be knowledge unless *E* provided adequate support for WELL. Suppose that as a matter of fact *E* doesn't provide adequate support for WELL. Suppose that the grandson's looks are not correlated with his state of health in the right sort of way, e.g. that the probability that the grandson is well given that he looks like that is not much higher than .5. I claim that if this were the situation the grandmother would not come to know that her grandson is well as a result of his visit. This dependence of the epistemic status of the grandmother's belief on the evidential link between *E* and WELL is incompatible with treating her belief as a case of non-inferential knowledge. The same point can be made with respect to the Dack the dachshund case.

I think this point can also be used to address Fernández's concern that if we accept my construal of these cases, we will end up treating all knowledge as evidential, since

> it seems always possible to find the sort of evidential proposition(s) and the sort of evidential connections between them and the belief of the subject, that Zalabardo finds in the grandmother and the Oscar cases and that renders them cases of IK.

My reply to this would be that in order for a belief in a proposition *H* to count as a case of inferential knowledge based on evidence *E*, it's got to be the case that the epistemic status of the belief is contingent on the truth of *E*, on the obtaining of the evidential connection between *E* and *H*, on the subjects belief in *E* and in the connection, and on the epistemic status of these beliefs. This will still leave some cases in which a belief counts as both inferential and non-inferential knowledge, but in these cases there won't be, as Fernández suggests, *uncertainty* as to how to classify them. They will simply be cases of over-determination, where a belief satisfies more than one sufficient condition for knowledge. I think this phenomenon is perfectly common. We encounter it in cases in which a belief tracks the truth and the subject is aware of the features of his epistemic situation that make her belief track the truth.

Reply to Fred Adams

Fred Adams offers a very interesting critical discussion of the relative merits of my analysis of knowledge with respect to Nozick's original tracking account. He first complains that my conditions for truth tracking are not sufficiently strong. On my account, Tom's belief that Mandela died will track the truth so long as (a) Tom is much more likely to believe that Mandela died if Mandela is dead than if he is alive and (b) the probability that Mandela died, given that Tom believes that he died, is sufficiently high. Adams's complaint is that this makes room for Tom's belief counting as knowledge even though it is possible (although unlikely) that he believes falsely that Mandela died. Adams is certainly right that my account has this consequence, but whether this should be regarded as a problem for the account depends entirely on where one stands on the fallibilism/infallibilism debate. For fallibilists, like me, the feature that Adams highlights is precisely the right result. This is not the place to defend fallibilism. So my point is simply that whether Adams has found a problem here depends entirely on the outcome of this debate.

What's not so clear to me is that Nozick's account is free from this consequence. Adams writes: "On traditional tracking accounts if Tom even *might* still believe Mandela is dead when Mandela is alive, Tom doesn't know Mandela died". I'm not sure this is true of Nozick's account. If Tom's belief tracks the truth, then in the nearest world *w* in which Mandela is alive Tom doesn't believe that he is dead. But this is compatible with the existence of worlds, only marginally more distant from actuality than *w*, in which Mandela is alive but Tom believes that he is dead. In this situation it seems reasonable to say that 'Tom might still believe Mandela is dead when Mandela is alive', but, on Nozick's account, Tom knows that Mandela is dead.

Adams blames this feature of my account on the fact that while "traditional accounts of truth tracking forge a lawful connection between the facts and the true belief", my account 'diminishes' that lawful connection, thereby robbing tracking theories of their strongest virtue. Adams is right that lawful connections don't figure directly in my account of truth tracking, but they do figure in it indirectly: truth tracking is defined as a probabilistic link between belief and the facts, but on the construal of probability that I favour [SRB, p. 69], these probabilistic links are generated by lawful connections. This point might also speak to Adams's complaint that unless we invoke something like

Nozick's methods or Dretske's reasons, "a tracking account would make it look like magic that beliefs correspond with the truth, when they do". The correspondence between beliefs and the truth that I call truth tracking is a probabilistic link. This isn't generated by magic, but by the nomological order in which probabilistic facts are grounded.

Adams then takes issue with my treatment of Saul Kripke's version of the fake-barn case. First of all let me apologize for the confusion I may have created by swapping the colours of the barns. Since swapping them back at this point might only exacerbate the problem, I'll stick to the colour scheme of SRB: I am driving through an area that contains red barns, blue barns, and red barn fakes, but no blue barn fakes. I know nothing about the fakes and their colours. I see a blue barn and form the true belief that that's a blue barn (BLUEBARN). Is my belief knowledge? Kripke argued that in this case Nozick is committed to an inadmissible failure of closure: he has to say that I know that it's a blue barn, since my belief tracks the truth, but I don't know that it is a barn (BARN), since my belief to this effect doesn't track the truth, even though I know that BLUEBARN entails BARN.

In SRB I argue that, on my account, my belief in BLUEBARN doesn't track the truth. The reason, in a nutshell, is that, although there are no blue fakes, the probability of blue fakes is made high by the existence of red fakes. I think Adams accepts that *if* the probability of blue fakes were high, as I claim, then I would not know BLUEBARN. Our disagreement concerns the probability of blue fakes. I claim it is high, but according to Adams "the objective physical probability (or likelihood) of a blue barn façade is zero".[3] I accept that if he is right about this, then I have to count as knowing BLUEBARN. Who is right? I think we are both right. Our disagreement can be traced back to how we spell out the details of Kripke's case.

Kripke's objection to Nozick did not appear in print until 2011, when the final draft of SRB had already been submitted to the press. Prior to that, it had been known through a circulated typescript and hearsay. I didn't see the text until its publication. In SRB I rely on second-hand accounts of Kripke's case. One feature of the version of the case that I consider in SRB is crucial to the issue under discussion: I was assuming that the fact that none of the fakes was blue was simply a coincidence — that it just so happened that all the fakes had been painted red.[4] Now, on this assumption, I still claim, the probability of blue fakes is made high by the existence of fakes of other colours, even if, as a matter of fact, none of the fakes is actually blue. If whether a structure is a barn or a fake is stochastically independent of whether it is blue or red, then the probability that a blue structure is a fake is the same as the probability that a structure of either colour is a fake. Hence, on my assumption, it follows, as I argue in SRB, that my belief in BLUEBARN doesn't track the truth.

But Adams's description of the case differs from mine in this respect. On his description of the case (adapted to my colour scheme) blue barns "nomically cannot be faked". This way of filling it the details, I know now, is more faithful to Kripke's original discussion. In one of the versions of the case that Kripke considers, "for some chemical reason the cardboard in the counterfeit barns cannot be painted [blue]".[5] When the case is described in this way, Adams's verdict is absolutely right: the probability of a blue fake is *not* increased by the existence of fakes of other colours. We can stipulate this probability to be very low, and then, as Adams suggests, I will have to count as knowing BLUEBARN, since my belief will track the truth. Notice, however, that this doesn't land me with Kripke's problem. My belief in BARN doesn't track the truth, but I can still know BARN inferentially on the basis of the evidence provided by BLUEBARN.[6] My verdict on this version of the case brings it in line with the Dack the dachshund case, which, as Adams and Fernández have pointed out, exhibits the same structure.[7]

Another case on which according to Adams my account yields the wrong verdict is Dretske's Rockaford example:

> Tom wants a new Porsche but can't afford one. However, his rich friend Rockaford offers to buy Tom a Porsche, if Tom doesn't win the lottery (a fair lottery). Tom doesn't win and Rockaford does buy Tom a Porsche. Sue knows only of the arrangement, but not of the results of the lottery. When Sue sees Tom driving his new Porsche, she correctly assumes that he got it from Rockaford. But does she know?

Adams suggests that this question should be answered in the negative, but that my account dictates that Sue knows that Tom got the Porsche from Rockaford (ROCKAFORD), since her belief satisfies my definition of truth tracking. I don't think Adams is right about this. Given what she knows about the situation, Sue isn't much less likely to believe ROCKAFORD if ROCKAFORD is false than if it is true. If ROCKAFORD is false, then Tom will have won the lottery and bought the Porsche himself, but Sue will still be just as likely to believe ROCKFORD, since her epistemic situation won't be different in any obvious way. It follows that the tracking ratio of her belief is very low, and hence that her belief doesn't track the truth. It can also be argued that her belief doesn't satisfy the conditions for inferential knowledge.

REPLY TO ADAM LEITE

Adam Leite poses some hard questions concerning the argumentative structure of SRB. He focuses first on the claim that, for standing beliefs, truth is a sufficient condition for knowledge. He contends that the claim stands in need of defence. I agree, even if, as I suggest in SRB, there might not be a specific need to defend it *in the eyes of the sceptic*. The reasons that I offer for not imposing probabilistic constraints in this case concern the role that standing beliefs play in our cognitive architecture. Standing beliefs, I argue, should not be subject to the same epistemic standards as beliefs that result from the operation of belief-forming mechanisms. It can't count as an epistemic shortcoming of a standing belief that we are not in a position to detect its truth value. The whole point of standing beliefs is to furnish us with information about our environment without investing in the cognitive devices that would enable us to detect their truth value. The claim that truth is the only appropriate standard of epistemic excellence is not based on the assumption that they are always, or often, true. As Leite points out, they are often false. Nor is it based on the fact that they are the result of evolutionary pressures. This justification would be problematic, since, as Leite reminds us, standing beliefs or the dispositions to form them wouldn't be selected for, directly, at any rate, on the basis of their truth. The reason why standing beliefs count as knowledge whenever they are true is simply that imposing any additional requirements on their epistemic excellence would involve a distortion of the role they are supposed to play in our cognitive life.

Leite then considers the use I make of my line on standing beliefs in my treatment of sceptical arguments based on sceptical possibilities. He argues that even if I have an innate predisposition to form the belief "that my sensory apparatus is hooked up to the world in more or less reliable ways", I don't have an innate predisposition to form the belief that I am not a brain in a vat. Suppose, for the moment, that this is right. Then, since, as Leite argues correctly, my belief that I am not a brain in a vat would not satisfy my conditions for inferential knowledge, we would have to explain the epistemic status of this belief as resulting from "an innate predisposition to form something like *substitution instances* of a general schema". This is important because, as Leite argues, if this form of knowledge is contemplated, it's hard to see how it couldn't be applied to the explanation of the epistemic status of CSAs, thereby offering a solution to the sceptical problem that I develop in chapter 7 of SRB.

But I don't accept Leite's claim that my belief that I am not a brain in a vat is not a standing belief. I certainly don't have an innate predisposition to form it come what may, since I don't have an innate predisposition to acquire the concepts of brain or vat. What I do have, I claim, is an innate predisposition to form the belief as soon as the proposition comes within my cognitive purview. If I can bear any propositional attitude to the proposition that I am not a brain in a vat, I will bear to it the attitude of belief. Hence treating this belief as knowledge does not require, as Leite claims, ascribing this status to beliefs that result from a predisposition to form substitution instances of a general schema.

CSAs don't satisfy the condition that confers the status of standing belief on my belief that I'm not a brain in a vat. If I don't have a standing belief in p, then I don't have an input-independent inclination to form the belief that my belief that *p* is true. Even if I can bear a propositional attitude to this proposition, whether I end up believing it will depend on a definite feature of my specific situation — it will depend on whether I have the belief that *p*. Because of this dependence on (reflective) input, CSAs are not standing beliefs. In order to qualify as knowledge they need to fall under the truth-tracking or evidential provisions.

Leite then presents a more wide-ranging challenge to my verdict on CSAs. He praises my willingness to accept that different requirements might apply to different sorts of knowledge, in order to accommodate our intuitions as to who knows what. But this attitude is in some tension with my claim that CSAs don't have the status of knowledge. We clearly have a strong pre-theoretical intuition against this claim. Hence, if we discover that CSAs don't satisfy any of the three sufficient conditions for knowledge that I contemplate, my methodological approach would seem to recommend introducing a fourth form of knowledge — one that enables us to accommodate our intuition that CSAs have this status.

I think the situation can be usefully characterised in terms of the contrast introduced by Roderick Chisholm between the approaches to the analysis of knowledge that he labelled *methodism* and *particularism*.[8] Methodism approaches the analysis of knowledge by considering in the first instance under which conditions someone should count as knowing something. Once this question has been answered, the methodist moves on to determining, in terms of this answer, which particular instances of belief should count as knowledge. Particularism approaches the task of analysing knowledge in the opposite direction, asking first which particular beliefs should count as knowledge. Once this question has been answered, the particularist tries to formulate necessary and sufficient conditions for knowledge that get particular knowledge ascriptions right.

On a purely particularist approach, Leite's challenge strikes me as incontestable. If we are inclined to ascribe the status of knowledge to a given belief, we have an equally strong inclination to ascribe this status to the subject's belief that this belief is true. From a particularist point of view, my 'sceptical' argument to the effect that CSAs don't satisfy any of my three sufficient conditions for knowledge would have to be taken as establishing that a fourth condition has to be introduced.

I accept that the way in which I approach the task of analysing knowledge in SRB and some of my methodological pronouncements may give the impression that I am committed to an exclusively particularist approach. Leite argues convincingly that if I want to make room for my results concerning scepticism, this cannot be my approach. It's of course not necessary to move to the other extreme — to an exclusively methodist approach. What we need is to strike a balance between the two, aspiring to a reflective equilibrium between our particular intuitions as to who knows what and our theoretical intuitions concerning the conditions under which knowledge is possible. On this approach, my analysis of knowledge cannot be defended exclusively on the grounds that it provides the best match for our particular intuitions. I need to argue, instead, that my account of knowledge provides a better match for our theoretical and particular intuitions that any of the rivals, including those that provide a better match for our particular intuitions by ascribing to CSAs the status of knowledge. Leite's proposal to introduce a fourth sufficient condition for knowledge to accommodate CSAs would have to be dealt with in this way. I believe that SRB contains a battery of arguments that could be deployed in support the methodist superiority of my account of knowledge over rivals that are preferable from a particularistic point of view. But meeting Leite's challenge would require making this case explicitly.

The situation becomes more complex once we take into account, as I propose in chapter 8, the possibility of accommodating our intuitions not with our analysis of knowledge, but with a revision of our metaphysical picture. The contest now is between, on the one hand, my analysis of knowledge together with a yet-to-be-specified revision to our metaphysics that removes the problem of the epistemic status of CSAs and, on the other hand, a metaphysical picture on which CSAs do not satisfy the sufficient conditions for knowledge that I have defended paired with an alternative account of knowledge that incorporates a provision to deal with this lack. The contest will be won by the proposal that offers the best overall match for our particular and theoretical intuitions.

Leite then argues that my line on CSAs cannot make room for the phenomenon of double-checking. I'm not sure I agree with his description of what's going on in these cases. I work out the tip with pencil and paper and I come to know, on the basis of the evidence provided by the calculation, that it is $12.83. You then come up with a different answer. Once this information is added to my body of evidence, it no longer provides sufficient support for the $12.83 answer to turn my belief into knowledge. Consulting the calculator restores the support for the $12.83 answer. My belief is knowledge once again. None of this has to do with the transition from the proposition that the tip is $12.83 to the proposition that my belief that the tip is $12.83 is true. It seems to me that knowledge of the latter should not require anything over and above what's required by knowledge of the former. The hope for my overall position is that its metaphysical aspects will secure this result.

Leite is suspicious of the principle PI. He attacks it with the contention that in Sosa's garbage chute example my position cannot make room for the idea that I know that the garbage has reached the basement. As Leite points out in a footnote, I think this case has the same structure as Vogel's ice-cube case [SRB, pp. 129-32] — they both pose the problem of knowing that the unlikely hasn't happened. It seems to me that Sosa's case can be treated in the same way as Vogel's: I know that the garbage has reached the basement inferentially on the basis of the evidence provided by general truths about the behaviour of garbage bags in garbage chutes. Leite objects that "there is no law of nature concerning what happens when one drops a trash bag down a trash chute". He is right that what happens in these cases isn't governed by a single law of nature, but I can't see how this would make a difference, so long as we accept that the laws of nature as a whole govern the behaviour of garbage bags down garbage chutes.

REPLY TO BARRY STROUD

A central claim of SRB is that the problem of scepticism is not solved by the adoption of an externalist epistemology. Barry Stroud has done more than anyone else to defend this claim, and his work in this area has been a major source of inspiration for the research that resulted in SRB. In his paper, Stroud expresses some important concerns about my specific account of why epistemological externalism doesn't remove the sceptical problem. I think his concerns are genuine. They amount to an agenda for future research and here I can hope to do no more than sketch a few ideas from which an adequate response might one day emerge. I want to focus on two of the points that Stroud raises.

First a relatively minor point. Stroud questions my construal of CSAs as consisting in the ascription of a predicate (truth) to a belief singled out with a definite description:

> I do not see why a singular term referring to a belief of mine must be used in expressing my "cognitive self-appraisal" of my believing what I do. What is in question for Zalabardo in that "appraisal" is my ascription of truth to the belief I have that *p*. And the question whether I can know such a thing can be put by asking whether I can know that in believing that *p* I believe truly that *p*. If I do know that I believe that *p*, that seems to leave me only with the question whether it is true that *p*.

Stroud's idea seems to be that the right construal of a CSA concerning my belief that *p* is as the proposition that I believe that *p* and *p*. Stroud is taking sides in a debate between those who propose to construe reflective beliefs as of the form *I don't believe p falsely* ($\neg(\mathrm{Bel}(p)\ \&\ \neg p)$) and those who construe them as of the form *I believe p truly* ($\mathrm{Bel}(p)\ \&\ p$).[9] Stroud is endorsing the second of these options.

Which of these construals we adopt will have very important consequences for our assessment of the epistemic status of CSAs on a truth-tracking account of knowledge, since my belief in Bel(p) & p will be sensitive so long as my belief in p is sensitive, whereas my belief in $\neg$(Bel(p) & $\neg p$) will always be insensitive. This is a puzzling result, since, intuitively, if we assume bivalence, it's hard to see how my belief in Bel(p) & p could have a better epistemic status than my belief in $\neg$(Bel(p) & $\neg p$).

The puzzle disappears if we construe the propositions that I believe p truly and that I don't believe p falsely as *presupposing* that I believe p. For the truth values of Bel(p) & p and $\neg$(Bel(p) & $\neg p$) come apart only when I don't believe that p. Then Bel(p) & p is false and $\neg$(Bel(p) & $\neg p$) is true. But if I believe p, Bel(p) & p and $\neg$(Bel(p) & $\neg p$) are guaranteed to have the same truth value as one another — true if and only if p. Construing CSAs as presupposing that I believe p explains our intuition that the choice between construing them as of the form *I believe p truly* and construing them as of the form *I don't believe p falsely* should have no major epistemic consequences. This is what is achieved by my proposal.

The second point I want to address is Stroud's suspicion that I haven't identified a genuine target for the sceptic's attacks. The sceptical argument that I develop seeks to raise a problem for the epistemic status of beliefs of the form: my belief that p is true, even if the epistemic status of my belief that p is not in question. However, for Stroud, my belief that my belief that p is true is not really different from my belief that p. Hence, if we have an account of how I can know that p, there isn't a further problem concerning whether I know that my belief that p is true:

> anyone who believes that p already regards it as true that p. To believe something is to take it to be true or to endorse it or put it forward as true. That does not require using or even having a word 'true'. It requires only a conception of something or other's being so. Someone who believes something takes something or other to be so. For anyone who understands what he is saying or thinking in ascribing a belief to himself, there is therefore nothing in need of further explanation in his endorsing or regarding what he believes as something that is so. He regards the belief he ascribes to himself as true.

There are three separate claims one could make concerning the relationship between my belief that p and my belief that my belief that p is true. First, we have the claim that, at least if you have the concept of true belief, you can't have the former without having the latter. Once it is suitably qualified to take account of the fact that further evidence or reflection might lead us to abandon beliefs we used to hold, this claim strikes me as undeniable. It is the reason why my belief that my belief that p is true doesn't track the truth: if my belief that p were false I would still believe that it is true.

Second, one could claim that these beliefs necessarily have the same epistemic status — in particular, that if you know that *p* then you also know that your belief that *p* is true. I argue in SRB that, pending a revision of our background metaphysics, this claim is usually false: if my belief that *p* is not a standing belief, then even if I know that *p* I don't know that my belief that *p* is true, as the second-order belief is a non-standing, non-truth-tracking belief for which I can't obtain adequate evidence.

Third, one could claim that these beliefs necessarily have the same content. My belief that my belief that *p* is true presupposes that I believe that *p*, but subject to this presupposition, the claim goes, it has the same propositional content as my belief that *p*. Call this the *synonymy claim*. Clearly, if the synonymy claim were true, the first two claims would follow as a matter of course. In particular, my claim to have found a suitable target for sceptical reflection would have to be abandoned. Once we accept that I know that *p*, there wouldn't be a further question concerning whether I also know that my belief that *p* is true.

It seems to me that Stroud's suspicion is grounded in the synonymy claim: he doesn't think that I have identified a suitable target for sceptical reasoning because he thinks that my belief that my belief that *p* is true has the same content as my belief that *p*. All I want to do here is to clarify where I disagree with Stroud's position on this point. I agree that if the synonymy claim is true, then the sceptical argument that I present in SRB can't even get started. I also agree that the synonymy claim is true: given that I believe that *p*, my belief that this belief is true has the same content as the first-order belief itself. Furthermore, as I suggest in the very last section of SRB, I think that something along the lines of the synonymy claim will ultimately provide the solution to the sceptical problem.

But after accepting all this, I still think that the sceptical problem developed in SRB can play the role that I ascribe to it. I argue that sceptical reasoning should be seen as exposing the unacceptable epistemological consequences of a realist construal of cognition, and I maintain that a realist construal of cognition is incompatible with the synonymy claim. If this is right, and if there's no solution to the sceptical problem unless we embrace the synonymy claim, then solving the sceptical problem will require abandoning realism.

This is how I expect things to pan out. However, in order to show that this is the right diagnosis of the problem we need to achieve a much better understanding of realism and its relationship to the synonymy claim than what I offer in the last chapter of SRB. On this point, I have recently been struck by the similarity between what I want to say and Huw Price's discussion of the bearing of deflationism about truth and other semantic notions on the debate between expressivism and representationalism. The following passage from a joint paper with David Macarthur is representative:

> Provided we take it that the core of the expressivist position is what we've called a pragmatic account of the key functions of the judgments in question — an account not cast in representational, "descriptive", or semantic terms — then deflationism about the key semantic notions is a *global* motivation for expressivism. It is a global reason for thinking that whatever the interesting theoretical view of the functions of a class of judgments turns out to be, it cannot be that they are referential, or truth-conditional.[10]

If we think of the synonymy claim as a deflationist thesis, of Macarthur and Price's representationalism as a realist construal of cognition, and of their expressivism as the kind of alternative to realism that I am hoping to articulate, then Macarthur and Price's claim is what we need in order to vindicate the relevance of the sceptical problem presented in SRB. If they are right, then the realist is not entitled to the synonymy claim and therefore can't take advantage of its anti-sceptical power: the sceptical argument will have served its anti-realist purpose.

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NOTES

[1] See my discussion of the evidential constraint on epistemic rationality and responsibility [SRB, pp. 32-33].

[2] My case for LR is strengthened, I think, in an article by David Glass and Mark McCartney, "A New Argument for the Likelihood Ratio Measure of Confirmation", forthcoming in *Acta Analytica*.

[3] Adam Leite voices a similar complaint.

[4] I am not alone in construing the case in this way. Here is Stewart Cohen's description (with yet another colour scheme): "The residents of the region picked out all the sites and at each one flipped a coin to determine whether they would put up a real barn or a replica. As it turns out all the replicas are green." [Stewart Cohen, "Structure and Connection: Comments on Sosa's Epistemology"; in *Ernest Sosa and His Critics*, edited by John Greco, pp. 17-21. Malden, MA: Blackwell, 2008, p. 20.]

[5] Saul A. Kripke. "Nozick on Knowledge"; in *Philosophical Troubles. Collected Papers Vol I*, pp. 162-224, Oxford, Oxford University Press, 2011, p. 186.

[6] On this account, knowing BARN will require believing BLUEBARN. If I haven't noticed the colour (suppose I'm colour-blind), then my true belief in BARN won't have the status of knowledge.

[7] Adams has argued in joint work that Nozick's account has the resources for ascribing to my beliefs in BLUEBARN and BARN the status of knowledge. See Fred Adams and Murray Clarke, "Resurrecting the Tracking Theories"; *Australasian Journal of Philosophy* 83 (2005), pp. 207–21.

[8] See Roderick M. Chisholm, *The Problem of the Criterion*; Milwaukee, Wis.: Marquette University Press, 1973.

[9] For the first option see Jonathan Vogel, "Reliabilism Leveled"; *Journal of Philosophy* 97 (2000), pp. 602-23. For the second see Kelly Becker, "Is Counterfactual Reliabilism Compatible with Higher-Level Knowledge?"; *Dialectica* 60, no. 1 (2006), pp. 79–84.

[10] David Macarthur and Huw Price; "Pragmatism, Quasi-Realism, and the Global Challenge"; in *New Pragmatists*, edited by Cheryl Misak, pp. 91-121; Oxford, Clarendon Press, 2007, p. 106.