5

E & ¬H*

Jonathan Vogel

A significant issue arises in epistemology which I call the *problem of misleading evidence*.[1] Suppose you have good inductive evidence E for the hypothesis H, and you are justified in believing H on the basis of E. It is possible that your evidence is misleading.[2] That is, it is possible that E justifies H for you, but H is false. (E & ¬H) is the case, instead.[3] To a first approximation, the problem of misleading evidence is to provide an account of how you can be justified in believing that your evidence isn't misleading. What justification do you have for ¬(E & ¬H)?

Certain philosophical views imply that it is impossible for you to have such justification. In the first place, (E & ¬H) entails E. Since E is exactly the evidence you would expect to have if (E & ¬H) were true, one might doubt that E can be evidence against (E & ¬H). This point generalizes. It appears that:

Entailment Principle. If X entails Y, then Y doesn't justify ¬X.

Of course, a consequence of the Entailment Principle is that E can't justify ¬(E & ¬H).

The Entailment Principle is related to another thesis:

Confirmation Principle. Y justifies X only if Y *confirms* X. That is, Y justifies X only if Pr(X/Y) > Pr(X).[4]

* For their help in writing this paper, my thanks go to Maria Aarnio, Dylan Dodd, Hartry Field, Jeff Horty, Chris Meacham, Jim Pryor, Susanna Siegel, Martin Smith, Jonathan Westphal, Elia Zardini, and an anonymous referee. I am especially grateful to David Christensen and Daniel Greco, who have been so very generous with their time and expertise.

[1] See Vogel (2004, 2007). See also Huemer (2001).

[2] When I use expressions such as 'justification' 'justify', 'justify in accepting', or 'justify in believing' I generally mean the positive, non-graded epistemic status typically discussed by 'traditional' epistemologists.

[3] Since your evidence is inductive, E doesn't entail ¬H, and (E & ¬H) is at least logically possible.

[4] These are epistemic probabilities. This principle, which I reject, is quite closely related to the principle 'LDLC' defended by Brian Weatherson in this volume (Ch. 4). The Confirmation Principle places a condition on the justification of a hypothesis by evidence, while LDLC places a condition on 'learning' a hypothesis on the basis of evidence.

The idea here is that if Y is to justify one's believing X, Y ought to increase how much reason one has to believe X. The unconditional probability Pr(X) is supposed to represent how much reason one has to believe X, apart from any reason provided by Y. The conditional probability Pr(X/Y) is supposed to represent how much reason one has to believe X, given that Y holds. So, if Y increases how much reason one has to believe X, Pr(X/Y) exceeds Pr(X). It follows that Y justifies X only if Pr(X/Y) is greater than Pr(X), as the Confirmation Principle requires. A theorem of the probability calculus is that if X entails Y, then $Pr(\neg X/Y) \leq Pr(\neg X)$.[5] This fact, combined with the Confirmation Principle, implies that if X entails Y, then Y doesn't justify ¬X. In other words, given certain assumptions about the relationship between epistemic probabilities and justification, the Entailment Principle is a consequence of the Confirmation Principle. And, so, the Confirmation Principle implies that E can't justify ¬(E & ¬H).[6]

The problem of misleading evidence now comes into sharper focus. Assume that E is your *total relevant evidence*. If the Entailment Principle holds, E doesn't justify ¬(E & ¬H). It follows that you have no empirical justification for ¬(E & ¬H). There is a second consideration. Many philosophers maintain that there can't be a priori justification for believing contingent propositions. Call this claim *Hume's Principle*. Whether ¬(E & ¬H) holds is a contingent matter. Therefore, Hume's Principle implies that there can't be a priori justification for believing ¬(E & ¬H).[7] Putting these points together, it seems that you have neither empirical justification nor a priori justification for believing ¬(E & ¬H); you have no justification whatsoever for believing that your evidence isn't misleading. Is there any way to avoid this unwelcome conclusion?

The rest of the paper will proceed as follows: Section 5.1 shows how the problem of misleading evidence can serve as the basis for an argument that justification isn't closed under known logical implication. I defend closure for justification. Section 5.2 makes the case that one has empirical justification for believing that one's evidence isn't misleading; this justification is provided the evidence itself. Section 5.3 bolsters this view by appeal to some considerations about theory choice. Section 5.4 addresses an argument that we have a priori justification for believing that our evidence isn't misleading. Section 5.5 responds to a probabilistic argument, similar to Roger White's well-known objection to dogmatism, that there can't be empirical justification for believing that one's evidence isn't misleading. Section 5.6 defends dogmatism against White's objection, and proposes a way to deal with a certain version of scepticism about induction. An overarching theme is that both the Entailment Principle and the Confirmation Principle are false.

[5] Proof: Assume X entails Y. (i) If so, $Pr(Y/X) = 1$. (ii) From (i), $Pr(Y/X)\, Pr(X)/Pr(Y) = Pr(X)/Pr(Y)$. (iii) Assuming that Pr(Y) is such that $0 < Pr(Y) < 1$, then $Pr(X)/Pr(Y) > Pr(X)$. (iv) From (ii) and (iii), $Pr(Y/X)\, Pr(X)/Pr(Y) > Pr(X)$. (v) According to Bayes's Theorem, $(X/Y) = Pr(Y/X)\, Pr(X)/Pr(Y)$. (vii) So, from (iv) and (v), $Pr(X/Y) > Pr(X)$. (viii) From (vii), $1- Pr(X/Y) < 1- Pr(X)$. (ix) From (viii), $Pr(\neg X/Y) < Pr(\neg X)$.

[6] In the first part of this paper, I approach matters in qualitative terms, and my concern is primarily with the Entailment Principle. I turn to the role and status of the Confirmation Principle in §5.

[7] Examples of the contingent a priori proposed by Saul Kripke (1980) provide no reason to think that there is a priori justification for believing ¬(E & ¬H). Hume's Principle should be understood as restricted to 'deeply contingent' propositions. For discussion of this issue, see Hawthorne (2002) and Weatherson (2005).

5.1. Misleading Evidence, Epistemic Closure, and Scepticism

According to the *Closure Principle for Justification*, if someone is justified in believing a proposition, then she is also justified in believing any other proposition she knows to be entailed by the first. In symbols:

1.1. (CJ) If $J(X)$ and $K(X \Box Y)$, then $J(Y)$.[8]

CJ brings us face-to-face with the problem of misleading evidence. An instance of CJ is:

1.2. If $J(H)$ and $(H \Box \neg(E \,\&\, \neg H))$, then $J(\neg(E \,\&\, \neg H))$.

Suppose you are justified in believing H:

1.3. $J(H)$.

Obviously:

1.4. $H \Box \neg(E \,\&\, \neg H)$.

If CJ applies, then you must be justified in believing $\neg(E \,\&\, \neg H)$:

1.5. $J(\neg(E \,\&\, \neg H))$.

Thus, it follows from CJ that you aren't justified in believing a proposition unless you are justified in believing that your evidence for that proposition isn't misleading.

There is serious trouble now. For the reasons rehearsed above, it appears that you aren't justified in believing that your evidence isn't misleading. Hence, given CJ, you aren't justified in believing H. It turns out that any belief which is supposed to enjoy inductive justification really isn't justified at all. We have arrived at full-scale scepticism about inductive justification.[9]

This dialectic can be framed as a trilemma. Either:

I. Inductive scepticism prevails.
II. CJ is false.

or

III. One can have justification—somehow—for believing that one's evidence isn't misleading.

[8] I use the following notation: 'J(X)' means that one is justified in believing X; 'X □ Y' means that X (logically) entails Y; 'K(X)' means that one knows X; 'APJ(X)' means that one is justified a priori in believing X. Throughout, I will assume that the pertinent entailments are known by the subjects.

[9] A number of philosophers think that the problem of misleading evidence is the essence of all sceptical arguments. See Huemer (2001) and, with some caveats, Weatherson (2005). From this standpoint, there is no significant difference between scepticism about induction and scepticism about the external world. For further discussion, see Section 5.6.

Recent work by Fred Dretske (2005) suggests that we ought to endorse (II). On his view, one can't have justification for believing ¬(E& ¬H). But if CJ doesn't apply, one can be justified in believing H nevertheless. Since we can avoid scepticism by rejecting CJ, we do just that. The problem of misleading evidence leads us to abandon CJ.[10]

This motivation for rejecting CJ is ultimately unsound. The claim that we lack justification for ¬(E & ¬H) rests on the Entailment Principle and Hume's Principle taken together. But it is doubtful that these principles are jointly acceptable, regardless of CJ. Consequently, the argument against CJ is no good.

To see this, consider a situation in which two conflicting scientific theories entail all the available (relevant) evidence. I will say that such theories are *empirically concurrent*.[11] For example, let's pretend that the Copernican hypothesis (CH) and the Ptolemaic hypothesis (PH) both entail certain facts about the observed motions of the planets (O), and that O is the only relevant evidence on hand. By Hume's Principle, we don't have a priori justification for ¬PH. If the Entailment Principle also holds, then O provides no empirical justification for ¬PH. We have no justification at all for ¬PH. This outcome is disturbing, to say the least. In this case, lacking justification for ¬PH would be as bad as lacking justification for CH. If we aren't justified in believing ¬PH and similar claims, then a substantial form of scepticism prevails.[12]

The important thing to notice is that this result follows from the combination of Hume's Principle and the Entailment Principle, whether CJ holds or not. Grant, as you should, that we are justified in rejecting PH. It follows that either Hume's Principle, the Entailment Principle, or both must be wrong. In that event, there is no reason to deny that we have justification for ¬(E & ¬H). And, if it is possible to have justification for ¬(E & ¬H), CJ can be true without leading to scepticism. The motivation for rejecting CJ is gone.[13]

[10] Dretske holds that we don't know propositions like ¬(E & ¬H). Then, given closure for knowledge, scepticism results: 'If, in order to see (hence, know) that there are cookies in the jar, wine in the bottle, and a zebra in the pen, I have to know that I am not being fooled by a clever deception, that the "appearances" (the facts on which my judgments are based) *are not misleading,* then scepticism is true.' (2005: 16–17, emphasis added). From Dretske's point of view, scepticism is out of the question, so we have to give up closure for knowledge.

[11] Usually, two theories are said to be 'empirically equivalent' if what they imply about the evidence, actual or possible, is the same. I am introducing the term 'empirically concurrent' to designate two or more theories that imply all the actual (relevant) evidence. Empirically concurrent theories may diverge in what they entail about other, not yet available evidence.

[12] Refuting a hypothesis is no less important nor different in kind from establishing one. Accordingly, an experiment may be highly significant because it establishes that a particular theory is false, not that some other theory is true. The Michelson–Morley experiment was important because it overturned a version of the ether theory (although it wasn't viewed that way at the time it was conducted). Or, to take another example, the Meselson–Stahl experiment refuted the accounts of DNA replication due to Delbrueck and to Stent, as much as it supported the Watson–Crick account. For discussion, see Weber (2009).

[13] In other words, the correct response to the trilemma of the previous section is to endorse alternative III rather than alternative II. However, it must be said that the argument in the text is less than a full defence of CJ. Let 'E(X)' mean that the evidence E supports X. One can formulate a closure principle for epistemic support: (CE) If E(X) and (X □ Y) then E(Y). See Hempel's classic discussion of the 'Special Consequence Condition' (1965). CE is widely rejected on intuitive or formal grounds. Let's say that CE is false, and that one's evidence E supports X and doesn't support Y. Since E supports X, one may be justified in believing X on that basis, J(X). Y, however, enjoys no support from E, so one might lack justification altogether for Y,

At this point, the foe of CJ might turn to Dretske's treatment of knowledge for help. Dretske says that closure for knowledge may fail for what he calls 'heavyweight' logical consequences, although it holds for 'lightweight' ones (2005: 16). The idea would be that ¬PH is a heavyweight consequence of CH. You are justified in believing CH, even though you have no justification for believing ¬PH. Since you lack justification for ¬PH, there is no reason to deny either the Entailment Principle or Hume's Principle.

This manoeuvre is unavailing. Dretske's distinction between heavyweight and lightweight logical consequences doesn't apply here. Y is a heavyweight logical consequence of X if the reason one has for believing X fails to justify Y. Or, as Dretske also says, Y is a heavyweight consequence of X just in case 'the way you know X' isn't a way for you to know Y (2005: 15–16).[14] Take these characterizations in order. Suppose that, in the astronomy example, you choose CH over PH because CH is simpler than PH in some important way. Your reason for believing CH is the same as your reason for believing ¬PH, namely that CH is simpler than PH. So, ¬PH isn't a heavyweight implication of CH according to Dretske's first criterion. ¬PH isn't a heavyweight consequence of CH according to the second criterion, either. In the situation just described, you could come to believe that CH is true and PH is false via inference to the best explanation. You would then know that CH is true and that PH is false in the same way. Thus, following Dretske provides no basis for maintaining that we are justified in believing CH but not ¬PH.

A further point applies. Grant that there is some principled way to classify ¬PH as a heavyweight logical consequence of CH. We could follow Dretske and say that you can be justified in believing CH without being justified in believing ¬PH. Even if this path is open, we shouldn't take it. Suppose you really are justified in believing CH. In that case, to deny that you are also justified in believing ¬PH isn't shrewd philosophy, it is a rejection of the fruits of science.[15]

In short, the attack on CJ by way of the Entailment Principle and Hume's Principle overshoots. If it succeeded, we would also have to say—incorrectly—that a justified choice between empirically concurrent hypotheses is impossible. Since the

¬J(Y). In that event, J(X) and ¬J(Y), despite (X □ Y). Failure of closure for justification seems to follow from failure of closure for epistemic support. The problem of misleading evidence may be seen as a special case of this phenomenon. My defence of CJ in the text doesn't apply straightforwardly to broader worries about CJ brought on by the possibility of violations of CE. However, I argue elsewhere that this threat to CJ is more illusory than real. See Vogel (forthcoming).

[14] Dretske makes this point in connection with perceptual justification specifically. He writes that perception 'does not transmit its evidential backing to *all* the known consequences of what is perceived. We can see (hear, smell, feel) that P, but some of the Qs that (we know) P implies are just too remote, too distant, to inherit the positive warrant the sensory evidence confers upon P . . . For perception there are *always* heavyweight implications, known implications to what one perceives (P) that one's perceptual reasons for P are powerless to reach' (2005: 15–16).

[15] Or in Dretske's words, 'that sounds like chutzpah, not philosophy, to me' (2005: 24). A scientific anti-realist might object that we don't have justification for either CH or ¬PH. Therefore, a defense of CJ that depends upon our having justification for ¬PH can't work. But the anti-realist doesn't say that we have justification for CH and not for ¬PH. Therefore, the anti-realist has no reason to deny CJ. Rather, she is a sceptic, at least so far as a broad range of scientific theories go.

combination of the Entailment Principle and Hume's Principle leads to this unacceptable result, at least one of those two principles has to be false. The argument against CJ breaks down, because at least one essential premise of that argument is incorrect.

5.2. The Empiricist Solution to the Problem of Misleading Evidence

The previous section presented a reason to think that Hume's Principle and the Entailment Principle aren't jointly acceptable. This result opens the door for the possibility that there is some kind of justification for believing that one's evidence isn't misleading. It may be that E itself provides empirical justification for rejecting (E & ¬H). Call this the *empiricist solution* to problem of misleading evidence. Alternatively, we might have a priori justification for ¬(E & ¬H).

The empiricist solution runs counter to the Entailment Principle, but that principle appears to be vulnerable to certain counterexamples. What's more, these cases provide direct support for the empiricist solution. Consider:

Devil's Island. Let N = No prisoner has ever escaped from Devil's Island before. Let F = Brittany (who is incarcerated on Devil's Island) will be the first prisoner to escape from there. It is plausible that N is a good reason to reject to F, even though F entails N.
Seasons. Let W = Winter has always been followed by spring. Let S = This winter will be the first not to be followed by spring. It is plausible that W is a good reason to reject S, even though S entails W.
Emeralds. Let O = All observed emeralds are green. Let U = Even though all observed emeralds are green, there is at least one unobserved non-green emerald somewhere. It is plausible that O is a good reason to reject U, even though U entails O.

Let's look more closely at the Devil's Island example, in particular. Suppose that (N), no prisoner has ever escaped from Devil's Island before, is evidence for (B) Brittany won't escape from Devil's Island. N is *misleading* evidence just in case (N & ¬B). To deny that N is misleading evidence for B is to affirm ¬(N & ¬B). If ¬(N & ¬B) is true, then it isn't the case that no one has escaped from Devil's Island before and Brittany will escape from Devil's Island. Or, more simply, if ¬(N & ¬B) is true, then Brittany won't be the first prisoner to escape from Devil's Island. That no one has escaped from Devil's Island before seems to be a good reason to believe that Brittany won't be the first prisoner to escape from Devil's Island. If this impression is correct, then N itself is good reason to believe ¬(N & ¬B). That is, N may justify the belief that N itself isn't misleading, bearing out the empiricist solution. A similar lesson may be drawn from the other examples.

An opponent might question whether this bit of epistemic phenomenology should be taken at face-value. But somewhat more can be said in favor of the empiricist solution. In the Devil's Island Case, the possibility that one's evidence is misleading is expressed as a conjunction, (N & ¬B). The evidence N bears on the conjunction in two ways. It supports the first conjunct, which is just N itself. At the same time, N also counts strongly against the second conjunct, ¬B. For the whole conjunction (N & ¬B) to be true, both conjuncts must be true. And, insofar as N provides good reason to believe that one of the conjuncts is false, it seems that N provides strong reason to believe that the entire conjunction is false. To that extent, it doesn't really matter whether N supports the other conjunct. N may justify rejecting (N & ¬B) all the same. [16] Perhaps this analysis of the Devil's Island Case provides a template for how the empiricist solution works in general.

I need to be clear about what I am saying and not saying. My claim isn't that our intuitive judgments about justified belief have to run along these lines. Nor am I asserting that justification operates by the mechanism just described. My point is rather that our intuitions about the Devil's Island Case and others like it aren't bizarre or inscrutable. Those intuitions may not be dispositive by themselves, but they can't be dismissed, either.[17]

5.3. More About Theory Choice

The empiricist solution to the problem of the problem of misleading evidence has some immediate plausibility, as we have seen. The principal reason to demur is allegiance to the Entailment Principle. However, broad considerations about theory choice tell against the Entailment Principle and bolster the empiricist solution.

Let's return to the sort of situation described in Section 5.1. You are choosing between two empirically concurrent hypotheses H1 and H2. Both entail your evidence E. Let's grant that E supports both hypotheses to some extent. Still, E may support H1 much more strongly than it supports H2, and in such circumstances E may justify rejecting H2 in favor of H1. If so, the Entailment Principle is violated, because H2 entails E. This account of theory choice bears immediately on the nature of our justification for believing that our evidence isn't misleading, i.e. ¬(E & ¬H). H and (E & ¬H) are competitors. It may be allowed that both are supported to some extent by E. But if E supports H more strongly than E supports (E & ¬H), then E may justify accepting ¬(E & ¬H) all the same.

To get clearer about these issues, it will help to look at another example:

[16] David Christensen has pointed out to me that our intuitions about some cases like these can be affected by the order in which the details of the case are presented. My best guess is that this variability is due to a shift in attention from one conjunct to the other. If we are led to focus on the conjunct which entails the evidence, we lose sight of the fact that the evidence counts against the other conjunct, and therefore against the conjunction as a whole. But Christensen's observations deserve a fuller response than I can provide here.

[17] There is further discussion of this pattern of justification in Section 5.5. See also Vogel (forthcoming).

Mouse in the Wainscoting. C = When you leave a piece of cheese by the little hole in the wainscoting at night, it is gone next the morning. There are two hypotheses about what is going on. W1 = There is a mouse in the wainscoting that, during the night, eats the cheese you left by the hole. W2 = Your neighbour owns an exterminating company, and he sneaks into your house after you go to bed and removes the cheese by the hole. He does this in order to drum up business for his firm.[18]

My view is that, while C provides some reason to believe W2, C provides much more reason to believe W1. Thus, C justifies accepting W1 and rejecting W2. In other words, C justifies accepting ¬W2, despite the fact that W2 entails C. The upshot is that the Entailment Principle doesn't hold in this case and, presumably, in other instances like it. To that extent, we are free to adopt the empiricist solution to the problem of misleading evidence.

Of course, this account of theory choice is open to dispute. The opposing view is that the Entailment Principle does hold in situations like the Mouse-in-the-Wainscoting Case. Therefore, C can't justify believing ¬W2. If you are justified in believing ¬W2 after all, then your justification for ¬W2 must be a priori (at least in part).[19]

This sort of position is supported by a line of thought that seems to have widespread appeal. The following version is due to Michael Huemer:

> But now consider examples of likely candidates for reasons for preferring one hypothesis over another. Simplicity is often suggested in this connection—that is, the fact that h_1 is the simpler hypothesis in some sense may be a reason for preferring h_1 over h_2 . . . But if h_1 is simpler than h_2, then it is a necessary truth that h_1 is simpler than h_2 (assuming that simplicity is an objective characteristic of propositions) . . . Now, if we take this route, we will get an interesting result. If the relevant necessary truth(s) can be known a priori, then it appears that we can also have a priori knowledge of (or at least justification for) synthetic, contingent truths. For if e is a reason for preferring h_1 over h_2, then it appears to be a reason for thinking that if either h_1 or h_2 is the case, h_1 is the case. Now the proposition, if either h_1 or h_2 is the case, h_1 is the case, is contingent, but it is apparently justified by an a priori truth. And whatever is justified by an a priori truth is justified a priori. (2001: 389)[20]

Various readings of this passage are possible, but here is one that seems to capture the gist. Suppose that H1 and H2 are empirically concurrent theories. You are justified in accepting H1 and rejecting H2 on the basis of simplicity considerations. Then:

3.1. □ (H1 is simpler than H2). Assumption.
3.2. □ (H1 is simpler than H2) □ APJ(H1 is simpler than H2). Assumption.
3.3. APJ(H1 is simpler than H2). From 3.1, 3.2.

[18] This example is a modified version of one presented in Van Fraassen (1980): 19–20.

[19] The Mouse-in-the-Wainscoting example is nice and vivid, but it isn't fully apt as an illustration, because it is natural to assume that C wouldn't be your total relevant evidence. In ordinary situations, you would have background information which bears on how likely it is that your neighbour would be in such a situation and would act as described. But nothing turns on these points, so I will continue to use the example for its heuristic value.

[20] Huemer (2001) explores the view that a priori justification plays a crucial role in theory choice, but he doesn't endorse it without qualification.

3.4. H1 is simpler than H2 $\Box$ ((H1 $\vee$ H2) $\supset$ H1). Assumption.

3.5. APJ((H1 $\vee$ H2) $\supset$ H1). From 3.3 and 3.4.[21]

The upshot of 3.5 is that if one is justified in accepting H1 on the basis of simplicity considerations, one's justification for H1 must be a priori (at least in part). This is contrary to the view of theory choice presented above, according to which one's evidence can provide (fully) empirical justification for accepting one of two empirically concurrent hypotheses.

The argument as given might be challenged at a number of points. But, above all, premise 3.4 is pretty clearly false. Even though H1 is simpler than H2, it could be that H2 is true nevertheless. Suppose so. If H2 is true, (H1 $\vee$ H2) is true. But the truth of H2 excludes the truth of H1, so H1 is false. Therefore, it is false that ((H1 $\vee$ H2) $\supset$ H1). This can be so despite the greater simplicity of H1. Hence, the consequent of 3.4 can be false, while the antecedent is true. 3.4 as a whole is false, so that the argument 3.1-3.5 is unsound.

One might try to re-formulate the argument, substituting 3.9 for 3.4:

3.6. $\Box$ (H1 is simpler than H2). Assumption.

3.7. $\Box$ (H1 is simpler than H2) $\Box$ APJ(H1 is simpler than H2). Assumption.

3.8. APJ(H1 is simpler than H2). From 3.6, and 3.7.

3.9. APJ[H1 is simpler than H2 $\supset$ ((H1 $\vee$ H2) $\supset$ H1)]. Assumption.

3.10. APJ((H1 $\vee$ H2) $\supset$ H1). From 3.8 and 3.9.

But 3.9 is just as questionable as 3.4. The argument in the quoted passage rests on the idea that the recognition of necessity yields a priori justification. No other ground of a priori justification is put forward or suggested. We would have some basis for accepting 3.9 if

3.11. $\Box$[H1 is simpler than H2 $\supset$ ((H1 $\vee$ H2) $\supset$ H1)].

were true. However, 3.11 is just a rewriting of 3.4, which is mistaken. We have, as yet, no reason to think that choosing between empirically concurrent theories requires any beliefs to be justified a priori.

There is another argument in the same general spirit which seems to avoid the difficulties besetting 3.1–3.5 and 3.6–3.10. Consider:

3.12. When there are two empirically concurrent hypotheses H1 and H2, the total relevant evidence E justifies (H1 $\vee$ H2), but not H1.

3.12 implies that E alone is insufficient to secure the justification of H1. Therefore, if you are justified in believing H1, you must be justified in believing something in addition to E, call it 'X':

3.13. J(H1) $\supset$ J(X).

[21] The transition from 3.3 and 3.4 to 3.5 could be underwritten by a plausible closure principle for a priori justification: If APJ(X) and (X $\Box$ Y) then APJ(Y).

Allow that E and X together justify H1. Since E by itself isn't sufficient to justify H1, E can't justify X. So, if X is justified, there must be a priori justification for X:

3.14. $J(X) \supset APJ(X)$.

From 3.13 and 3.14:

3.15. $J(H1) \supset APJ(X)$.

The general conclusion is that when two theories entail the available evidence, and we are justified in accepting one and rejecting the other, then the justification we have must be a priori (at least in part). In that event, we aren't able to reject either hypothesis on the basis of empirical evidence alone.

However, the argument 3.12–3.15 is ill-motivated. There is simply no reason to accept the primary assumption, 3.12. Essentially, there are two different models of how, given E as our evidence, we could be justified in accepting H1 (and rejecting H2). These models may be displayed graphically, where the arrows indicate direct epistemic support:

3.16.
$$\begin{tikzcd}[column sep=small]
E \arrow[r] & (H1 \vee H2) \arrow[dr] & \\
& & H1 \\
& X \arrow[ur] &
\end{tikzcd}$$

3.17. $E \rightarrow H1$.

Premise 3.12 declares that E alone doesn't provide sufficient reason to accept H1 (say, by way of inference to the best explanation). That is, 3.12 stipulates that the justification for H1 can't have the structure displayed in 3.17 and must have structure displayed in 3.16. But no grounds supporting this claim have been provided.[22]

Someone might insist that 3.12 is correct, appealing to a case like the following to make the point:

Diagnosis Case. A patient exhibiting symptom S goes to see the doctor. There are only two diseases, D1 and D2, which produce S, and both invariably do so. However, D2 is much rarer than D1. In nearly all cases, if someone has S, he has D1. The doctor is aware of this fact and concludes that her patient has D1, not D2.

[22] One reason to accept it is a bad one, namely deductivism about justification. The thought would be that E can't directly justify H1 because E doesn't entail H1. To close the logical gap, one needs (a priori) justification for something else, X. A (putatively) different, narrower motivation would go as follows. Inference to the best explanation is illegitimate unless there is some basis for believing that it is likely to yield true results. If E justifies H1 via explanatory inference, this further consideration plays the role of X in 3.16. But in my view, the initial 'truth-demand' carries no weight, and inference to the best explanation need not have that structure. See Vogel (in preparation).

Suppose that both the claim that a patient has D1 and the claim that he has D2 entail that the patient exhibits symptom S. It is natural to say in this instance that the patient's having S doesn't justify the doctor in believing that the patient suffers from D1 rather than D2. What does so is the doctor's belief that D2 occurs much less frequently than D1. The relations of epistemic support in this instance are captured by 3.18 rather than 3.19:

$$\begin{tikzcd}[column sep=small] \text{3.18. Patient has S} \arrow[r] & \text{(Patient has D1} \lor \text{Patient has D2)} \arrow[dr] & \\ & & \text{Patient has D1} \\ & \text{D2 occurs much less frequently than D1} \arrow[ur] & \end{tikzcd}$$

3.19. Patient has S ➝ Patient has D1.

A defender of the Entailment Principle might maintain that the Diagnosis Case is the model for all situations in which two competing hypotheses entail all the relevant evidence.[23]

Certainly, the structure of justification in *some* circumstances conforms to the pattern 3.18 rather than 3.19. But there is a crucial difference between the Diagnosis Case and situations which bear on the status of the Entailment Principle. In a word, the Diagnosis Case isn't one in which the competing hypotheses entail *all* the relevant evidence. Part of the doctor's evidence is the information that D2 occurs much less frequently than D1, and the hypothesis that the patient has D1 (or D2) doesn't entail anything about the prevalence of D1 and D2. What's more, in the Diagnosis Case, the total relevant evidence *does* support D1. The Diagnosis Case doesn't fit the schema:

3.20. The total relevant evidence E justifies (H1 ∨ H2), but not H1.

All in all, the Diagnosis Case provides no support for 3.20 and, hence, has no bearing on the status of the Entailment Principle.[24]

To summarize: if one's evidence can provide empirical justification for choosing between two empirically concurrent hypotheses, the Entailment Principle is false and the empiricist solution is on a firm footing. The arguments 3.1–3.5, 3.6–3.10, and 3.12–3.15 are meant to show, to the contrary, that one's justification for choosing one empirically concurrent hypothesis rather than an other can't be (wholly) empirical. However, these arguments are flawed, and the Diagnosis Case does nothing to strengthen the last. There is no reason to think that when we choose between empirically concurrent hypotheses, we inevitably rely on some a priori justified belief. The empiricist solution is sustained.

[23] I am indebted to David Christensen for pressing me to consider this kind of case.

[24] See n. 24.

5.4. Is There A Priori Justification for ¬(E & ¬H)?

The empiricist solution will be undermined if we do have a priori justification for believing ¬(E & ¬H).[25] This section will take up an argument for that conclusion due to Brian Weatherson (2005). Weatherson defends a qualified claim. He tries to show that, if certain substantial commitments are in place, it follows that we are justified a priori in believing that our evidence isn't misleading.

Here is a modified and somewhat simplified version of Weatherson's line of thought.[26] Assume that if E is my total evidence, I am justified in believing H. Let J*(X) stand for 'I am justified in believing X,' let APJ*(X) stand for 'I am justified a priori in believing X,' and let 'ETE' stand for 'E is my total evidence.' Then, as a matter of logic:

4.1. □ [ETE ∨ ¬ETE].[27]

By hypothesis:

4.2. ETE □ J*(H).

From 4.2 and CJ:

4.3. ETE □ J*(¬ETE ∨ H).

Now, suppose that a form of internalism is true, so that what evidence I have is fixed by facts about my mind. Then, whether ETE in particular is true is determined my current mental state. Grant as well that mental states are 'luminous'. That is, if M is some fact about my current mental state, then J*(M). Then:

4.4. ¬ETE □ J*(¬ETE).

From 4.4 and CJ:

4.5. ¬ETE □ J*(¬ETE ∨ H).

From 4.1, 4.3, and 4.5:

4.6. □ [J*(¬ETE ∨ H)].

[25] In principle, there could be justification of both sorts for believing that one's evidence isn't misleading, preserves the Entailment Principle, which is inimical to empirical justification for ¬(E & ¬H). See Section 5.1.

[26] Weatherson (2005) builds up to this argument over a number of pages. The final variant, which I am following here, is set out at pp. 325–6. One difference between Weatherson's original formulation and my retelling is that, for Weatherson, the initial assumption is 'all my possible evidential states are G or not G', where 'having evidence that is G justifies belief in snow next winter' (Weatherson's choice of H) (2005: 326). Also, Weatherson tries to address objections to luminosity assumptions due to Timothy Williamson. The luminosity principle Weatherson adopts is: M □ ¬K¬KK(¬M) (2005: 325). The discrepancy between Weatherson's more hedged luminosity assumption and the simpler one I use in the text doesn't affect anything which follows.

[27] In the interests of clarity, I will set aside complications that arise from the contingency of my existing and of my having epistemically significant mental states.

Another assumption comes into play at this point. Suppose that, necessarily, I am justified in believing some proposition X. I will be justified in believing X regardless of what empirical evidence I happen to have. It seems, then, that my justification for believing X must be a priori.[28] If necessarily having justification implies having a priori justification, 4.6 gives us:

4.7. APJ* (¬ETE ∨ H).

Note that (¬ETE ∨ H) is logically equivalent to ¬(ETE & ¬H). Informally, 4.7 says something like 'I have a priori justification for believing that if E is my total evidence, then E isn't misleading'.

There is reason to be suspicious of this argument. Consider:

4.8. □ [[illegible]¬(E is *your* total evidence) ∨ H)].

I could have reason to believe that E is your total evidence, and justifies *you* in believing H, even though H is false all the same. Since such a situation is possible, 4.8 is false. Hence, there is no basis for the claim that:

4.9. APJ*(¬E is *your* total evidence ∨ H).

This result indicates that something is wrong with 4.1–4.7. How can I have a priori justification for believing that E isn't misleading if E happens to be *my* total evidence (i.e. 4.7), but not if E happens to be *your* total evidence (4.9)? How can the fact that the evidence is mine and not yours make any difference as to whether ¬(E & ¬H) is true?

Arguably, one defect in Weatherson's argument is that 4.4 is false. In order to be aware of what my total evidence is, I need to *survey* the contents of my mind. I have to exercise some capacity for introspection.[29] If I introspect and E isn't my total evidence, I am justified in believing ¬ETE. However, it seems wrong to claim that I am justified in believing ¬ETE even if I don't introspect. Thus we have:

4.10. ¬ETE □[illegible] introspect, then J*(¬ETE)).

But then:

4.11. ¬ETE □[illegible]ETE)

isn't true in full generality.[30]

[28] I have my doubts, but I'll let them pass.

[29] Weatherson writes: 'The strategy I've used to build the argument is fairly transparent: find a disjunctive a priori knowable proposition by partitioning the possible evidence states into a small class, and adding a disjunct for every cell of the partition. In every case, the disjunct that is added is one that is known to be known given that evidence. If one of the items of knowledge is ampliative, if it goes beyond the evidence, then it is possible the disjunction will be deeply contingent. But the disjunction is known no matter what. If internalism is true, then the partition can divide up evidential states according to the *introspective* properties of the subject' (2005: 14, emphasis added).

[30] Let me add that I am not taking for granted that introspection is some kind of inner sense. For useful discussion that bears on these matters, see Mc Laughlin and Tye (1998).

What happens to the argument 4.1–4.7, if 4.4 is replaced by 4.10? As before:

4.12. □ [ETE ∨ ¬ETE].

4.13. ETE □ J*(H).

From 4.13 and CJ:

4.14. ETE □ J*(¬ETE ∨ H).

And, as a matter of logic:

4.15. ETE □ If I introspect, then J*(¬ETE ∨ H).

From 4.10 and CJ:

4.16. ¬ETE □ If I introspect, then J*(¬ETE ∨ H).

But now the proper conclusion from 4.12, 4.15, and 4.16 is no stronger than:

4.17. □ [If I introspect, then J*(¬ETE ∨ H)].

4.17 falls short of 4.7. At the very least, Weatherson's argument goes awry because introspection is the source of our (empirically) justified beliefs about what evidence we have. Bearing that fact in mind, we shouldn't accept the conclusion that one is justified a priori in believing that one's total evidence isn't misleading.

5.5. The Confirmation Principle and the Dynamics of Justified belief

The burden of Sections 5.2–4 was that the Entailment Principle is false and that E can justify ¬(E & ¬H). Since the Confirmation Principle implies the Entailment Principle, this result counts against the Confirmation Principle, too. But the Confirmation Principle fails for a further reason, to be explored below. As a related matter, there is a prominent strain of argument which proceeds from considerations about the acquisition of justified belief to the conclusion that E can't justify ¬(E & ¬H). But, as we will see, the difficulty that arises for the Confirmation Principle vitiates this line of thought as well.

The Confirmation Principle stipulates that evidence justifies a hypothesis only if that evidence makes the hypothesis more likely to be true. In particular, if one acquires evidence E, E justifies H only if the acquisition of E increases the strength of one's basis for believing H. But this claim isn't correct in general. To put the point most simply, suppose you originally have evidence E1 which makes H extremely likely, giving you justification for H. You then obtain further evidence E2 which expunges the justification for H provided by E1. At the same time, E2 independently makes H very likely, although a bit less likely than E1 did. Since E2 makes H very likely, E2 may justify you in believing H, even though the probability of H has decreased. In these circumstances, the Confirmation Principle fails.[31]

Here is an illustration:

Thermometers. Rex is inside an air-conditioned room. He looks out the window at his deluxe model thermometer, which reads 80 degrees. Given Rex's background information, it extremely likely that the outside temperature is 80 degrees. The deluxe model thermometer is slightly more reliable than the standard model, so in general one ought to place slightly less confidence in the readings of the standard model than in the readings of the deluxe model. Still, the standard model is an excellent instrument, used for many demanding applications. If Rex had used a standard model thermometer instead of the deluxe one, he would have been justified in believing that the outside temperature is 80 degrees. Now, it happens that Omar, the thermometer repair man, comes over and checks Rex's deluxe thermometer. Omar discovers that, extraordinarily, it isn't working properly. Fortunately, however, Omar is carrying a standard model thermometer which reads 80 degrees. Omar tells Rex that his deluxe thermometer is broken, but also that Omar's own standard thermometer reads 80 degrees. Given the new evidence provided by Omar, Rex ought to be slightly less confident than he was before that the outside temperature is 80 degrees. Nevertheless, what Omar tells Rex justifies him in believing just that.

If the Confirmation Principle were correct, Rex wouldn't be justified in believing that the temperature outside is 80 degrees. Therefore, the Confirmation Principle is incorrect.

A proponent of the Confirmation Principle might resist in various ways. One thing she might say is that Rex's confidence that it is 80 degrees out falls when he learns that his deluxe thermometer is broken. But then, when Omar tells Rex what the standard thermometer says, Rex's confidence goes back up. Rex's new evidence justifies his belief that the temperature outside is 80 degrees insofar as it raises Rex's confidence from its previously lowered level. There is no violation of the Confirmation Principle. Another idea is that the description of the case oversimplifies the way justification works, and thus how the Confirmation Principle governs justification. Confirmation and justification obtain with respect to a background or default probability function which may diverge from the credences one actually has at a particular time.[32] While the information Omar gives Rex lowers his actual credence that it is 80 degrees outside, the same information elevates Rex's credence vis-à-vis its background or default value, and that is why Rex's belief is justified. Thus, the Confirmation Principle holds, but with respect to background or default levels of rational belief, specifically.

I doubt that either of these responses saves the Confirmation Principle. But instead of pursuing this issue right away, let's consider an argument that might be offered in

[31] Martin Smith (2010) arrived independently at a similar point about knowledge to which Weatherson (Ch. 4, this volume) has responded. He writes: 'The same goes for knowledge; things that remove defeaters of knowledge are importantly different in kind to the underlying bases for knowledge.' However, the point that a defeater may still confer justification on a hypothesis remains, and I see no reason why it wouldn't carry over to knowledge as well.

[32] I am indebted here to Hartry Field.

support of the principle. It will become apparent in this context why the defences just aired are unsuccessful. The argument in favour of the Confirmation Principle is suggested by a line of thought Roger White has directed against dogmatist replies to scepticism.[33] Both focus on the 'dynamics' of justified belief, that is, how we come to believe H on the basis of evidence E. So I will call the argument for the Confirmation Principle the 'Dynamical Argument'.

The Dynamical Argument is directed against the view I have been defending, namely, that when you have E as your evidence for H, E can justify you in believing that E itself isn't misleading. The Dynamical Argument proceeds within a Bayesian setting. We assume that rational credences at a time are probabilities, and these credences are updated by conditionalization. Beyond this basic Bayesian framework, we also suppose that one is justified in believing a proposition if one's rational credence in that proposition is sufficiently high. The Dynamical Argument goes as follows:

5.1. Suppose you acquire E at t2, and thereby come to be justified in believing H for the first time.[34] (Assumption).

5.2. Your belief that H is arrived at by conditionalization, so that Pr(H) at t2 = Pr(H/E) at t1. (Assumption).

5.3. Since Pr(H) at t2 is equal to or greater than the threshold for justification, so too is Pr(H/E) at t1. (From 5.1 and 5.2).

5.4. Pr(¬(E & ¬H)) at t1 is greater than or equal to Pr(H/E) at t1. (Probability fact).

5.5. Pr(¬(E & ¬H)) at t1 is greater than or equal to the threshold for justification, so at t1 you are justified in believing ¬(E & ¬H). (From 5.3 and 5.4).

5.6. At t1, you have yet to acquire E, so at t1 E doesn't justify ¬(E & ¬H). (From 5.1).

5.7. Acquiring E doesn't alter the justification you have for ¬(E & ¬H), if any. (Assumption).[35]

5.8. Hence, even when you acquire E as your evidence, your justification for ¬(E & ¬H) isn't due to E. (From 5.5, 5.6, and 5.7).

The Dynamical Argument might be resisted to the extent that it relies on aspects of Bayesianism that are controversial.[36] But suppose we grant the correctness of the argument through 5.6. Even so, the argument doesn't go through. The trouble lies with premise 5.7. When you acquire E, your justification for believing ¬(E & ¬H), if any, may change. E may remove whatever reason you had beforehand to believe ¬(E & ¬H), while also giving you a new reason to believe ¬(E & ¬H). So, when you acquire E, E justifies your belief that ¬(E & ¬H) after all.

[33] White (2006).

[34] This restriction is meant to guarantee that when you do have justification for H, that is because E in particular justifies H. The question then is whether E also justifies ¬(E & ¬H).

[35] This step could be supported by appeal to the Confirmation Principle or treated as independently credible. But, as I will maintain, it ought not to be accepted one way or the other.

[36] White declares that his Bayesian commitments are 'very modest' (2006: 535) and seeks to justify their employment by way of various examples.

Here is the point in more detail. Consider your epistemic situation at t1. Pr(¬(E & ¬H)) at t1 is high. ¬(E & ¬H) may be rewritten as (¬E ∨ H). Thus, at t1, Pr(¬E ∨ H) is high. It is a probability fact that Pr (¬E ∨ H) = Pr(¬E) + Pr(H) – Pr(¬E & H). Therefore, Pr(¬E ∨ H) is high at t1 only if the sum Pr(¬E) + Pr(H) is high at t1. By hypothesis, at t1, you don't have a justified belief in H, so at t1 Pr(H) is low. Hence, if the sum Pr(¬E) + Pr(H) is high at t1, that is because Pr(¬E) is high at t1.[37] So, at t1, you are justified in believing (¬E ∨ H) to the extent that you are justified in believing ¬E.[38]

Now suppose that you acquire E as evidence, and update. The justification you had for ¬E is lost, and with it you lose your original justification for (¬E ∨ H). But also, when you update on E, your probability for H, which was low, rises to the threshold for justification or beyond. And, since Pr(H) is high at t2, Pr (¬E ∨ H) must be high at t2 as well. That is to say, at t2 you are justified in believing (¬E ∨ H), just as you were at t1. However, *the source of your justification changes*, in a way that may be masked by the fact that Pr(¬E ∨ H) is high at both t1and t2.[39] Upon acquiring E, the job of justifying (¬E ∨ H) is 'handed off' from ¬E to E. E gives you empirical justification for believing ¬(E & ¬H)—whatever may have been the case beforehand.[40] Thus, the Dynamical Argument fails to establish that E can't justify ¬(E & ¬H). If anything, the considerations raised by the argument support the opposite conclusion.[41]

The preceding discussion has been quite abstract, so working through an example may be helpful. Suppose that, before you embark on your investigation of emeralds, you have no empirical evidence as to their colour. You then examine many emeralds and acquire as evidence O, that many emeralds have been observed, all of which are green. O supports G, that all emeralds are green.[42] Let's grant, for the sake of the

[37] In some cases, Pr(¬E) and Pr(H) are both middling at t1, so that their sum exceeds the threshold for justification at t1, even though neither Pr(¬E) nor Pr(H) does individually. But such cases aren't all the cases; the ones that are of interest here are those that are discussed in the text.

[38] We may allow for the sake of argument that, at t1, your justification for ¬E, and, thus, for (¬E ∨ H) is non-empirical. But then you have non-empirical justification for (¬E ∨ H) only insofar as you have justification for ¬E.

[39] The broader point is that the Bayesian apparatus has trouble dealing with relations of epistemic priority, because facts about epistemic priority don't reduce to facts about conditional and unconditional epistemic probabilities. This difficulty bedevils attempts to analyze putative failures of 'warrant transmission' in probabilistic terms. For a recent attempt, and a review of previous ones, see Moretti (2012).

[40] This observation meets the objection that Rex's degree of belief goes down and then up. If E doesn't entail H, Pr(¬(E & ¬H)/E) has to be lower than Pr(E & ¬H). So, on a Bayesian model of justification, the level of justification for ¬(E & ¬H) provided by E can't be as high as the level of justification one had before acquiring E. But E may justify ¬(E & ¬H) nevertheless. The same point applies to the objection that, in the earlier example, the Confirmation Principle might be preserved because Rex's credence in ¬(E & ¬H) could deviate from that assigned by a background or default probability function. Suppose Rex's credences conform exactly to those assigned by that function. When Rex acquires E as evidence, his credence with respect to ¬(E & ¬H) must fall. Even so, E may justify Rex in believing ¬(E & ¬H).

[41] Weatherson (Ch. 4, this volume) asks: 'Could there be a defeater that prevents someone knowing a priori that E ⊃ H even though the a priori probability of E ⊃ H is very high?' Yes, in this case: E.

[42] Of course, this description is a caricature, and some may doubt whether enumerative induction as such can provide justification at all. However, I think that the particular mechanism of inductive justification at work is really irrelevant; the situation would be structurally the same in any case. For that reason, the toy example in the text should raise no such worries.

argument, that before you acquire O, you have justification for (¬O ∨ G). It seems out of the question to suggest that before acquiring O you are justified in believing G. In the absence of empirical information like O, your rational credence with respect to G ought to be pretty low. So, if you are initially justified in believing (¬O ∨ G), that is because you have justification for ¬O. You then examine lots of emeralds and obtain O as evidence. O defeats ¬O, which was your initial justification for (¬O ∨ G). But, at the same time, O justifies you in believing G and (¬O ∨ G) as well.

Two further observations are called for. First, a Bayesian of a certain stripe might maintain that in order to acquire empirical justification for ¬(E & ¬H) at a later time, one must have a priori justification for ¬(E & ¬H) at an earlier time. What I have said in response to the Dynamical Argument doesn't foreclose such a possibility; I haven't tried to show that a priori justification for ¬(E & ¬H) is impossible. And it may well be that the details of Bayesian epistemology plus other commitments make it attractive or necessary to claim that there can be a priori justification for ¬(E & ¬H). However, a proponent of the Dynamical Argument is looking for a much stronger result. Her goal is to show that E can't provide empirical justification for ¬(E & ¬H) under any circumstances. But, as I have maintained, the Dynamical Argument does nothing of the sort.

The second observation to make is this. I have argued that the Confirmation Principle should be rejected. That is, justification and confirmation (i.e. probabilification) sometimes come apart. But one might think that, surely, confirmation is connected to justification somehow. That may be, although exactly how the two are related seems to me to be an open question. Here is one proposal that I offer with some diffidence. Suppose, at least to a first approximation, that justification amounts to high probability on one's total evidence. Then:

5.9. R raises the probability of S

implies

5.10. If you are otherwise justified in believing S, and you add R to your stock of evidence, you will remain justified in believing S[43]

and

5.11. R lowers the probability of S

implies

5.12. If you are otherwise justified in believing S, and you add R to your stock of evidence, you may not remain justified in believing S.

Whether 5.10 and 5.12 are exactly right is of secondary importance. What matters most is that rejecting the Confirmation Principle doesn't mean severing any link whatsoever between justification and confirmation.

[43] To be more careful, let me specify that I am taking confirmation as relative to a particular credence function; the implication holds only with respect to an agent with that particular credence function.

Let's recall the main points made in this section. If the Confirmation Principle holds, E can't justify ¬(E & ¬H). However, the Confirmation Principle fails when one's previous justification for a proposition is defeated by new evidence, and the new evidence supports the proposition, albeit at a slightly lower level. Precisely that may happen when the proposition is ¬(E & ¬H) and one acquires E as one's evidence. The Dynamical Argument is meant to demonstrate that E can't be evidence for ¬(E & ¬H). However, that argument is defective for roughly the same reason that the Confirmation Principle is. All in all, these results promote the view that we can have empirical justification for denying that our evidence is misleading.

5.6. Scepticism and Misleading Evidence

As we saw earlier, the possibility that one's evidence is misleading gives rise to a certain line of sceptical argument: Suppose that E is your evidence for H. By CJ, you aren't justified in believing H unless you are justified in believing ¬(E & ¬H). You have neither empirical justification nor a priori justification for ¬(E & ¬H). Hence, you aren't justified in believing H. A common view is that many or all familiar sceptical challenges take this form.[44]

Consider Cartesian scepticism. The classical argument for this kind of scepticism raises the possibility that your experience might be thoroughly unveridical. For example, suppose A, it appears to you that there's a sand-dune in front of you, but ¬D, there really is no such thing, because you are the victim of massive sensory deception. If your experience is unveridical in this way, then (A & ¬D). One might construe A as your evidence for D. In that case, the possibility that your experience is unveridical amounts to the possibility that your evidence is misleading. If, in general, you aren't justified in believing that your evidence isn't misleading, then you aren't justified in believing that your experience is veridical. Cartesian scepticism prevails. Seen in this light, Cartesian scepticism is just a specific version of the problem of misleading evidence.

Consider next the problem of induction. Suppose that you believe G, all emeralds are green, on the basis of the evidence O, all observed emeralds are green. One version of inductive scepticism attacks the claim that O provides a reason to accept G. But a more concessive way for the sceptic to proceed is to allow, for the sake of argument, that O is evidence for G. Even so, the sceptic will say, your evidence could be misleading. It could be the case that all observed emeralds are green, yet not all emeralds are green. If so, (O & ¬G). The sceptic will then argue via the Entailment Principle that O doesn't justify ¬(O & ¬G). If the closure principle CJ holds, then you aren't justified in believing G after all.[45]

[44] See n. 9.

[45] The first sceptical problem may be thought of as a version of the 'old riddle of induction', and the second may be thought of as a version of the 'the new riddle'. My approach to these issues is influenced by Gemes (1999).

I would like to make two points, one about inductive scepticism, the other about Cartesian scepticism. First, the concessive argument for inductive scepticism relies on the Entailment Principle. But that principle is false, or so I have argued. O can justify ¬(O & ¬G). This result contradicts the claim that you lack justification for ¬(O & ¬G), which is an essential premise of the concessive argument. Therefore, that argument is unsound. Admittedly, this reply presupposes that O is evidence for G. It therefore carries no weight with respect to the first version of inductive scepticism described above. There is some gain, nevertheless. If the first version of inductive scepticism can be dealt with, then the second version poses no further difficulty, contrary to what one might have supposed.

Turning now to Cartesian scepticism, one prominent anti-sceptical position is dogmatism. The dogmatist holds that EXP, your experience as of seeing a hand, gives you justification for believing HAND, that there is a hand before you. Either directly, or by way of your justified belief that HAND, EXP is also evidence for the claim ¬SK, that you aren't the victim of massive sensory deception.[46] An influential criticism of dogmatism challenges it on this score. If you are massively deceived, then it appears to you that there is a hand before you even though there really isn't. This would be a situation in which you have misleading evidence, i.e. (EXP & ¬HAND). If the Entailment Principle holds, then EXP can't be evidence for ¬(EXP & ¬HAND). That is, EXP can't be evidence for ¬SK, contrary to what the dogmatist maintains.[47] However, this objection to dogmatism is by no means conclusive. It depends squarely on the Entailment Principle, but the Entailment Principle isn't generally acceptable. To that extent, dogmatism remains tenable.[48]

5.7. Conclusion

The problem of misleading evidence is connected to a number of important issues in epistemology. Among these are the status of the closure principle for justification, the workings of theory choice, and the fortunes of various kinds of scepticism. How we ought to regard the problem of misleading evidence depends on the standing of the Entailment Principle and the Confirmation Principle. Neither principle turns out to be well motivated. Without them in place, it seems that evidence for a hypothesis can justify the belief that the evidence itself isn't misleading. The problem of misleading evidence does have a solution—the empiricist one.

[46] My presentation of dogmatism deviates from James Pryor's (2000). He generally denies that one's experience is evidence for one's perceptual beliefs.

[47] This criticism of dogmatism is a non-probabilistic variant of the one due to Roger White (2006), and others; see above. White's criticism would also apply to the attempt to foil Cartesian scepticism by an appeal to inference to the best explanation.

[48] For related discussion, see Zardini (Ch. 3, this volume).

References

Dretske, Fred (2005). 'The Case Against Closure.' In M. Steup and E. Sosa (eds.), *Contemporary Debates in Epistemology*, 13–26. Oxford: Blackwell.

Gemes, Ken (1999). 'Carnap-Confirmation, Content-Cutting, and Real Confirmation.' Unpublished essay.

Hawthorne, John (2002). 'Deeply Contingent A Priori Knowledge', *Philosophy and Phenomenological Research*, 65: 247–69.

Hempel, Carl (1965). 'Studies in the Logic of Confirmation.' In id., *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, 3–52. New York: Free Press.

Huemer, Michael (2001). 'The Problem of Defeasible Justification.' *Erkenntnis*, 54: 375–97.

Kripke, Saul (1980). *Naming and Necessity*. Cambridge, Mass.: Harvard University Press.

McLaughlin, Brian and Michael Tye (1998). 'Is Content-Externalism Compatible with Privileged Access?' *Philosophical Review*, 107: 349–80.

Moretti, Luca (2012). 'Wright, Okasha and Chandler on Transmission Failure.' *Synthese*, 184: 217–34.

Pryor, James (2000). 'The Sceptic and the Dogmatist.' *Noûs*, 34: 516–49.

Smith, Martin (2010): 'What Else Justification Could Be.' *Noûs*, 44: 10–31.

Van Fraassen, Bas (1980). *The Scientific Image*. Oxford: Oxford University Press.

Vogel, Jonathan (2004). 'Sceptical Arguments.' *Philosophical Issues*, 14: 426–55.

—— (2007). 'Subjunctivitis.' *Philosophical Studies*, 134: 73–88.

—— (forthcoming). 'Closure Preserved.' *Philosophical Studies*.

—— (in preparation). 'Explanation, Truth and the External World.'

Weatherson, Brian (2005). 'Skepticism, Rationalism, and Empiricism.' In T. Gendler and J. Hawthorne (eds.), *Oxford Studies in Epistemology*, Vol. I, 311–31 Oxford: Oxford University Press.

Weber, Marcel (2009). 'The Crux of Crucial Experiments: Duhem's Problems and Inference to the Best Explanation.' *British Journal for the Philosophy of Science*, 60: 19–49.

White, Roger (2006). 'Problems for Dogmatism.' *Philosophical Studies*, 131: 525–55.

6

Inference and Scepticism*

José L. Zalabardo

6.1. Moorean Inferences

In 1939 G. E. Moore provided what he took to be a perfectly rigorous proof of the existence of the external world. It consisted in holding up his hands and saying, as he made a certain gesture with the right hand, 'here is one hand', and adding, as he made a certain gesture with the left, 'and here is another' (Moore 1939).

The thought that scepticism can be refuted in this way has been embraced by some contemporary epistemologists, who extract from it a strategy for dealing with challenges to our knowledge claims based on sceptical possibilities (Pryor 2004). The strategy offers an account of how we can know, contrary to what the sceptic claims, that a sceptical hypothesis doesn't obtain. The proposal is that I can know, for example, that I am not a brain in a vat inferentially, using as (deductive) evidence any of the everyday propositions that entail this, as, for example, the proposition that I have hands. Let me refer to an inference of this kind as a *Moorean inference*.

The claim that Moorean inferences can be genuine cases of inferential knowledge would seem to follow from a plausible sufficient condition for inferential knowledge:

Transmission: If S knows E non-inferentially, S knows that E logically entails H and S believes H, then S knows H inferentially on the basis of evidence provided by E.[1, 2, 3]

* I have presented versions of this material at the Northern Institute of Philosophy and the University of Geneva. I am grateful to these audiences. I am also grateful for their comments to the editors of this volume and an anonymous referee.

[1] By restricting the principle to cases in which E is non-inferentially known we rule out cases of logical circularity.

[2] It could be argued that a plausible transmission principle would need to include a clause to the effect that the subject's belief in H is based on her belief in E. I don't accept this point. See Zalabardo (2012a) for details.

[3] A subject can have both inferential and non-inferential knowledge of a given proposition. This sufficient condition for inferential knowledge might be satisfied in cases in which the subject also knows H non-inferentially.

Let's assume that I can know that envatted brains don't have hands. Then, if I can know that I have hands non-inferentially (say, by sense perception), Transmission will enable us to conclude that the Moorean inference can bestow the status of knowledge on my belief that I am not a brain in a vat.

Is this a good thing? Here we have arguments pulling in opposite directions. On the one hand, having an explanation of how we know that sceptical hypotheses don't obtain is certainly a desirable outcome. And for those who are convinced that no other explanation can work, its appeal will be almost irresistible. On the other hand, our goal in this area cannot be simply to find some hypothesis on the nature of knowledge which, if correct, would explain how we know that sceptical hypotheses don't obtain. We should seek a hypothesis that is as a matter of fact correct—one that provides an accurate representation of how we know the problematic propositions, if we do indeed know them. From this point of view, the proposal is highly counterintuitive. Our intuitions seem to be firmly against the possibility of knowing that I am not a brain in a vat by means of a Moorean inference. However, although intuitions must play an important role in the assessment of a theory of knowledge, other factors must also be taken into consideration, and we should be prepared in principle to accept a theory that yields some counterintuitive results.

The appeal of treating Moorean inferences as cases of knowledge would be drastically reduced if we could find a cogent reason for withholding this treatment from them—a feature that can be argued to pose an obstacle to the ascription of knowledge and is exhibited by these cases but not by unproblematic cases of inferential knowledge. If, on the contrary, we failed to identify an objectionable feature of Moorean inferences, the claim that they can produce knowledge would be harder to resist.

6.2. Transmission Principles

A Moorean inference involves no logical circularity—its conclusion is not among its premises. Nevertheless, many have harboured the suspicion that the way in which the conclusion of a Moorean inference is related to its premise can be legitimately characterized as a form of circularity, not logical, but epistemic, and that epistemic circularity poses as serious an obstacle to inferential knowledge as the logical variety. Clearly, the challenge for this approach is to characterize epistemic circularity as a feature that is (a) present in Moorean inferences and absent from unproblematic cases of inferential knowledge and (b) intuitively incompatible with inferential knowledge.

One appealing proposal in this connection is to say that an inference is epistemically circular when the subject's knowledge of the premises requires that she has independent knowledge of the conclusion. In order to provide an adequate formulation of this proposal, it will help to have at our disposal the concept of *warrant*, understood as the property that turns a true belief into knowledge.[4] More precisely, warrant will be a

[4] The term is used in this sense in Plantinga (1993).

relation between subjects and propositions such that S knows p just in case S believes p, S is true, and S bears to p the warrant relation.[5]

Now one constraint that is widely accepted by contemporary epistemologists is that warrant is not a primitive property—that when a proposition p has warrant for a subject S, there are some different facts in virtue of which p has warrant for S. Let me refer to the facts that play this role as *warrant-constituting facts*. On the assumption that warrant requires warrant-constituting facts, we can provide a formulation of the proposal under consideration. We can say that an inference is epistemically circular when the subject having warrant for the conclusion is among the warrant-constituting facts of her warrant for the premise.[6]

I think it would be hard to deny that if epistemic circularity is characterized in this way, an epistemically circular inference will be incapable of producing knowledge of its conclusion. Inferential knowledge produced by the transmission principle under consideration requires knowledge of the premises, but if an inference is epistemically circular, in this sense, the fact that the subject knows the premises will be grounded in the fact that she knows the conclusion.[7] Hence she will be incapable of using the inference to acquire knowledge of the conclusion, as the intended outcome is among the preconditions of this method of knowledge acquisition.

However, even if we accept, as I propose, that epistemically circular inferences cannot produce knowledge of their conclusions, we won't be able to use this circumstance to argue that Moorean inferences cannot produce knowledge unless we can show that Moorean inferences are epistemically circular. The issue here turns on our explanation of how we know the premise of a Moorean inference, and on many contemporary accounts of knowledge, the claim that Moorean inferences are epistemically circular has no plausibility. Suppose, for example, that I can know that I have hands by virtue of the fact that my belief to this effect tracks the truth (Nozick 1981), or that it was formed with a reliable belief-forming method (Goldman 1986). Then truth tracking, or reliable formation, will be the warrant-constituting fact of my belief that I have hands, and neither truth tracking nor reliable formation includes, or requires, that I have warrant for the proposition that I'm not a brain in a vat. If my belief that I have hands can obtain the status of knowledge in one of these ways, Moorean arguments are not epistemically circular.[8]

[5] Notice that the definition doesn't rule out the possibility that warrant entails belief or truth. Thus, e.g., if warrant is what Nozick calls truth tracking, warrant will entail both.

[6] Crispin Wright (1985, 2000, 2002) has explored several versions of this diagnosis of the inadequacy of Moorean inferences. Notice that on the formulation that I am using, the presence of the warrant-constituting facts for the conclusion among the warrant-constituting facts for the premise doesn't pose a problem.

[7] There may be cases of inferential knowledge in which the premises are not known (Luzzi 2010) or not even true (Warfield 2005).

[8] Versions of this point have been made in Brown (2003) and Pryor (2004). See also my (2012b).

One could try to rescue this approach with a different account of why Moorean inferences are epistemically circular. The idea now would be that what's wrong with a Moorean inference is that the subject's knowledge of the premise presupposes, not that she has knowledge of the conclusion, but that the conclusion is true. We can pursue this strategy with a new account of epistemic circularity. Now an inference will be epistemically circular when the truth of the conclusion is among the warrant-constituting facts of the subject's warrant for the premise.

We could argue that on this characterization of epistemic circularity the standard Moorean inferences are epistemically circular. The facts about me and my relationship to my environment that enable my belief that I have hands to track the truth, or to have been formed reliably, are clearly incompatible with my being a brain in a vat. If I were a brain in a vat my belief that I have hands would not track the truth, and the procedure with which I formed the belief wouldn't be reliable. Suppose it could be argued that my not being a brain in a vat is actually included among the facts that make my belief that I have hands count as tracking the truth or reliably formed. Then my inference from the premise that I have hands to the conclusion that I am not a brain in a vat would count as epistemically circular, on the construal under discussion. Then the prospects of the proposal would turn on the plausibility of claiming that inferences that exhibit epistemic circularity, on this construal, are incapable of producing knowledge of their conclusions.

It has been argued that this claim is incorrect—that there are inferences that exhibit this brand of epistemic circularity but seem perfectly capable of producing knowledge of their conclusions (Davies 1998: 352; Pryor 2004: 358–9). Here I want to argue that there is an additional source of concern for the proposal. The problem on which I want to focus is that the proposal doesn't deal with all the cases of the pathology that afflicts Moore's inference. I am going to present the argument using a process-reliabilist account of non-inferential knowledge, but the argument will work in the same way for some tracking accounts.

Consider Fred Dretske's example of a child—call her Molly—who goes to the zoo and upon seeing the animals in the enclosure marked 'zebras' forms the belief that they are zebras (Dretske 1970). According to process reliabilism, Molly's belief has warrant, since it has been formed with a reliable belief-forming process. Hence, if the belief is true, if the animals are actually zebras, it will have the status of knowledge.

Suppose now that Molly knows that the proposition that the animals are zebras (call it ZEBRAS) entails the proposition that they are not mules cleverly disguised by the zoo authorities to look like zebras (call it ~MULES). It follows from our assumptions and Transmission that Molly can know ~MULES inferentially on the basis of the evidence provided by ZEBRAS. However, this inference seems to be afflicted by the same pathology as the inference from HANDS (the proposition that I have hands) to ~BIV (the proposition that I'm not a brain in a vat). It is plausible to assume that the right verdict of the difficulty that invalidates the latter inference as a case of inferential knowledge will also apply to the former.

However, on the construal of epistemic circularity currently under discussion, the inference from ZEBRAS to ~MULES would not count as epistemically circular. To see this, notice that the reliability of the process involved in the production of Molly's belief in ZEBRAS doesn't require that ~MULES is true, so long as the reliability that is required for knowledge is not perfect reliability. Molly could have formed her belief with highly reliable perceptual devices even if the animals she is looking at are cleverly disguised mules. Of course, the reliability of the relevant perceptual devices requires that this kind of deception is sufficiently rare, but not that it never occurs, or that it is not occurring on this occasion.

I want to treat the fact that the strategy doesn't deal with all the instances of the problem as an indication that its source hasn't been addressed. We haven't identified the feature of the relationship between premise and conclusion of Moorean inferences that explains why they shouldn't be treated as cases of inferential knowledge.

6.3. An Idea from Nozick

I am going to argue that there is a more appealing explanation of why Moorean inferences shouldn't be treated as cases of knowledge. My proposal is based on the intuition that what's wrong with a Moorean inference has to do with the circumstances under which you would believe its premise. The problem is that you would still believe it if its conclusion were false. You can't have inferential knowledge of ~BIV based on the evidence provided by HANDS because envatted brains believe in HANDS, and Molly can't have inferential knowledge of ~MULES based on the evidence provided by ZEBRAS because people who are looking at cleverly disguised mules believe in ZEBRAS. Nozick formulated a principle based on this intuition, as a condition for when inferring q from p yields knowledge of q:

I: If q were false, S wouldn't believe p (or S wouldn't infer q from p) (Nozick 1981: 231).

I believe this principle is along the right lines, but instead of the counterfactual formulation used by Nozick, I want to propose a formulation of the thought in terms of conditional probability. My proposal is that what's wrong with these cases is that you are not less likely to believe your evidence if the hypothesis is false than if it is true. We can reformulate the condition in terms of the notion of incremental confirmation. Evidence E confirms hypothesis H just in case the probability of E given H is higher than the probability of E given ~H.[9] Hence we can say that the problem with a Moorean inference is that the subject's belief in its premise (that is, the fact that she believes the premise) doesn't confirm its conclusion. The proposal is, then, that we can avoid treating Moorean inferences as cases of inferential knowledge by imposing the following condition on inferential knowledge:

PI: S can have inferential knowledge of H based on the evidence provided by E only if S's belief in E confirms H.[10]

[9] This formulation of incremental confirmation is equivalent to the more standard $p(H \mid E) > p(H)$.

[10] A closer analogue of Nozick's principle, taking account of the bracketed clause, would require that H is confirmed either by S's belief in E or by her belief in the proposition that E supports H. I shall not take this line here.

This proposal deals easily with the standard cases. I can't know ~BIV inferentially on the basis of the evidence provided by HANDS because the probability of my believing HANDS is not affected by whether I am a brain in a vat.[11] And Molly can't know ~MULES inferentially on the basis of the evidence provided by ZEBRAS because she is no less likely to believe ZEBRAS if she is looking at cleverly disguised mules than if she isn't.[12]

6.4. Closure and Transmission

Before we proceed, we need to make a distinction between the principle that I have called Transmission and another principle that's implicated in the issues under discussion. Transmission is a principle that stipulates sufficient conditions for inferential knowledge in the specific case of deductive inferences. The other principle that I want to consider has, in the first instance, nothing to do with inferential knowledge in particular, although it also concerns cases in which E entails H. It stipulates that the following four states of affairs are incompatible: (a) S knowing E, (b) S knowing that E logically entails H, (c) S believing H and (d) S not knowing H. It is usually formulated as a conditional:

Closure: If S knows E non-inferentially, S knows that E logically entails H and S believes H, then S knows H.[13]

In spite of the superficial similarities, there are important differences between Transmission and Closure. Notice that Transmission is stronger than Closure. On the one hand, S will know H whenever she knows H inferentially on the basis of evidence provided by E. On the other hand, S could know H in some other way. Hence Closure might be universally valid even if Transmission has counterexamples.

[11] I think that the claim that the probability of my believing HANDS is not affected by whether I am a brain in a vat is part of what we stipulate when we describe the brain-in-a-vat scenario. If this is not, explicitly or implicitly, part of the description of the case, it might turn out that, as a matter of fact, envatted brains are less likely to believe HANDS than normal people. If this were the situation, then my inference form HANDS to ~BIV would satisfy PI. I am not sure whether, in these circumstances, it would be wrong to say that the inference produces knowledge. On this point, see Brueckner (1994: 829).

[12] A word on the notion of probability that I am assuming here. The probabilities that I have in mind are neither logical, a priori discoverable facts about events, nor subjective degrees of belief by actual or ideally rational subjects. They are instead objective, contingent facts about states of affairs, knowable only by empirical investigation. They arise from the nomological order: the probability of states of affairs is determined by the laws of nature (Lewis 1986, 1994). The notion of probability that I am assuming differs from Lewis's in two important respects. First, propositions can have non-trivial probabilities even in a deterministic world. See Hoefer (2007) and Glynn (2010) for proposals as to how to achieve this. Second, propositions about past events can have non-trivial probabilities. This is required by the thought that which evidential relations propositions bear to one another is not affected by whether or not they represent states of affairs in the past. We can achieve this by rejecting Lewis's idea that the history of the universe is taken into account in the determination of probabilities.

[13] The plausibility of the principle does not depend on the restriction to cases in which E is known non-inferentially. I introduce the restriction here to facilitate comparison with the version of Transmission under discussion.

The most important difference between Transmission and Closure for our purposes concerns how they relate to PI. Transmission, on the one hand, is directly incompatible with PI. Whenever S knows E non-inferentially, S knows that E logically entails H and S believes H, Transmission will require that S knows H inferentially on the basis of evidence provided by E. But these conditions are compatible with S's belief in E failing to confirm H, and when this happens PI will rule out inferential knowledge of H.

Hence, the adoption of PI would force us to weaken transmission as follows:

*Transmission**: If S knows E non-inferentially, S knows that E logically entails H, S believes H and S's belief in E confirms H, then S knows H inferentially on the basis of evidence provided by E.

Closure, by contrast, is not directly threatened by PI. If S's belief in E doesn't confirm H, then, according to PI, S won't know H inferentially on the basis of evidence provided by E. But this is compatible with S knowing H, by some other means, and this is all that Closure requires.

Nevertheless, the adoption of PI would also have adverse consequences for Closure. With Transmission in place, the satisfaction of Closure is guaranteed. However, as we have seen, the adoption of PI forces us to replace Transmission with Transmission*, and the latter no longer guarantees the satisfaction of Closure. If S's belief in E doesn't confirm H, Transmission* won't rule out the possibility that the antecedent of Closure is satisfied but its consequent isn't.

In sum, PI is incompatible with Transmission, but compatible both with Closure and with its negation. Hence, PI can be incorporated in a theory of knowledge for which Closure is universally valid. However, this would require ensuring that Closure is satisfied by some other means, since the account of inferential knowledge won't guarantee its satisfaction. I am not going to consider at this point whether we should take this step. Clearly PI will deprive Closure of the support that it might derive from its connection with Transmission, but the principle might be recommended by independent considerations.

6.5. Reflective Knowledge

I want to consider next another type of inference that raises similar issues to those that we have considered in connection with Moorean inferences. Suppose that I read in a reliable newspaper that the Bulls won the game last night and form as a result the belief that the Bulls won the game.[14] On a reliabilist or truth-tracking account of knowledge, if the Bulls did indeed win, my belief to this effect will have the status of knowledge.

Consider now the proposition that the newspaper report was veridical. As we are about to see, there is an issue as to how this proposition should be analysed, but on each plausible analysis it is logically entailed by propositions that I know—the proposition

[14] Keith DeRose discusses this case in his (1995: 18).

that the Bulls won and the proposition that the newspaper report says that the Bulls won. There is also no reason why I shouldn't know this entailment. Assuming that I do, I will know that the proposition that the report was veridical is a logical consequence of propositions that I know. If this is sufficient for inferential knowledge, I will know that the report is veridical inferentially, on the basis of evidence provided by the proposition that the Bulls won the game.

Intuitively this is the wrong result. If the newspaper report is my only source of information for the match result, I cannot use it as evidence to obtain inferential knowledge of the veracity of the report. Notice that I am not arguing that it is impossible to know the veracity of the report or that it is possible to know that the Bulls won without knowing that the report was veridical. All I am arguing is that if knowledge that the report was veridical is to be inferential, then, in the circumstances that I have described, it cannot be based on evidence provided by the proposition that the Bulls won the match. In other words, I am arguing that the inference from the premise that the Bulls won the match to the conclusion that the newspaper report was veridical should be a counterexample to Transmission. I am not taking sides on the question whether we can obtain from this case a counterexample to Closure.

I want to argue that PI succeeds in ruling this out as a case of inferential knowledge. Now, whether this result holds depends on how we analyse the proposition that the newspaper report is veridical. One natural approach is to analyse it as a truth function of the proposition that the Bulls won (BULLS) and the proposition that the newspaper reported that the Bulls won (REPORT). There are at least two plausible options as to how to do this. The first is to analyse it as the proposition that the newspaper didn't falsely report a Bulls' victory, i.e. ~(REPORT & ~BULLS). The second is to treat it as the proposition that the newspaper reported a Bulls victory veridically, i.e. REPORT & BULLS.

Which of these options we take is going to make a difference to whether the inference contravenes PI. Consider first REPORT & BULLS. My evidence for this proposition will consist of the propositions REPORT and BULLS. Hence, in order to assess the inference from the point of view of PI we need to determine whether p(Bel(REPORT) & Bel(BULLS) | REPORT & BULLS) is greater than p(Bel(REPORT) & Bel(BULLS) | ~(REPORT & BULLS)). I think it's clear that the answer is yes. Notice that ~(REPORT & BULLS) is the same proposition as ~REPORT $\vee$ (REPORT & ~BULLS).[15] We can assume that p(Bel(REPORT) & Bel(BULLS) | REPORT & ~BULLS) is the same as p(Bel(REPORT) & Bel(BULLS)) | REPORT & BULLS), since the newspaper report is my only source of information about the game result. However, p(Bel(REPORT) & Bel(BULLS) | ~REPORT) can be expected to be much lower, since I am unlikely to believe in a nonexistent newspaper report. Hence, to show that p(Bel(REPORT)

[15] I am assuming, for simplicity, that propositions are individuated semantically, i.e. up to logical equivalence.

& Bel(BULLS) | REPORT & BULLS) is greater than p(Bel(REPORT) & Bel(BULLS) | ~(REPORT & BULLS)), it will suffice to show that

p(Bel(REPORT) & Bel(BULLS) | REPORT & ~BULLS)>
p(Bel(REPORT) & Bel(BULLS) | ~REPORT)

entails

p(Bel(REPORT) & Bel(BULLS) | REPORT & ~BULLS)>
p(Bel(REPORT) & Bel(BULLS) | ~REPORT ∨ (REPORT & ~BULLS))

This can be easily shown (see Appendix).

I want to suggest that the way in which this result is obtained should render the analysis suspect. The reason why PI is satisfied is due entirely to the low probability of my belief in REPORT if REPORT is false. But intuitively this circumstance should not affect the adequacy of a piece of evidence as support for the veridicality claim. It seems that this should be assessed exclusively in terms of how the probability of my belief in the evidence is affected by whether or not the report is veridical. The situation in which the report doesn't exist shouldn't come into play.

Let's consider now the other proposal as to how to analyse the veridicality claim as a truth function of REPORT and BULLS, i.e. to take it as ~(REPORT & ~BULLS). Notice first of all that now REPORT is no longer needed as a premise, since ~(REPORT & ~BULLS) follows from BULLS alone.[16] Hence, in order to determine whether the inference satisfies PI on this construal, it will suffice to compare p(Bel(BULLS) | ~(REPORT & ~BULLS)) with p(Bel(BULLS) | REPORT & ~BULLS). I am going to argue that p(Bel(BULLS) | ~(REPORT & ~BULLS)) is actually smaller than p(Bel(BULLS) | REPORT & ~BULLS), contrary to what PI calls for.

Notice first that on the assumption that I have no source of information about the match result other than the newspaper report, we have that p(Bel(BULLS) | REPORT & ~BULLS) = p(Bel(BULLS) | REPORT & BULLS) = p(Bel(BULLS) | REPORT). Hence it will suffice to show that p(Bel(BULLS) | ~(REPORT & ~BULLS)) is smaller than p(Bel(BULLS) | REPORT & BULLS).

Notice that p(Bel(BULLS) | ~(REPORT & ~BULLS)) can be rewritten as p(Bel(BULLS) | ~REPORT ∨ (REPORT & BULLS)). Now, clearly, p(Bel(BULLS) | ~REPORT) is smaller than p(Bel(BULLS) | REPORT & BULLS), given my propensity to believe the report. Hence (see Appendix), we have that p(Bel(BULLS) | ~(REPORT & ~BULLS)) is smaller than p(Bel(BULLS) | REPORT & BULLS), as desired. We can conclude that Bel(BULLS) doesn't confirm ~(REPORT & ~BULLS), and hence that on this construal of the veridicality claim, the inference is ruled out by PI.

I suggested above that this is the outcome that is in line with our intuitions about this kind of case. However, this construal is open to the same objection as the previous

[16] REPORT is, of course, my evidence for BULLS. The point I'm making here is that, as REPORT is ineffectual as evidence for ~(REPORT & ~BULLS).

one, since we are still taking into account the probability that I believe the evidence if the report doesn't exist. Inspection of the argument shows that p(Bel(BULLS) | ~(REPORT & ~BULLS)) is dragged down by the relatively low value of p(Bel(BULLS) | ~REPORT), i.e. by the low probability that I believe that the Bulls have won in the absence of the report. Hence the reason that I have offered for rejecting the previous proposal cannot be used as a reason for preferring this alternative. If the right analysis of the veridicality claim cannot make the admissibility of a piece of evidence depend on what I would believe if the report didn't exist, the second proposal is as inadequate as the first.[17]

I want to try a different approach to the analysis of veridicality claims. My proposal is to analyse the proposition that the newspaper report is veridical as ascribing a predicate (veridical) to an individual picked out by a definite description (the newspaper report of the Bulls' victory). Thus, if V stands for ... *is veridical*, and R stands for ... *is a (unique) newspaper report asserting that the Bulls won the match*, the veridicality proposition can be symbolised as V ℩x Rx. Clearly, REPORT and BULLS logically entail V ℩x Rx. Hence, according to Transmission*, I will be able to have inferential knowledge of V ℩x Rx on the basis of the evidence provided by REPORT and BULLS unless the case is ruled out by PI.

In order to determine whether this inference satisfies PI, we need to compare p(Bel(REPORT) & Bel(BULLS) | V ℩x Rx) with p(Bel(REPORT) & Bel(BULLS) | ~V ℩x Rx). PI will be satisfied just in case the former is greater than the latter. The issue turns on the familiar ambiguity of scope afflicting ~V ℩x Rx. If we take the definite description to have narrow scope, the proposition will be true if the report is not veridical, or if it doesn't exist (or if it's not unique). If we take it to have wide scope, the proposition will be true just in case the report is not veridical, i.e. just in case there exists a (unique) newspaper report asserting that the Bulls won the game and this report is not veridical.

Given the source of our dissatisfaction with previous analyses of veridicality, it should be clear that the wide-scope reading is to be preferred.[18] Adopting the narrow-scope reading would assign a role in the assessment of my evidence for the veridicality proposition to how likely I am to believe the evidence if the report doesn't exist. With the wide-scope reading, however, this factor is completely excluded. Now the assessment of my evidence will depend exclusively on what I am likely to believe if the report is veridical and if it is not veridical, as intuition recommends.

Once the veridicality claim is analysed in this way, it is clear that my evidence for it doesn't satisfy PI. The probability of my believing REPORT and BULLS is unaffected

[17] Elia Zardini has suggested to me that these analyses of the veridicality proposition can be rejected on independent grounds. On the one hand, REPORT & BULLS obviously entails BULLS, but it could be argued that this shouldn't count as a logical consequence of the proposition that the newspaper report is veridical. On the other hand, ~(REPORT & ~BULLS) is compatible with the proposition that the newspaper falsely reported that the Lakers won, unlike the proposition that the report is veridical.

[18] Notice that, on the wide-scope reading, p(A | V ℩x Rx) > p(A | ~V ℩x Rx) is no longer equivalent to p(V ℩x Rx | A) > p(V ℩x Rx).

by whether or not the report is veridical. This is the reason why the information that I have obtained from the report cannot be used as evidence of its veridicality.[19]

6.6. Not Falsely Believing

Similar issues are raised by beliefs concerning the truth value of my own (current) beliefs. Take, for example, my belief that I don't falsely believe A, where A is a proposition that I also believe. How could this belief acquire the status of knowledge? Here I want to discuss one possible answer to this question—the view that I can know that I don't falsely believe A inferentially on the basis of the evidence provided by A. Clearly A logically entails the proposition that I don't falsely believe A. Hence, if we assume that I know this entailment, and that I know A non-inferentially, the antecendent of Transmission will be satisfied. Therefore, whether Transmission* treats this as a case of inferential knowledge will depend on whether PI is satisfied.

I want to suggest that this inference is intuitively objectionable for exactly the same reason as the inferences that we have already considered. I argued that the inference from HANDS to ~BIV cannot produce knowledge of its conclusion because if the conclusion were false, if I were a brain in a vat, I would still believe the premise. Similarly, I can't know that the newspaper report is veridical inferentially on the basis of the evidence provided by the Bulls' victory because if the conclusion were false—if the report were not veridical—I would still believe the evidence. The inference from A to *I don't falsely believe A* provides an extreme example of this situation: if the conclusion were false—if I falsely believed A, I would, of necessity, still believe the premise, since it's not possible to falsely believe A without believing A. This seems to me to be a powerful intuitive reason for rejecting this inference as a legitimate source of knowledge.

Propositions to the effect that a proposition is not falsely believed do not allow us to concentrate exclusively on knowledge of the truth values of beliefs, since the proposition ~(Bel(A) & ~A) is true not only when A is truly believed, but also when A is not believed. A similar problem afflicts propositions of the form Bel(A) & A, which are false, not only when A is falsely believed, but also when it is not believed.

We face in effect the same situation as in the case of reflective knowledge, and I propose to adopt the same strategy. This involves concentrating on propositions that ascribe a predicate (…*is true*) to an object identified with a definite description (…*is a belief of mine with A as its content*), and assuming that in its negation the description has wide scope. Thus if V stands now for…*is true* and B stands for…*is a belief of mine with A as its content*, the proposition will be symbolized as V ɿx Bx. The idea of knowing V ɿx Bx inferentially on the basis of evidence provided by A is as unattractive as the

[19] Elia Zardini has suggested to me an alternative strategy for discounting the effect of what I am likely to believe if the newspaper report doesn't exist. Zardini's proposal is to consider whether PI is satisfied on the assumption that p(REPORT) = 1. It is easy to see that, on this assumption, my inference for the conclusion that the report is veridical violates PI on both truth-functional construals of this conclusion.

idea of knowing in this way that I don't falsely believe A, but since it is in general possible to know A and to know that A entails V ɿx Bx, the inference will be able to produce knowledge, according to Transmission*, unless it violates PI. But we can see easily that it does. To see this, we need to compare p(Bel(A) | V ɿx Bx) with p(Bel(p) | ~V ɿx Bx). Satisfying PI would require the former to be greater than the latter, but this is impossible, since p(Bel(A) | ~V ɿx Bx) = 1: I cannot falsely believe that A without believing that A. Notice also that PI doesn't just manage to rule out the case that we want to rule out. It also reflects the source of our intuitive reluctance to accept that knowledge of the truth value of my belief in A can result from an inference from A. The problem is that I wouldn't be less likely to believe the premise if the conclusion were false than if it were true.

6.7. Bootstrapping

I want to turn now to another form of inference that has received considerable attention in this connection. In an example that Jonathan Vogel borrows from Michael Williams, Roxanne forms the belief that the petrol tank in her car is full (FULL) whenever she sees that the gauge on the dashboard reads F (GAUGE) (Vogel 2000). Her gauge is highly reliable. Hence, if reliable formation or truth tracking is sufficient for knowledge, then the true beliefs that Roxanne forms in this way will have to be accorded the status of knowledge, even though she has no evidence of the reliability of the gauge. Now suppose that when Roxanne sees that the gauge reads F, in addition to coming to believe FULL, she comes to believe GAUGE, i.e. that the gauge reads F. Let's assume that Roxanne can come to know GAUGE in this way.

What would an inductive argument for the reliability of the gauge look like? Notice first that the claim that the gauge is reliable can be understood as the claim that the gauge reading F provides adequate evidence for the hypothesis that the tank is full. Suppose that we cash out this notion in terms of incremental confirmation, construed, as I think it should be (Zalabardo 2009), as a lower bound on the *likelihood ratio*: p(E | H) / p(E | ~H), written LR(H, E). Then the claim that the gauge is reliable is the claim that LR(FULL, GAUGE) is sufficiently high. An inductive argument for this conclusion would seek to derive it from premises concerning observed frequencies. Thus, from the premise that the proportion of F readings to be found among the observed cases in which the tank is full is considerably higher than the proportion of F readings to be found among the observed cases in which the tank is not full, the argument would conclude that LR(FULL, GAUGE) is high.

Let's assume that the premises of this argument provide adequate support for their conclusion—that evidence concerning observed frequencies provides, in suitable circumstances, adequate support for conclusions about probabilities—and let's assume that Roxanne knows this. Clearly, Roxanne could also know the premises of the argument. Her true beliefs as to whether or not the gauge reads F can have the status of knowledge, and since the gauge is reliable, the true beliefs about the contents of the tank that she forms with the help of the gauge will have to be treated as knowledge by reliabilist and truth-tracking accounts. It follows that Roxanne will have knowledge of the observed relative frequencies that figure in the premises of the argument.

Consider now a non-deductive version of Transmission: if (a) S knows E, (b) S knows that E provides adequate non-deductive support for H and (c) S believes H, then S knows H inferentially on the basis of the information provided by E. If we accepted this principle, we would have to conclude that her inference enables Roxanne to know that the gauge is reliable.

This is a highly counterintuitive outcome. Roxanne cannot use this inference to gain knowledge of the reliability of the gauge. The problem with her procedure doesn't concern the argument itself, or the epistemic status of Roxanne's belief in the premises or in the connection between premises and conclusion. Hence accommodating our intuitive rejection of this form of knowledge acquisition would require arguing that even though Roxanne knows the premises of the argument and she knows that the premises support the conclusion, there is another condition on inferential knowledge that she fails to meet.

Much of the recent literature on this topic assumes that we could avoid counting Roxanne as coming to know with her inductive argument that the gauge is reliable only if we invoked a principle that treats knowledge of the reliability of the gauge as a precondition for obtaining from the gauge knowledge of the contents of the tank (Cohen 2002; Van Cleve 2003). But principles along these lines have been accused of leading directly to scepticism and of being incompatible with reliabilist theories of knowledge. I have argued elsewhere that the problems faced by these principles are not as serious as they might seem at first (Zalabardo 2005). But here I want to present a different strategy for ruling out Roxanne's inference as a case of inferential knowledge.

It seems to me that the most intuitive explanation of the inadequacy of Roxanne's inference focuses on the fact that the gauge is the only method at her disposal for ascertaining whether the tank is full. This circumstance should pose no obstacle to her beliefs about the contents of the tank having the status of knowledge, but it should rule out these beliefs as premises in an inference for the reliability of the gauge. I want to argue that the reason why this feature of Roxanne's situation poses a problem is that it severs the connection between Roxanne's belief in the premises and the truth value of the conclusion. The problem is, once more, that PI is not satisfied: Roxanne is no less likely to believe the premises of the argument if the conclusion is false than if it's true. Given Roxanne's state of information, the probability that she will believe the premises of her inductive argument for the reliability of the gauge is not affected by the value of LR(FULL, GAUGE). She will be just as likely to believe that the observed cases in which the gauge reads F are precisely the cases in which the tank is full if these values are low as if they are high.

On Vogel's construal, Roxanne's inductive argument for the reliability of the gauge involves, for every time t at which she forms belief in GAUGE and FULL in the way described, a lemma to the effect that the gauge is reading accurately on that occasion (ACCURATE). Vogel suggests that the epistemic status of Roxanne's belief in ACCURATE is already problematic, but since she has validly inferred ACCURATE from GAUGE and FULL, he thinks that any shortcoming of the epistemic status of her belief in ACCURATE would also have to affect her belief in FULL (assuming that her knowledge of GAUGE is above suspicion). I share Vogel's misgivings about the epistemic status of Roxanne's belief in ACCURATE. I don't think she can know this proposition inferentially on the basis of the evidence provided by GAUGE and FULL. But my proposal has the resources for securing this result without withholding from Roxanne's belief in FULL the status of knowledge.

We can use the ideas that we presented in our discussion of reflective knowledge to explain why this inference should be ruled out as a case of inferential knowledge. Notice that ACCURATE can be construed as an instance of the veridicality propositions that we considered there. If R denotes a plausible description of the reading, and V is the predicate that ascribes accuracy to it, ACCURATE can be analysed as V ℩x Rx. Hence, in order to determine whether Roxanne's inference gives her inferential knowledge of ACCURATE, we need to compare p(Bel(GAUGE) & Bel(FULL) | V ℩x Rx) with p(Bel(GAUGE) & Bel(FULL) | ~V ℩x Rx). As I argued in Section 6.5, the description in ~V ℩x Rx should be understood as having wide scope. Hence the question that we need to ask is whether Roxanne is less likely to believe GAUGE and FULL if the gauge is reading inaccurately (i.e. GAUGE & ~FULL) than if it is reading accurately (i.e. GAUGE & FULL). And this question should be answered in the negative. So long as GAUGE is true, the probability that Roxanne believes GAUGE and FULL will be high, and unaffected by the truth value of FULL.

Contrast this situation with one in which Roxanne can ascertain whether or not the tank is full independently of the gauge, say, using a dipstick. Intuitively this would make all the difference to Roxanne's ability to gain knowledge of the reliability of the gauge from the argument. And PI registers this difference. In this new scenario, a low value for LR(FULL, GAUGE) will decrease the probability that she believes that the gauge reads F more often in the cases in which the tank is full than in those in which it isn't full. If LR(FULL, GAUGE) is low, i.e. if p(GAUGE | FULL) is not much higher than p(GAUGE | ~FULL), it is likely that there will be nearly as many observed cases in which the gauge reads F among the cases in which the tank is not full as among those in which it is full, and Roxanne, with her dipstick, will be able to detect this. These points about her inference for the reliability of the gauge can also be applied to her inference for the proposition that the gauge is reading accurately on a given occasion. Armed with her dipstick, Roxanne will be more likely to believe GAUGE and FULL if the gauge is reading accurately than if it isn't.[20]

[20] PI will not rule out some inferences that appear illegitimate. Consider, e.g., the inference from HANDS to the proposition that I believe I have hands (Bel(HANDS)) and I'm not a brain in a vat. Suppose that the correlation between HANDS and Bel(HANDS) if I'm not a brain in a vat is such that HANDS provides adequate support for Bel(HANDS) & ~BIV. The inference will satisfy PI so long as p(Bel(HANDS)

6.8. Roush on Inferential Knowledge

In her recent book (2005), Sherrilyn Roush has defended an account of knowledge for which she uses the label *recursive tracking*. She presents the aspect of her position on which I want to concentrate in the following passage, where she uses the term *Nozick-knows* to refer to the knowledge that results from truth tracking:

> . . . on the new view Nozick-knowing is not the only way to know. From what we Nozick-know we can get by known implication to other beliefs that are also knowledge. Thus, to analyze the concept of knowledge I combine the notion of Nozick-knowing with a recursion clause: For subject S and proposition p, S *knows* that p if and only if:
>
> S Nozick-knows that p
>
> or
>
> p is true, S believes p, and there is a q not equivalent to p such that q implies p, S knows that q implies p, and S knows that q.
>
> According to this analysis, anything that you derive from something you Nozick-know by n steps of deduction, for some finite n, is also something you know. (Roush 2005: 42–3)

This is only a preliminary formulation of Roush's highly sophisticated proposal, but it adequately highlights the features on which I want to focus.

There are some aspects of this position that I find very appealing. I agree with Roush that truth tracking should be treated as a sufficient condition for knowledge, but we should be able to have inferential knowledge in cases in which we don't track the truth. There are, however, two important respects in which my views differ from hers.

The first is that her recursion clause contemplates inferential knowledge involving only *deductive* inference. Notice that this feature of Roush's account doesn't entail that knowledge cannot be acquired by non-deductive inference. What it does entail is that this will be possible only when as a result of the inferential process the subject's belief in the conclusion comes to track the truth. Roush is explicit about this: 'it is clear that on this view all inductive routes to knowledge must be such that through them we satisfy the tracking conditions' (Roush 2005: 52). I find this aspect of Roush's position unsatisfactory. It seems to me that non-deductive inference should also enable us to obtain inferential knowledge of propositions whose truth we don't track, but I am not going to defend this point here.

The second aspect of Roush's position that I find unsatisfactory is directly connected to the issues that I have discussed in this paper. Roush is prepared to accept as knowledge all cases in which I know that a proposition I believe is deductively entailed by

| Bel(HANDS) & ~BIV) is greater than p(Bel(HANDS) | ~(Bel(HANDS) & ~BIV)), but p(Bel(HANDS) | Bel(HANDS) & ~BIV) equals one, and p(Bel(HANDS) | ~(Bel(HANDS) & ~BIV)) will be less than that. Notice that if we decided to treat this inference as legitimate we wouldn't be forced to accept that I can know ~BIV inferentially. I can't know ~BIV inferentially on the basis of the evidence provided by Bel(HANDS) & ~BIV, since the inference doesn't satisfy PI. I am no less likely to believe this evidence if I am a brain in a vat than if I'm not. See, in this connection, Nozick's discussion of the possibility of knowing a conjunction without knowing each of its conjuncts (Nozick 1981: 228). If we weren't prepared to take this route, we would have to settle for treating PI as a partial diagnosis of the family of difficulties that we have discussed.

known evidence, whereas I have argued that we shouldn't treat in this way cases in which PI is not satisfied. As we have seen, the main consequence of this restriction is to rule out three types of case: Moorean inferences, inferences from a belief to the veridicality of its source (or to the truth of the belief), and inductive-bootstrapping arguments. Roush discusses all these cases in some detail.

Concerning Moorean inferences, there can be no question that it follows from Roush's recursive-tracking account of knowledge that if my belief in HANDS tracks the truth, and I know that HANDS entails ~BIV, then I know ~BIV. Furthermore, on her account, knowledge of ~BIV would be 'gained via known implication from beliefs that are already knowledge' (Roush 2005: 51).

Nevertheless, Roush seems reluctant to accept this consequence of her view. She writes:

> According to this view of knowledge I may know that there is a table in front of me, in which case I also know that I am not a brain in a vat (by known implication), or I may not know that there is a table in front of me, because I do not know that I am not a brain in a vat. Recursive tracking does not determine which of these positions one must adopt . . . (Roush 2005: 55)

Her choice of everyday proposition in this passage is unfortunate, since the proposition that there is a table in front of me does not entail ~BIV, but let's assume for the sake of the argument that the entailment holds. My main point about this passage is that if I believe that my belief that there is a table in front of me tracks the truth, then, contrary to what Roush suggests, recursive tracking does tell me which of these propositions to adopt: I have to believe that I also know ~BIV (by known implication). If Closure is treated as an independent constraint on our knowledge ascriptions, then it is indeed neutral as between the two options that Roush describes. But the same cannot be claimed for recursive tracking. If I believe that my belief in HANDS tracks the truth, and that I know that HANDS entails ~BIV, then recursive tracking doesn't leave me the option of saying that I don't know HANDS because I don't know ~BIV. It forces me to say that I know HANDS (by tracking) and ~BIV (by known implication).

Roush's discussion of Moorean inferences reveals another important consequence of her account. It seems natural to suppose that, when a proposition p is known inferentially, the evidence on which this knowledge is based will provide the subject with adequate reasons or justification for p. Roush, however, doesn't expect her inductive clause to throw any light in general on the justificatory status of beliefs. She writes:

> . . . though we may, and ordinarily think we do, have knowledge that we are not brains in vats, we lack, and will always lack, the ability to offer justification for such claims. This would mean that there can be knowledge without justification, a view that I hold on other grounds . . . (Roush 2005: 56)

On her view, knowledge of ~BIV is gained by known implication from HANDS, even though HANDS provides no justification for ~BIV. It seems to me that it would be desirable to preserve the link between justification and inferential knowledge, and

I want to suggest that PI is a step in this direction, as the inferences that violate it don't seem to provide the subject with justification for their conclusions.

Concerning reflective knowledge, once again, it seems hard to deny that recursive tracking dictates that, if my belief that p tracks the truth, and I know that p entails that my belief that p is true,[21] I will count as knowing that my belief that p is true by known implication. Roush accepts that I can come to know that my belief that p is true in this way for any p that I know, but once again she seems reluctant to accept this consequence of her view. This reluctance is manifested in her discussion of someone's belief that there is no motion of the earth relative to the ether. Assume that this belief is knowledge and that the subject believes that it is not false. Roush writes:

> Still, though it does seem possible that her reflective belief is knowledge we seem to need to know more than that she knows p in order to see her as knowing that she does not falsely believe p, even when she believes the latter. . . . I conclude that it ought to follow from a view of knowledge that there are ways of acquiring the reflective knowledge in question but that it is not automatic, and less effort may be needed for this in the case of easily known statements like 'I have hands' than is required for more elaborate beliefs whose status as knowledge itself required much more deliberate effort (on the part of someone, not necessarily the subject). (Roush 2005: 60)

I agree that it ought to follow from a view of knowledge that the acquisition of reflective knowledge should not be automatic. However it is clear that Roush's view does not satisfy this requirement. It follows from her view that if I know p, and I believe that my belief that p is true (or not false), the reflective belief will have the status of knowledge so long as I know that p entails the corresponding reflective belief. It seems to me that knowledge of this entailment is a sufficiently weak requirement for the resulting reflective knowledge to count as automatic, since knowledge of the entailment would seem to be required for possessing the concept of true belief, which is required, in turn, for having reflective beliefs. At any rate, knowledge of the entailment won't be harder in cases in which knowledge of p requires deliberate efforts than in cases in which p is easily known, as Roush thinks it should be.

Notice that this route to automatic reflective knowledge is blocked by PI. Knowing p and knowing that p entails that my belief that p is true is not sufficient for inferential knowledge of the reflective proposition, since, as we have seen, p does not provide me with adequate evidence for the proposition that my belief that p is true.

Let's turn now to bootstrapping inferences for reliability claims. As Roush explains, recursive tracking is not committed to the view that these inferences confer the status of knowledge on their conclusions. Roxanne's belief that the petrol gauge in her car is reliable doesn't come to track the truth as a result of her inference, and it doesn't acquire the status of knowledge by known implication either, since as Roush points out, the inference involves non-deductive steps.

[21] Here and elsewhere I am assuming that I know that I have the belief whose truth value is at issue.

Notice, however, that recursive tracking *is* committed to conferring the status of knowledge on Roxanne's beliefs to the effect that the gauge is reading accurately on particular occasions. As Roush puts it,

> The steps of S's procedure that are deductive—conjunction and the inference from 'F and the gauge says "F"' to 'the gauge was accurate this time'—cannot be objectionable to recursive tracking, which allows that knowledge is preserved by deduction. (Roush 2005: 120)

I have argued above that this outcome is in conflict with our intuitions. Roxanne might know these propositions, and knowing them might be required for gaining knowledge from the gauge about the contents of the tank. What I find implausible is the idea that Roxanne can come to know that the gauge is reading accurately by virtue of the fact that she knows this proposition to be entailed by the propositions that the gauge is reading F and the tank is full, which she also knows. PI explains why this account of how Roxanne knows that the gauge is reading accurately cannot be right. Recursive tracking, by contrast, treats it as the right account.

Appendix

Theorem: If $p(A \mid B) > p(A \mid C)$ and B & C is logically false, then $p(A \mid B) > p(A \mid C \vee B)$.
Proof:

$p(A \mid B) > p(A \mid C)$

↓ (by the definition of conditional probability)

$$\frac{p(A \,\&\, B)}{p(B)} > \frac{p(A \,\&\, C)}{p(C)}$$

↓

$p(A \,\&\, B) \cdot p(C) > p(A \,\&\, C) \cdot p(B)$

↓

$p(A \,\&\, B) \cdot p(C) + p(A \,\&\, B) \cdot p(B) > p(A \,\&\, C) \cdot p(B) + p(A \,\&\, B) \cdot p(B)$

↓

$p(A \,\&\, B) \cdot (p(C) + p(B)) > p(B) \cdot (p(A \,\&\, C) + p(A \,\&\, B))$

↓

$$\frac{p(A \,\&\, B) \cdot (p(C) + p(B))}{p(B) \cdot (p(C) + p(B))} > \frac{p(B) \cdot (p(A \,\&\, C) + p(A \,\&\, B))}{p(B) \cdot (p(C) + p(B))}$$

↓

$$\frac{p(A \,\&\, B)}{p(B)} > \frac{p(A \,\&\, C) + p(A \,\&\, B)}{p(C) + p(B)}$$

↓ (by the addition axiom, since C & B is logically false)

$$\frac{p(A \& B)}{p(B)} > \frac{p((A \& C) \vee (A \& B))}{p(C) + p(B)}$$

↓

$$\frac{p(A \& B)}{p(B)} > \frac{p(A \& (C \vee B))}{p(C) + p(B)}$$

↓ (by the addition axiom, since C & B is logically false)

$$\frac{p(A \& B)}{p(B)} > \frac{p(A \& (C \vee B))}{p(C \vee B)}$$

↓ (by the definition of conditional probability)

$p(A \mid B) > p(A \mid B \vee C)$.

References

Brown, Jessica (2003). 'The Reductio Argument and Transmission of Warrant.' In S. Nuccetelli (ed.), *New Essays on Semantic Externalism and Self-Knowledge*, 117–30. Cambridge, Mass.: MIT Press.

Brueckner, Anthony (1994). 'The Structure of the Skeptical Argument.' *Philosophy and Phenomenological Research*, 54: 827–35.

Cohen, Stewart (2002). 'Basic Knowledge and the Problem of Easy Knowledge.' *Philosophy and Phenomenological Research*, 65: 309–29.

Davies, Martin (1998). 'Externalism, Architecturalism, and Epistemic Warrant.' In C. Wright, B. Smith, and C. Macdonald (eds.), *Knowing Our Own Minds*, 321–61. Oxford: Oxford University Press.

DeRose, Keith (1995). 'Solving the Skeptical Problem.' *Philosophical Review*, 104: 1–52.

Dretske, Fred (1970). 'Epistemic Operators.' *Journal of Philosophy*, 67: 1007–23.

Glynn, Luke (2010). 'Deterministic Chance.' *British Journal for the Philosophy of Science*, 61: 51–80.

Goldman, Alvin I. (1986). *Epistemology and Cognition*. Cambridge, Mass.: Harvard University Press.

Hoefer, Carl (2007). 'The Third Way on Objective Probability: A Sceptic's Guide to Objective Chance.' *Mind*, 116: 549–96.

Lewis, David (1986). 'A Subjectivist's Guide to Objective Chance.' In *Philosophical Papers, Volume II*, 83–113. Oxford: Oxford University Press.

—— (1994). 'Humean Supervenience Debugged.' *Mind*, 103: 473–90.

Luzzi, Federico (2010). 'Counter-Closure.' *Australasian Journal of Philosophy*, 88: 673–83.

Moore, G. E. (1939). 'Proof of an External World.' *Proceedings of the British Academy*, 25: 273–300.

Nozick, Robert (1981). *Philosophical Explanations*. Cambridge, Mass.: Harvard University Press.

Plantinga, Alvin (1993). *Warrant: The Current Debate*. New York and Oxford: Oxford University Press.

Pryor, James (2004). 'What's Wrong With Moore's Argument?' *Philosophical Issues*, 14: 349–78.

Roush, Sherrilyn (2005). *Tracking Truth*. Oxford: Oxford University Press.

Van Cleve, James (2003). 'Is Knowledge Easy—or Impossible? Externalism as the Only Alternative to Skepticism.' In S. Luper (ed.), *The Skeptics: Contemporary Essays*, 45–59. Aldershot: Ashgate.

Vogel, Jonathan (2000). 'Reliabilism Leveled.' *Journal of Philosophy*, 97: 602–23.

Warfield, Ted A. (2005). 'Knowledge from Falsehood.' *Philosophical Perspectives*, 19: 405–16.

Wright, Crispin (1985). 'Facts and Certainty.' *Proceedings of the British Academy*, 71: 429–72.

—— (2000). 'Cogency and Question-Begging: Some Reflections on McKinsey's Paradox and Putnam's Proof.' *Philosophical Issues*, 10: 140–63.

—— (2002). '(Anti-)Sceptics Simple and Subtle: G. E. Moore and John McDowell.' *Philosophy and Phenomenological Research*, 65: 330–48.

Zalabardo, José L. (2005). 'Externalism, Skepticism and the Problem of Easy Knowledge.' *Philosophical Review*, 114: 33–61.

—— (2009). 'An Argument for the Likelihood-Ratio Measure of Confirmation.' *Analysis*, 69: 630–5.

—— (2012a). *Scepticism and Reliable Belief*. Oxford: Oxford University Press.

—— (2012b). 'Wright on Moore.' In A. Coliva (ed.), *Wittgenstein, Epistemology and Mind: Themes from the Philosophy of Crispin Wright*. 304–22. Oxford: Oxford University Press.