

Epistemic closure, skepticism and defeasibility

Claudio de Almeida

Received: 11 March 2011 / Accepted: 11 March 2011 / Published online: 24 March 2011
© Springer Science+Business Media B.V. 2011

Abstract Those of us who have followed Fred Dretske’s lead with regard to epistemic closure and its impact on skepticism have been *half-wrong* for the last four decades. But those who have opposed our Dretskean stance, contextualists in particular, have been *just wrong*. We have been half-right. Dretske rightly claimed that epistemic status is not closed under logical implication. Unlike the Dretskean cases, the new counterexamples to closure offered here render every form of contextualist pro-closure maneuvering useless. But there is a way of going wrong under Dretske’s lead. As the paper argues, Cartesian skepticism thrives on closure failure in a way that is yet to be acknowledged in the literature. The skeptic can make do with principles which are weaker than the familiar closure principles. But I will further claim that this is only a momentary reprieve for the skeptic. As it turns out, one of the weaker principles on which a skeptical *modus tollens* must rest can be shown false.

Keywords Epistemic closure · Deductive closure · Skepticism · Defeasibility · Contextualism · Epistemology of reasoning · Fred Dretske · Peter Klein

We have been led to believe, from time immemorial, that both epistemic rationality and knowledge piggyback on monotonicity in cases of deductive reasoning—that is to say, that valid deductive reasoning infallibly transmits epistemic status from premises to conclusions.¹ Speaking for so many philosophers since the dawn of logic itself, Olin (2003, p. 9) efficiently expresses the view as follows.

¹ I adhere to the standard practice of using the terms “epistemic rationality” and “epistemic justification” interchangeably.

Valid arguments do not share with inductive arguments the feature of [epistemic] defeasibility. Given premises we are entitled to accept as true, valid reasoning will yield a conclusion we are entitled to accept as true, *no matter what further information we have*.²

Although Edmund Gettier is often credited with having been the first to claim that epistemic justification is infallibly transmitted by valid reasoning, it is hard to understand why anybody would *ever* have cared as much as we do for the concept of validity if it were believed that truth alone is closed under logical implication. Tacit acceptance of epistemic closure principles is ancient beyond memory or record. Authors who still subscribe to the view are, literally, too numerous to mention.³ This is, as we know, in spite of the alarm sounded against the view by Fred Dretske forty years ago.⁴ Decades later, it is safe to say that the Dretskean opposition to the view is no louder than a whisper in the contemporary scene. And that is, as we also know, largely due to the resounding success of a contextualist pro-closure move initiated by [Stine \(1976\)](#) and carried further in the works of Stewart Cohen, among others. Epistemic-closure-preservation has been used as a major selling point for contextualism in epistemology.

In defense of epistemic closure, contextualists were joined by invariantist Peter Klein at an early stage of the debate. The resulting, impressive pro-closure armada has since shown so much more firepower than the Dretskean opposition that one must scramble to find any recent achievement by that Brancallean army of Dretske loyalists.⁵

Well, if I'm not much mistaken, I have news for you from the Dretskean ranks. I want to give you a new opportunity to see that Dretske was right in the main, that both the contextualist and invariantist pro-closure moves fail, but also that Dretske was never, after all, completely right. That takes some stage-setting, however. So, let me briefly recap the crucial moments in the closure drama.

1 Closing in on closure

Act 1: Dretske's assault on closure was framed, from the start, with a view to drawing major conclusions as regards Cartesian skepticism. Not only did he put us in a position to see that it is a fundamental mistake in the epistemology of reasoning to think that valid reasoning is epistemically indefeasible—though, again, he didn't quite put it

² In her 2005 paper on the issue, Olin develops her own case against epistemic closure. I trust she would now want to reconsider this claim from her 2003 book.

³ Authors who have *explicitly* subscribed to the view that valid reasoning is epistemically indefeasible include John Pollock and Joseph Cruz, Michael Williams, Matthias Steup, and Mark Sainsbury. Quotations from these sources are offered in my [2007](#).

⁴ Unfortunately, Dretske doesn't make the point as generally as it can be made, as the point that valid reasoning with premises that are at least justified (if not instances of knowledge) is epistemically defeasible too. His point is given as the more narrow one about epistemic operators. See Dretske's seminal [1970](#) paper and his [2005](#) restatement of his anti-closure arguments.

⁵ In addition to [Nozick \(1981\)](#), notable members of the Dretskean brigade include [Audi \(2003\)](#) and ([Adams 2005](#); [Adams et al. 2011](#)).

like that—he also noted that a familiar argumentative strategy for skepticism seems fatally wounded by the refutation of epistemic closure claims. Let us have that familiar skeptical template in mind (the version of it to which I will refer in what follows):

- (i) If S is justified in believing (or knows) that P (some contingent proposition about empirical matters), then S is justified in believing (or, given suitable assumptions, knows) that not-SH (where the skeptical hypothesis SH is “not-P, but S is perfectly deceived into believing that P”).
- (ii) S is not justified in believing (does not know) that not-SH.
- (iii) Therefore, S is not justified in believing (does not know) that P.

If Dretske is right about the way skepticism relies on closure—as most have agreed that he is—we need only note that premise (i) in the skeptical template is an instance of closure, once we make the unproblematic assumption that P implies not-SH.⁶

So, let us have those closure principles before us from the get-go. Dretske has suggested that neither justification nor knowledge is closed under known implication (even though his case against justification-closure is for the most part only implied in his writings). In what follows, I will discuss weaker versions of the principles that occupied Dretske. The noteworthy difference is that I will not be concerned with *known* implication. So, if successful, my anti-closure case should be stronger than his.⁷

Although knowledge obviously cannot be closed under logical implication, because belief is not thus closed, let us stick with the familiar terminology and focus on the following “closure” (or “transmission of epistemic status”) principles (JC being the genuine closure principle here):⁸

(KC) *Knowledge-closure*: If S knows that $P_1, \dots, P_{n(n \geq 1)}$, and P_1, \dots, P_n imply Q (and S’s belief that Q is formed by inference from the known P_1, \dots, P_n), then S knows that Q.⁹

⁶ Philosophers who, in various ways, downplay the importance of closure to the skeptical argument include Brueckner (1994), Cohen (1998), Vogel (2004), and David and Warfield (2008). For criticisms of David & Warfield’s arguments, see Brueckner (2010).

⁷ I’m in the habit (admittedly, and old-fashioned one) of using the term “logical implication” to designate the *semantic* notion of consequence, keeping “entailment” for the syntactic notion. Many use the term “entailment” as synonymous with “logical implication”. Nothing hangs on the terminological discrepancy.

⁸ For the ways the concept of closure has been abused in the epistemological literature, see Jonathan Kvanvig (2008). It should also be noted that, contrary to popular belief, Warfield (2004) holds that the non-closure of knowledge cannot trivially be derived from the non-closure of belief. Brueckner (2004) replies to Warfield. The dispute has no effect on our discussion here.

⁹ Hawthorne (2004, p. 33, 2005, p. 29) would qualify KC in one respect. Naturally, inference takes time, and Hawthorne thinks your conclusion cannot be a case of knowledge if, by the time you form that belief, you have ceased to know any of the premises of your inference. So, he would revise KC accordingly. But that’s a mistake. Can we keep knowledge that is gained on the basis of knowledge destroyed by misleading evidence (Hawthorne’s worry)? It’s definitely not obvious that we cannot. But, notice: The closure issue is not the issue of whether transmitted knowledge can be *kept*. It is simply the issue of whether knowledge is infallibly *transmitted* by valid reasoning. All that matters is whether knowledge is inputted into the process of valid reasoning, and whether this transmission process is intrinsically fallible.

(JC) *Justification-closure*: If S is justified in (has a justification for) believing that $P_1, \dots, P_{n(n \geq 1)}$, and P_1, \dots, P_n imply Q, then S is justified in (has a justification for) believing that Q.^{10,11}

Act 2: In a spectacular counterattack, Gail Stine initiated what became the contextualist line of defense of closure by claiming that Dretske's attack on closure was fallacious. When you consider SH, she argued, there is a crucial shift in epistemic context, from the everyday, low-standards context where one's belief that P is regarded as knowledge to the high-standards context where a skeptical appraisal of the situation seems unavoidable. So, there's epistemological *equivocation* in Dretske's anti-closure move, according to her. His alleged counterexamples to closure assume that logical implication should transmit low-standards epistemic status to the belief that not-SH, which it cannot do, since that SH-context is a high-standards context. And it should not come as a surprise that low-standards epistemic status is not transmissible in a high-standards epistemic context. Stewart Cohen developed Stine's main point and a contextualist pro-closure argument was set in motion.¹²

Act 3: Invariantists join the closure ranks. A particularly impressive, deceptively simple invariantist pro-closure move was made by Peter Klein in 1981 (and again, more fully, in 1995). He suggests that Dretske's counterexamples are effective only against a principle of evidential transmission through implication, which, adapted to our purposes, looks as follows:

(EC) *Evidential closure*: If body of evidence E justifies $P_1, \dots, P_{n(n \geq 1)}$ for S, and P_1, \dots, P_n imply Q, then E justifies Q for S.¹³

According to Klein, in failing to distinguish between JC and the stronger EC, the Dretskean refutation of JC is fallacious. He claims that JC is compatible with thinking that, once P_1, \dots, P_n are justified for S, those justified beliefs themselves can be the *sole* source of justification of Q for S. And, surely, one's *antecedently justified* beliefs do seem to transmit epistemic justification to their logical consequences. Call this defense of JC "Klein's Point".

Most have tended to agree with Richard Feldman's (1995, 1999) expression of outrage at the attack on closure: "[T]he idea", he writes, "that no version of the closure

¹⁰ The now-popular multi-premise/single-premise distinction for closure principles seems untenable to me, which, in this regard, puts me in the company of Stine. Like her, I'm surprised by some authors' readiness to "solve" the paradoxes involving highly intuitive Conjunction principles for knowledge and justification with the *stunning* rejection of such principles (the motivation for which *cannot* be made as appealing as the principles themselves). This is not the place for a discussion of those paradoxes, however. Authors who have thought the multi/single-premise distinction philosophically sound include Klein (1998), Foley (1979), Stanley (2005), Hawthorne (2004, 2005), Fumerton (2006), and Olin (2005).

¹¹ We're solely interested in what Crispin Wright called "transmission": the cases where one may reasonably expect to *acquire* justified belief or knowledge by valid inference. All the principles in the "closure family" discussed here should be interpreted accordingly. It is not obvious why we should care about epistemic closure otherwise. For more discussion of this point, see note 37 below. For a note of caution concerning Wright's optimistic view of closure, see my 2007, note 12.

¹² See Cohen (1988).

¹³ The label "evidential closure" is my own.

principle is true [...] is among the least plausible ideas to gain currency in epistemology in recent years” (Feldman 1999, p. 95). One can agree with Feldman either because one is a contextualist or because one cannot see that Klein’s Point is false. (Either way, one will be motivated by the need to do justice to the skeptic.)

Now, both the contextualist “equivocation” claim and Klein’s Point have met with resistance in the literature.¹⁴ But, whatever their ultimate fate may be, one thing seems indisputable: Their *prima facie* plausibility goes a long, long way toward accounting for the popular abhorrence of Dretske’s anti-closure stance. And the development of a *better* account of the closure situation must satisfactorily address each of these pro-closure moves. It’s what I do in what follows.

2 Closure refuted; contextualist closure refuted too

So, here’s my first major move in the debate. I want to suggest that we can do better than Dretske in finding counterexamples to closure that are immune to contextualist protestations.¹⁵ In so doing, I address the otherwise appealing contextualist “equivocation” claim by conclusively showing it false—what, by most accounts, Dretske’s counterexamples have been unable to do.

Here’s a first counterexample. This one is aimed at JC: the epistemic situation posed by Russell’s Paradox. (In fact, this particular paradox is only the toughest I’ve known from among a number of paradoxes involving deductive reasoning. You may want to replace it with your own favorite from that lot.)

I’ve made the point elsewhere.¹⁶ So, I’ll keep it short here. There is something deeply unsatisfactory in the view that we are not justified in believing the premises leading to Russell’s self-contradictory conclusion. If you hold on to JC and refuse to question the validity of the paradoxical argument, you are faced with a dilemma. The popular horn of the dilemma is the one according to which, appearances notwithstanding, you are not justified in believing the Russellian premises. But, as we know, geniuses fell for those appearances. They did, of course, because the intuitive pull of those premises is as strong as the logic of the argument. The logic of the argument assures us that there is falsehood in the premises. And yet, we remain as inclined to believe each of the premises as we are to disbelieve the self-contradictory conclusion. Those who wish to make the principled claim that we are not so inclined with any plausibility have had a century to prove us wrong and failed spectacularly.

To see that there is indeed a dilemma here, you have to look no further than the work of a Graham Priest.¹⁷ The less popular horn of the dilemma will have you hold,

¹⁴ For criticism of Klein’s Point, see Brueckner (2000). For a defense of Klein’s Point, see Coffman (2006) and Brueckner (2008) reply to Coffman.

¹⁵ Audi’s counterexamples are immune to contextualist claims as well. But they have met with resistance in the literature. See Feldman (1995) and Audi (1995) reply. See also Hawthorne (2004, pp. 71–73) for an intriguing hint of an objection against Audi’s cases.

¹⁶ See my 2007. The discussion here builds on what I say there, but goes beyond it.

¹⁷ See Priest (1986, 2006).

with Priest, that we can, after all, rationally believe that the Russellian class both is and is not a member of itself.¹⁸ Need I say why that is not an attractive option?¹⁹

Both horns of this dilemma exemplify a form of justification-closure dogmatism. If JC is false, we get an opportunity to reconsider the problem from an epistemological point of view. And the problem is that we are under pressure to explain why it seems that what is obviously false from a classical point of view—namely, the conjunction of the Russellian premises—may be the object of rational belief. If it were easily conceivable to us that some among those premises could rationally be disbelieved, there would be no paradox. But there is a paradox.

So, here are your options with regard to the Russell Paradox situation: (a) reject the logic of the argument; (b) believe the self-contradictory conclusion; (c) hold the conjunction of the premises rationally unbelievable, thus accepting that, by your standards for rationality, geniuses once irrationally believed them; (d) reject JC, and look for an explanation of why we remain tempted to believe what we now (classically) see must be false. Option (d) will be pursued in Sect. 3 below.²⁰

In any case, it should already be clear that JC is very much under suspicion here. This, by itself, should count as a counterexample to JC, unless we can satisfactorily explain why the perceived falsehood of the paradoxical conclusion makes believing the premises a case of epistemic irrationality. But notice: We don't have a general explanation of why the falsehood of a given proposition makes it irrational for us to believe whatever implies that proposition. We do think, after all, that a falsehood may rationally be believed. And it is not at all obvious that we are inclined to say, retrospectively, that a Frege was irrational when he believed the problematic axiom leading to Russell's Paradox. So, although we may think it rational for him to relinquish the problematic axiom when alerted to the paradox, we are reluctant to deem it *previously irrational* for him to believe it (while ignoring the paradoxical consequence). And yet, by JC, we shouldn't be thus reluctant. If we can't rationally believe the paradoxical conclusion, and the paradoxical argument is obviously valid, why can't we just say that Frege irrationally believed the paradoxical premises?²¹

My answer is forthcoming. But, for now, notice that, *contra* the contextualist, the reasoning constituting the Russell Paradox is not an instance of any form of epistemic "equivocation".

Here's a second counterexample. This one should work against both JC and KC.

Raymond Smullyan (1997, pp. 164–165) reports having written the following sentence on the blackboard of a conference room where he was about to speak: "You have no reason to believe this sentence." Interestingly, he made sure nobody saw him do it, so as to make the sentence look totally gratuitous and apocryphal. He then observed

¹⁸ In fact, given his assumptions, Priest can go as far as to say that belief in the Russellian contradiction is a case of knowledge, since, for him, that contradiction is true. But such an extreme view is not essential to our acknowledging a Priestian horn of our dilemma. What matters here is that the Priestian alternative for rational belief cannot be dismissed out of hand.

¹⁹ If you need an explanation of why it's not, you'll find one in my 2001, Sect. 5.

²⁰ I thank Tony Brueckner and Rodrigo Borges for pressing me on this point in correspondence.

²¹ Commenting on this passage in correspondence, John Williams recalls the well-motivated attempts to square the circle prior to the von Lindemann proof. Likewise, there seemed to be no irrationality there.

how people reacted to it. He asked a nine-year-old boy in the audience whether the boy “believed the sentence”. The boy replied that he did, because he noticed that, indeed, he had no reason to “believe the sentence”. The boy’s answer was enthusiastically endorsed by Smullyan himself.

Never mind Smullyan’s sloppy talk of “believing a sentence”. But notice that he is just not thinking like an epistemologist. Smullyan does not seem at all troubled by the fact that, in the scenario he created, the believer is faced with what obviously looks like a truth to him, but, equally obviously, cannot rationally be believed. In fact, the Smullyan sentence gives us what we might describe as the flip side to the Russell Paradox situation. There, we find it very hard to disbelieve what we see must be false. Here, we find it very easy to disbelieve what we see is true, since, obviously, it’s true only if rationally unbelievable.

Now, consider how we may develop Smullyan’s cue in a case against both JC and KC. Suppose I have just to Dr. Authoritative in the hallway leading to the conference room where I’m headed. He tells me he has just left a philosophical message for me on the blackboard of that empty conference room. That message, he assures me, is true of me. And I have every reason to believe everything Dr. Authoritative tells me. So, even before I walk into the empty conference room, I already have the following belief:

- (i) What the text on the blackboard says is true of me.

And I suppose this is something I justifiably believe and know on the basis of reliable testimony. When I walk into the empty room and read what’s written, I add the following to my stock of beliefs:

- (ii) What the text on the blackboard says is “You have no undefeated reason to believe this (very same) proposition”,

which, of course, I justifiably believe and know, given that I make no mistake in reading what’s written and understand every word of it. But, now, I’m under epistemic pressure to add the following to my stock of beliefs:

- (iii) Therefore, I have no undefeated reason to believe this (very same) proposition.

I don’t do so, however, because it seems clear to me that I couldn’t possibly be rational in believing that conclusion. No plausible epistemology would sanction such a belief. Whatever a fully developed epistemological analysis might say about that proposition as an object of belief, the simple point here is that such a belief is its own counterevidence—its own *overrider*, in my preferred terminology.²² The belief is epistemically self-defeating, if true. But, since, by hypothesis, the premises are true, and the argument is obviously valid, the conclusion is true. So, I can see that it is true. But, surprisingly, it can be neither known nor rationally believed.²³

Again, as in the Russell Paradox situation, truth and justification come dramatically apart. And, again, *contra* the contextualist, notice that no discernible shift in epistemic context occurs here.

²² This is the terminology in Klein (1981).

²³ I thank Rodrigo Borges for sharp criticism of an earlier version of this counterexample.

Now, a case against KC only. Recall the Goldman/Ginet Gettier-type barn-country case. And consider this variation on the fake barn theme:

Miss Leeds' barn: I'm going to see my friend Miss Leeds at her farm tomorrow. I call her today and ask for directions. Miss Leeds is very trustworthy. She tells me that the best way to locate her farm is to watch out for the first barn once I get on Rt. 13. That's her barn. So, on the basis of what she tells me on the phone, I reason as follows:

- (i) Everything Miss Leeds tells me is true.²⁴
- (ii) Miss Leeds tells me that the first barn I'll see once I'm on Rt. 13 is her barn.
- (iii) Therefore, the first barn I'll see on Rt. 13 is Miss Leeds' barn.

Unbeknown to me, however, a movie crew has just put up a perfect barn façade right next to her barn. The fake barn is actually the first barn-looking structure one can see on Rt. 13. So, tomorrow, when I get on Rt. 13, I'll see the two barn-looking structures standing side-by-side and will be unable to distinguish Miss Leeds' barn from the fake barn.²⁵

The situation here clearly seems to be one in which the premises can be cases of knowledge for me. But I'm Gettierized with respect to the conclusion. I have a justified true belief that (iii). The first barn I'll see on Rt. 13 while looking at the two barn-looking structures is, indeed, Miss Leeds' barn. But I won't be able to distinguish her barn from the fake barn standing right next to it. So, I *will* be seeing Miss Leeds' barn, I have excellent non-overridden reason to believe I'll be seeing her barn, and it *is* the first barn I'll be seeing, but I can't now know that I'll be seeing it. In typical fashion for a Gettier case, there are nearby worlds where the movie crew (or somebody else) builds a genuine barn right next to Miss Leeds' barn (and all else remains the same). In those nearby worlds, my belief that (iii) is false (or neither true nor false, if you allow for truth-value gaps)—either because I simultaneously see two barns when I get on Rt. 13 (so, no barn is *the first barn I see*) or because I only see the new barn (the first barn-looking structure on Rt. 13) and mistakenly take it to be Miss Leeds' barn. (You decide which.) Knowledge-closure fails here.²⁶

As compelling as the above cases seem to me, they are merely persuasive. Let me now try to *prove* to you that, given assumptions that you won't want to relinquish, both JC and KC fail.

So, let me display these widely held principles:

- (BD) *Belief-distribution:* If S believes that P&Q, then S believes that P and S believes that Q.²⁷

²⁴ I assume it obvious that this can be known (if one can have any empirical knowledge). We ordinarily hold beliefs about the reliability of our sources.

²⁵ Assume, if you will, that Miss Leeds is unaware of one's disadvantage while looking at her barn from Rt. 13. This will prove irrelevant to our analysis of the case.

²⁶ It might be thought that my belief that (i) is as Gettierized as my belief that (iii). That would be a mistake. Notice that, in defeasibility-theory parlance, as developed by Klein in his 1981 book, there is no "genuine defeater" of my justification for (i), only "misleading" ones. As an exercise, you may want to look for an "initiating defeater" that does not defeat by lending some support to the false "effective defeater" expressed by "Miss Leeds has given me false information". You will be wasting your time.

²⁷ For every conditional displayed as an epistemic principle, I omit, as usual, both the quantifiers and the necessity operator.

- (JD) *Justification-distribution*: If S has a justification for believing that P&Q, then S has a justification for believing that P and S has a justification for believing that Q;²⁸
- (AI) *Anti-incoherence*: If you believe that P and you believe that not-P (at the same time), you cannot know either, nor can you be justified in believing either.²⁹
- (KD) *Knowledge-distribution*: If S knows that P&Q, then S knows that P and S knows that Q;
- (FK) *Factivity of knowledge*: If S knows that P, then P;
- (K \Rightarrow B) *Knowledge-implies-belief*: If S knows that P, then S believes that P.

Now, assume that the premises of the following argument, (i) and (ii), are both true and justified for me:

- (i) Everything my doctor says about me is true.
- (ii) My doctor says that both, [P] I'm terminally ill, but I believe I am not [I believe that not-P].
- (iii) Therefore, I'm terminally ill, but I believe I am not.

Can (iii) be a case of justified belief for me just because it's implied by (i) and (ii) and inferred from them? No! *Proof*: Assume, for reductio, that (iii) is true and justified for me. By BD, I believe that P, and, given that (iii) is presumed true, it must be the case that I believe that not-P. By JD, I'm justified in believing that P. But, by AI, I'm not, since I also believe that not-P. So, I'm both justified and not justified in believing that P. So, either (iii) is false or it's not justified for me. But, by hypothesis, (i) and (ii) are true and the argument is obviously valid. So, (iii) is true. So, it is not justified for me. Therefore, JC is false.

Now, assume that (i) and (ii) are cases of knowledge for me. Can I know that (iii) just because it's implied by (i) and (ii) and inferred from them? No! *Proof*: Assume, for reductio, that my belief that (iii) is a case of knowledge. By KD, if I know that (iii), I know that P. So, I know that P. And, by K \Rightarrow B, if I know that P, then I believe that P. So, I believe that P. But, by FK, if I know that (iii), then the right-hand side of the conjunction is true—in which case, I believe that not-P. So, I believe that not-P. But, by AI, if I believe both P and not-P, neither belief is a case of knowledge for me. So, I don't know that P. So, I both do and do not know that P. So, I don't know that (iii). Therefore, KC is false.³⁰

And again, notice how this counterexample from the commissive form of Moore's Paradox is immune to contextualist maneuvering.³¹ Dretske was essentially right.

²⁸ If I'm not mistaken, Klein (1981, p. 79) is the original source for JD, though he gives it a different label.

²⁹ I'm not casual in my use of the term "anti-incoherence", instead of the more obvious "anti-inconsistency". For discussion of how, unlike incoherence, inconsistency can be thought compatible with epistemic rationality, see my 2007b. AI is only the most obvious corollary of an anti-incoherence principle for justification and knowledge.

³⁰ I thank Baron Reed for an excellent objection to a previous version of this argument.

³¹ Notice, also, that I have no use for the assumption that you can instantly see that (iii) is Moore-paradoxical. The argument shows you that, given our assumptions, you cannot have that conclusion as an object of either knowledge or rational belief.

3 Klein was right too, but not very either

But now we must part company with Dretske. Having taught us one of the most surprising lessons in the epistemology of reasoning, a lesson about how epistemic closure fails, Dretske will lead us into error with regard to both Klein's Point and Cartesian skepticism.

Consider his outrageous (Dretske 2005, p. 23) claim according to which Klein's Point is "verbal hocus pocus". If Dretske is right about it, our most fundamental assumption about valid reasoning is turned into an impenetrable mystery. On the Dretskean picture, there is just no accounting for the apparent fact that implying reasons are epistemically effective in a way that is unmatched by inductively strong reasons. The core of Klein's Point is simply the fundamental assumption that the degree of justification transmitted to a given belief by implying reasons is no lower than that of the premises in valid reasoning. If you start out with knowledge-grade (propositional) justification for your premises, and those premises imply your conclusion, then you have knowledge-grade (propositional) justification for your conclusion.³² Klein's Point, I submit, is either that or some invariantist mistake in view of our counterexamples. And yet, counterexamples notwithstanding, the appeal of Klein's Point seems undeniable. In fact, the Point is not partisan. A context-sensitive version of Klein's Point is, of course, assumed by the contextualist.

But, here, our efforts to make sense of the Klein/Dretske debate in its own, conceptually deficient terms will put us through a seemingly endless cycle of enigmas, misunderstanding and frustration. They won't let you have it all: On the Dretskean picture of the situation, you can have closure failure. But there's no finding any room for what motivates closure defenders, contextualists and Kleinian invariantists alike: either Klein's Point or fairness for the skeptic. Dretske would have you believe that skepticism cannot get off the ground, because no epistemic status ever gets transmitted to an "irrelevant" skeptical hypothesis. And yet, that surely seems a little too easy. Both Kleinian invariantists and contextualists will justly cry foul. They will get the crying right. But they will stick with closure. And that is simply not an option for us in view of our counterexamples.

But you *can* escape this decades-old merry-go-round. You will do so only if you notice that the debate has been carried by an impoverished conceptual apparatus. I have argued that the kernel of truth in Klein's Point is safely captured by the following principle:

(SC) *Semiclosure*: If S is *ultima facie* justified in believing that $P_1, \dots, P_{n(n \geq 1)}$, and P_1, \dots, P_n imply Q , then S is *prima facie* justified in believing that Q .³³

³² If I'm not mistaken, Klein would restrict his acceptance of certain epistemic closure principles to single-premise inferences only, in response to the Lottery and the Preface paradoxes—or would otherwise specifically reject a Conjunction principle for justification (the principle that, if P and Q are both justified for you, so is their conjunction). See Klein (1998). Again, to my mind, this popular move replaces a puzzle with a mystery.

³³ See my 2007. The label "semiclosure" is not used there. It should also be noted that my 2007 discussion of Klein's Point was developed in ignorance of E. J. Coffman's 2006 discussion of some of the same Kleinian views. I now see that Coffman, too, had availed himself of the *prima/ultima facie* distinction in his defense

SC requires our acknowledging a prima/ultima facie distinction for justification. But this must be done anyway. We don't really have a choice in the matter. As Thomas Senor (1996) and Michael Bergmann (1997) have argued, no tenable epistemology can afford to do without such a distinction. So, here it is in a nutshell: Prima facie justification is to be understood as *knowledge-grade* justification, regardless of whether your concept of justification is internalist or externalist. In fact, you can call "prima facie justification" whatever defeasibly epistemizes true belief according to your favorite epistemology. It turns into ultima facie justification for a given proposition and agent only if there are no ultimately non-overridden overrides of a justification of that proposition (or no ultimately "non-neutralized counterevidence" for it) in the agent's mental life.³⁴

Let me emphasize this point: Nothing here turns on whether you hold a substantially justificationist epistemology. We are merely interested in a *structural* property of knowledge that no one can afford not to acknowledge. All that we require is that your epistemology make room for *some* defeasibly epistemizing property of beliefs (in addition to truth). Let us, for the sake of terminological parsimony, call that property "justification" and identify its structural role with the prima/ultima facie qualification.

So, we begin to dismantle the charade posed by the Dretske/Klein debate by focusing on SC. SC seems immune to every counterexample to JC in the literature. Consider our counterexample to JC from Moore's Paradox. In that case, it seems perfectly harmless to suppose that I do have prima facie justification for believing (and knowing) that (iii), the Moore-paradoxical conclusion. But it should be clear that I have non-overridden counterevidence for my belief that (iii), namely, the fact that (iii) is paradoxical, if I see that it is, or the very argument I've just seen (in Sect. 2, above) against a justified belief in (iii). Take your pick! SC is not refuted by the counterexample. What is refuted is the assumption that the transmitted justification remains non-overridden for the conclusion. And that should not be confused with Klein's Point—though Klein himself may have failed to clearly distinguish his Point from this *additional*, and false, assumption.

Or consider Dretske's original Zebra case, for another instance. In that case, there is likewise no harm in thinking that SC stands. You do get as much justification for believing that what you see are not perfectly disguised mules as you did for believing that you are looking at zebras—though, as Klein would rightly note, the justifiers for the two may not be the same. But, now, you must acknowledge that you do not have ultima facie justification for believing that you're not looking at perfectly disguised mules, for, clearly, there is a non-overridden override provided by the reasons for the second premise in the skeptical template—i.e., the reasons for thinking that the agent cannot be justified in believing that a skeptical hypothesis does not obtain. Notice that you simply do not need to target SC and, implausibly, maintain that the justified premise in the Zebra case (the belief that you're looking at zebras) does nothing to compel you to believe the conclusion implied by that premise (the deduced belief

Footnote 33 continued

of what I'm calling "Klein's Point". Here, I cannot compare my analysis to his. Suffice it to say that the differences are significant.

³⁴ There is, of course, room for disagreement as regards the nature of overrides.

that you're not looking at perfectly disguised mules). *That* is stultifying. What you must admit—with the skeptic—is that your compulsion is effectively checked by the outstanding overrider.³⁵ You do feel the compulsion predicted by Klein's Point and acknowledged by all, except, perhaps, the die-hard Dretskean, for whom the skeptical hypothesis can properly be dismissed as irrelevant. But it seems wholly inappropriate for us to ignore the power of the skeptical argument—as a contextualist would rightly predict. Thus, you keep feeling, *contra* Dretske, the pull of the skeptical *modus tollens*.

And here's our very simple epistemological response to the Russell Paradox situation based on SC. Like the vast majority of us, lesser minds, would have, were we in his place, Frege had powerful *ultima facie* justification for believing that fateful fifth axiom leading to Russell's Paradox. When he learns about the paradox, his *prima facie* justification remains intact, but he may now have a non-overridden overrider of his *ultima facie* justification for believing the paradoxical premises. Now, unless we are prepared to require logical omniscience of the rational believer and censure him for the far-reaching consequences of what he is, at a given moment, justified in believing, we should accept that every commonsense notion of epistemic *entitlement* is more obviously applied to what is, at a given time, within one's mental landscape, or, in any case, *very easily* accessible by reflection and introspection. One may hold beliefs with untoward implications. But, unless those implications are, in a seemingly intractable way, *obvious* to the believer, by some relatively low standard for obviousness, we will be reluctant to apply our terms of epistemic censure to him. Somehow, we fuzzily think that the inferentially mediocre may still be rational. So, we are not inconsistent in our assessment of Frege's merits as a doxastic agent. His merits were as high as any, both when believing the fifth axiom and when withholding belief in it. We were just misled by the coarseness of the conceptual framework from which we derive JC. It is, at a minimum, deeply misleading to say that one is *not justified (simpliciter)* in believing the paradoxical conclusion, and to infer, from that, by JC and *modus tollens*, that one is *not justified (simpliciter)* in believing the Russellian premises. By SC, on the other hand, although Frege was *ultima facie* justified in believing the Russellian premises before learning about the paradox, logical implication transmits no more than *prima facie* justification to the Russellian contradiction.³⁶ So, we can consistently say that Frege rationally believed those premises while ignoring the paradox—that is, believed them with *ultima facie* justification. And the dispute with the dialetheist then becomes the one of whether he loses his *ultima facie* justification for believing the premises upon learning about the paradox. But, from our epistemological point of view, that issue is not especially worrisome. The major point here is that, for the closure defender who is an anti-Priest classicist, there seems to be no room for saying that Frege *ever was* justified in believing the Russellian premises. The anti-Priest, SC-equipped closure denier faces no such problem.

³⁵ I am not, here, analyzing how, exactly, the skeptic may achieve his goal in peddling the second premise in the skeptical template. This is a topic for further discussion.

³⁶ Obviously, when I say, for the sake of simplicity, that "Frege believed the Russellian premises", I mean simply that he made the metamathematical assumptions leading to the paradox—while fatefully ignoring the Russellian class.

4 Counterclosure and the leaner, meaner skeptic

But, now, armed with SC, we are led, in a completely novel way, to reconsider the Dretskean anti-skeptical rationale; for SC, by itself, cannot properly be Dretske's target, JC should be removed from the picture altogether at this point, and, yet, contrary to a Dretskean expectation, the skeptical argument still seems powerful. We do justice to the skeptic, I submit, if we accept that the skeptic can make do by conjoining the impervious SC with the following, insufficiently discussed principle:

(CC) *Counterclosure*: If O is an overrider of S's justification for believing that Q, and $P_1, \dots, P_{n(n \geq 1)}$ imply Q, then O is an overrider of any justification S may have for believing some of the P_i .³⁷

CC is a principle of the transmission of epistemic defeat through implication. Notice how plausible CC seems to be. If I have counterevidence against my belief that I'm at a conference, because, say, I have reliable testimony to the effect that I'm at a movie set that perfectly resembles a philosophy conference, with intelligent-looking people and all, then, surely, that *very same* piece of counterevidence renders unjustified my premise that I'm at the PUCRS conference.

The intuitive basis for CC is hardly distinguishable from the intuitive basis for JC. It does seem that the skeptic can reap all the benefits brought on by JC from the conjunction of SC and CC. So, notice, crucially, that the logic of the skeptical argument remains unaltered. The conditional in the skeptical *modus tollens* may still be the claim that not-SH is *ultima facie* justified for you, if P is. But we now see that there are two ways for the consequent to be false. It may be false because your (potential) belief that not-SH gets no *prima facie* justification from the justified belief that P—which, in accordance with Klein's Point, we are to assume is not the case—or because that *prima facie* justification for your (potential) belief that not-SH does not turn into *ultima facie* justification, given that, once confronted

³⁷ In correspondence, Klein calls my attention to the fact that, in his 1995 paper, note 5, he discusses his previous use of a version of CC (though not under this name) in his 1981 case for closure and finds reason to reject the principle. His objection depends on one's having a non-overridden justification for a conjunction one of the conjuncts of which has an overridden justification if considered apart from the other conjunct. So, as he notes, one may have an overrider of one's justification of P that is not an overrider of one's justification of P&Q. That should not be seen as a problem for CC, however. If we accept JD—as Klein himself does—we should think that Simplification is an epistemically circular rule of inference. You can't *originally* get a justification for P *from the conjunction*. Your having a justification for P is a necessary condition of your having a justification for the conjunction—even if your justification for the conjunction does not come from the conjuncts. So, despite the fact that P, by itself, may not be justified for you, you cannot have a justified belief that P&Q, infer that P from that conjunctive belief and end up with an unjustified belief that P. Again, as reported in note 11 above, all principles in the “closure family” are here understood as applying exclusively to cases where one expects to be able to acquire new knowledge or new justified belief by valid reasoning. Klein's worry about CC is thus filtered out. We have an explanation of why epistemic status is not transmitted by logical implication in that case that is independent of whether closure (or CC) fails in the interesting cases. A number of cases of apparent closure failure are uninteresting from our point of view. For instance, consider the case where one has non-overridden testimonial justification for believing a necessary falsehood and forms an irrelevant belief by inference from that necessary falsehood. Cases of “junk knowledge” are yet another instance of uninteresting “closure failure”. For a discussion of junk knowledge cases, see Hawthorne (2004, pp. 71–73). For the restriction of epistemic closure to compact entailment, see Bob Hale (2000, pp. 174–177).

with the skeptical reasons for the second premise in the skeptical *modus tollens*, your ultima facie justification for believing that not-SH becomes ultimately overridden. From the fact that you cannot be ultima facie justified in believing that not-SH, the skeptical *modus tollens* leads you to the conclusion that you're not ultima facie justified in believing that P. But you should now expect an explanation of why you're not ultima facie justified in believing that P, given that, by hypothesis, you have knowledge-grade evidence for that belief, and JC, a principle of ultima facie justification, has already been refuted. (In other words, *modus tollens* just gives you the falsehood of the antecedent; it gives you no insight into why the antecedent is false; and the explanation based on JC is no longer compelling.) So, here is where CC provides us with the required explanation: It is the tacit assumption of CC that allows us to regard the overrider of the ultima facie justification of a belief that not-SH as an overrider of the ultima facie justification of a belief that P.³⁸ This scheme works just fine for the skeptic. So, the skeptic may, after all, shrug his shoulders to the case against closure! As it turns out, despite the tremendous impetus we have derived from it, the Dretskean discussion of *skepticism* is misleading.

A welcome implication of the foregoing is that our understanding of Cartesian skepticism becomes better-integrated with the prevailing fallibilism in epistemology. There is a very unfortunate, misleading (if not downright erroneous) way of describing the main point of the skeptic, and the problem is widespread in the literature. It is often claimed that the skeptic's case takes the form of an argument to the effect that we don't have *enough evidence* for the beliefs that we ordinarily hold about the empirical world. It's a cliché to which even some of our venerable authors have succumbed. Take Richard Feldman (2003, , ch. 6), for instance. According to him, "[t]he issue raised by skepticism is largely about whether the reasons we have for our ordinary beliefs are good enough to yield knowledge." Consistently with this, Feldman goes on to characterize skepticism as a set of arguments that are anathema to fallibilism. Or take Laurence Bonjour (2002, pp. 259–261), for whom

the skeptical views that are both challenging enough to be interesting and reasonably plausible will [...] challenge whether the beliefs in question are *strongly* justified, that is, justified enough to have a reasonably high likelihood of truth [...] Such skeptical hypotheses [as Descartes' dreaming and evil genius hypotheses] [...] describe allegedly possible ways in which [...] a believer could still have the same evidence or reasons in favor of a certain class of beliefs [...] even though [...] the beliefs in question are in fact false, thereby apparently showing [...] that such evidence or such reasons do not genuinely constitute *good* reasons for thinking that the beliefs in question are true and so do not genuinely justify them.

This is more than a little unfortunate. Skepticism can and should be made compatible with fallibilist assumptions—and it should be *very clearly* compatible with them.

³⁸ The well-known Underdetermination Principle will give us a non-overridden overrider for the belief that not-SH. And CC then allows us to accept that overrider as an overrider of the ultima facie justification of P. The UP works in cahoots with CC. See Brueckner (2010) for a formulation of the Underdetermination Principle and its structural role in the skeptical argument.

The problem is not, of course, peculiar to Feldman’s way of speaking about skepticism, or to BonJour’s. The alleged skepticism/fallibilism clash has been fomented by contextualism. Skepticism is routinely represented by contextualists as the highest of the high-standard positions with regard to the quality of the evidence—the degree to which it can epistemize our beliefs about the empirical world. In fact, the skeptic is represented as holding *prohibitively high* standards for prima facie justification. But it’s high time we recognized this kind of description of the skeptical viewpoint as anachronistic at best. We expect skepticism to be a live option within our *fallibilist* ideology. (There is an invaluable, long-standing warning by Klein (1981) according to which an infallibilist skeptic is irrelevant to our concerns; but, according to him, common ground between the skeptic and the commonsense view *can* be found. This is not, of course, to say that at least some of the motivation for contextualism cannot be recast in a way that is compatible with a fallibilist characterization of the skeptic.)³⁹

On my account of the matter, it becomes readily clear that the skeptic is not under any pressure to deny that our non-implying reasons for our ordinary beliefs about the external world are “good enough”. No! The skeptic can easily grant that my reasons for thinking I’m in Brazil are as good as one can reasonably expect them to be. Given that they are, my seemingly ultima facie justified belief that I’m in Brazil surely gives me as much prima facie justification as I could possibly have for believing that I’m not hooked to the virtual reality machine at a space station orbiting Alpha Centauri, where it just seems to me that I’m in Brazil. An interesting skeptic will happily grant me all that! But, once confronted with the skeptical hypothesis, I cannot beat the overrider provided by my awareness of the fact that, if I now were hooked to that virtual reality machine, I would still have abundant evidence that I’m in Brazil, and that the evidence I have for the Brazil hypothesis does not allow me rationally to pick it over the competing hypothesis. With Klein, I can get all that I would ordinarily hope to get by way of prima facie epistemization of a denial of the virtual reality hypothesis from the unimpeachable SC. But, from CC and the Underdetermination Principle, the skeptic will then derive all the needed leverage to make me see that I’m not, after all, ultima facie justified in believing that I’m in Brazil.⁴⁰ On this account of the matter, the skeptic falls in line with common sense—only to emerge as defiant as he’s ever been. This must count as an advantage of the present analysis of the matter.

5 The leaner, meaner skeptic is malnourished

Now, the fly in the skeptical ointment: There seems to be good reason for thinking that CC is false. Consider this fundamental aspect of the epistemic situation posed by

³⁹ I should also note that, for all I care in this context, David Lewis (1996) may be entirely right in characterizing fallibilism as a form of madness that’s just barely preferable to the alternative.

⁴⁰ Unlike Klein, however, I can’t be charged with allowing for “easy knowledge”. The objection is made against Klein by Brueckner (2000), who identifies Stewart Cohen as his source. Klein (2004) replies to the objection as developed by Cohen (2002). For more discussion of the objection as directed against Klein, see Coffman (2006) and Brueckner’s (2008) reply to Coffman.

the Preface Paradox, and ignore all the other questionable features of that problem. In the Preface situation, there are seemingly *excellent* reasons for thinking that you are epistemically entitled to include/keep all of the following in your belief system: First, you should be able to keep

(i) P_1, P_2, \dots, P_n .

These are, by hypothesis, each of your justified first-order beliefs (or some large number of them). Second, you have excellent inductive reason for the second-order belief that there is at least one falsehood among your justified first-order beliefs, which gives you excellent reason to accept the first-order belief that

(ii) $\text{not-}(P_1 \ \& \ P_2 \ \& \dots \ \& \ P_n)$.⁴¹

But you also have *entailing* reasons for believing the conjunction of all your justified first-order beliefs (or some large number of them):

(iii) $P_1 \ \& \ P_2 \ \& \dots \ \& \ P_n$.

This is (or, in any case, most definitely should be) the uncontroversial core of the Preface situation.

Now, notice how this affects the acceptability of CC. Notice that you have counterevidence (an overrider) for your belief that (iii) that is *not* an overrider for any of your justified beliefs in (i)—namely, (ii). That is to say, your justified beliefs in (i) entail (iii). But you cannot be justified in believing that (iii) while you remain under unrelenting epistemic pressure to believe that (ii). As we have assumed, by AI, you cannot have *ultima facie* justification for believing either (ii) or (iii), since your *ultima facie* justification for either is overridden by the other. And, yet, your overrider for (iii) does not override your justification for believing any of the propositions in (i). So, we seem to have a counterexample to CC.

The skeptic is back in the frying pan! His *modus tollens* seems to rely on a false epistemic principle, CC.

6 Concluding remarks: is the meaner skeptic really leaner?

Lastly, how about the claim, in the abstract of this paper, that the Cartesian skeptic can rely on epistemic principles that are *weaker* than the closure/transmission principles JC and KC? Is that a clear implication of the foregoing?

I assume that SC is clearly weaker than JC. But I admit that it may not yet be clear that CC is weaker than JC. The relationship between CC and KC, on the other hand, is not obviously as interesting. If your epistemology is one according to which knowledge implies justification, *whatever justification may exactly be*, the job of showing that CC is weaker than KC boils down to the job of showing that CC is weaker than JC. If your epistemology is *radically* non-justificationist, we need to discuss the much

⁴¹ The objection that it is unrealistic to expect you to believe this very long conjunction (for a very large n) is beside the point. The point here concerns what is widely known as “propositional justification”—more specifically, what you are *prima facie* justified in believing given what your epistemology counts as evidence for a belief.

larger issue of how your epistemology accounts for epistemic defeasibility. So, I will ignore the latter possibility in these final remarks.

If CC is weaker than JC, it must, of course, be the case that it does not imply JC. Doesn't it? For CC to imply JC, it must be the case that it's impossible for CC to hold while JC doesn't. But I think I have given you a case where, clearly, JC doesn't hold, but CC does: the case from Moore's Paradox. Notice that, if I came to believe that the conclusion of my inference, the Moore-paradoxical proposition (iii), is rationally unbelievable to me, *without also being given a complex philosophical explanation of why that paradoxical proposition can be known by my doctor*, that belief would be an overrider of both my justification for (iii) and my justification for (i). Upon learning that (iii) is rationally unbelievable to him, the non-philosophical hearer would naturally have an overrider for the belief that everything the paradoxical speaker asserts is true. So, we are led to the conclusion that CC does not imply JC.

That JC does imply CC, on the other hand, is easily seen. Assume JC, assume that one has a justification for P, assume that P implies Q, and assume, for reductio, that O is an overrider of one's justification for Q but is not an overrider of one's justification for P.⁴² We readily see, with Klein (1981, pp. 75–76), that O must be either not-Q or not-P. If O were merely inductive counterevidence against one's basing the belief that Q on one's belief that P, JC would ensure that Q remained justified in the presence of O, since the justification provided by implying evidence cannot be weakened; it's an all-or-nothing affair.⁴³ Thus, assume that O is not-P. If so, then, by AI, P is not justified. So, P is both justified and not justified. So, O isn't not-P. Now, assume that O is not-Q. If so, by JC and the contrapositive of the assumed implication, we have a justification for not-P. But, again, by AI, we don't. So, given our assumptions, the conjunction of JC and not-CC is an impossibility.

So, CC is, indeed, weaker than JC and could conceivably be of use to the post-JC, leaner, meaner skeptic.

I think I have shown you how we've been misled in the closure/skepticism debate. These are the interesting implications of my story: Dretske is partly right: closure does fail. There is no such thing as an epistemically indefeasible form of reasoning. Klein is partly right: if you have implying knowledge-grade evidence, you have knowledge-grade evidence for the implied proposition. Contextualists are partly right: when it comes to dealing with the skeptic, Dretske will mislead you. Dretske and all anti-skeptics are partly wrong: the skeptic can laugh in the face of closure failure. The skeptic is wrong: he needs both SC and CC. SC is waterproof, but CC is another false principle.

Acknowledgments I am most deeply indebted to Peter Klein, Doris Olin and John Williams for much discussion of drafts of this paper. Insightful comments were also kindly provided by Fred Adams, Rodrigo Borges, Anthony Brueckner, Stephen Hetherington, Baron Reed, and Emerson Valcarengi. Drafts were presented in Brazil, at UFPR, and at *Epistemology: The Third Brazil Conference*, at PUCRS, and in Mexico, at the *First Latin-American Conference for Analytic Philosophy*, at UNAM. I'm grateful to my audiences on those occasions, especially to Michael Blome-Tillman, Maite Ezcurdia, Paulo Faria, Miguel

⁴² For simplicity, I will here assume that we're dealing with a single premise. Nothing hangs on this.

⁴³ I should note that I'm here lifting one of Klein's ideas from its original context. He used a version of CC in arguing for JC.

Ángel Fernández, Breno Hax, André Leclerc, Luiz Carlos Pereira, Agustín Rayo, and Nathan Salmon. Thanks are also due to the Latin-American Association for Analytic Philosophy (ALFAn), for funding my talk in Mexico, and to the Brazilian government agencies CAPES and CNPq, for sponsoring the PUCRS conference organized by Stephen Hetherington and me.

References

- Adams, F. (2005). Tracking theories of knowledge. In C. De Almeida (Ed.), *Perspectives in contemporary epistemology*, a special issue of *Veritas* 50.4 (pp. 11–35). Porto Alegre, Brazil: EDIPUCRS. <http://revistaseletronicas.pucrs.br/veritas/ojs/index.php/veritas/issue/view/178>.
- Adams, F., Barker, J., & Figurelli, J. (2011). Towards closure on closure. *Synthese* (this issue). doi:10.1007/s11229-011-9922-8.
- Audi, R. (1995). Deductive closure, defeasibility and scepticism: A reply to Feldman. *Philosophical Quarterly*, 45, 494–499.
- Audi, R. (2003). *Epistemology: A contemporary introduction to the theory of knowledge* (2nd ed.). New York: Routledge.
- Bergmann, M. (1997). Internalism, externalism and the no-defeater condition. *Synthese*, 110, 399–417.
- BonJour, L. (2002). *Epistemology: Classic problems and contemporary responses*. Lanham: Rowman & Littlefield Publishers.
- Bruceckner, A. (1994). The structure of the skeptical argument. *Philosophy and Phenomenological Research*, 54, 827–835.
- Bruceckner, A. (2000). Klein on closure and skepticism. *Philosophical Studies*, 98, 139–151.
- Bruceckner, A. (2004). Strategies for refuting closure for knowledge. *Analysis*, 64, 333–335.
- Bruceckner, A. (2008). Reply to Coffman on closure and skepticism. *Synthese*, 162, 167–171.
- Bruceckner, A. (2010). Skepticism and closure. In J. Dancy, E. Sosa, & M. Steup (Eds.), *A companion to epistemology* (2nd ed., pp. 3–12). Oxford: Wiley-Blackwell.
- Coffman, E. J. (2006). Defending Klein on closure and skepticism. *Synthese*, 151, 257–272.
- Cohen, S. (1988). How to be a fallibilist. In J. Tomberlin (Ed.), *Philosophical Perspectives* (Vol. 2, pp. 581–605). Oxford: Blackwell.
- Cohen, S. (1998). Two kinds of skeptical argument. *Philosophy and Phenomenological Research*, 58, 143–159.
- Cohen, S. (2002). Basic knowledge and the problem of easy knowledge. *Philosophy and Phenomenological Research*, 65, 309–329.
- David, M., & Warfield, T. (2008). Knowledge-closure and skepticism. In Q. Smith (Ed.), *Epistemology: New essays*. Oxford: Oxford University Press.
- De Almeida, C. (2001). What Moore's Paradox is about. *Philosophy and Phenomenological Research*, 62, 33–58.
- De Almeida, C. (2007a). Closure, defeasibility and conclusive reasons. *Acta Analytica*, 22, 301–319.
- De Almeida, C. (2007b). Moorean absurdity: An epistemological analysis. In M. Green & J. N. Williams (Eds.), *Moore's Paradox: New essays on belief, rationality, and the first person* (pp. 53–75). Oxford: Oxford University Press.
- Dretske, F. (1970). Epistemic operators. *Journal of Philosophy*, 67, 1007–1023.
- Dretske, F. (2005). The case against closure. In M. Steup, & E. Sosa (Eds.), *Contemporary debates in epistemology* (pp. 13–26). Oxford: Blackwell.
- Feldman, R. (1995). In defense of closure. *Philosophical Quarterly*, 45, 487–494.
- Feldman, R. (1999). Contextualism and skepticism. In Tomberlin, J. (Ed.), *Philosophical perspectives* (Vol. 13, pp. 91–114). Oxford: Blackwell.
- Feldman, R. (2003). *Epistemology*. Upper Saddle River: Prentice Hall.
- Foley, R. (1979). Justified inconsistent beliefs. *American Philosophical Quarterly*, 16, 247–257.
- Fumerton, R. (2006). *Epistemology*. Oxford: Blackwell Publishing.
- Hale, B. (2000). Transmission and closure. In Sosa E., & Villanueva, E. (Eds.), *Philosophical issues* (Vol. 10, pp. 172–190). Oxford: Blackwell.
- Hawthorne, J. (2004). *Knowledge and lotteries*. Oxford: Oxford University Press.
- Hawthorne, J. (2005). The case for closure. In M. Steup, & E. Sosa (Eds.), *Contemporary debates in epistemology* (pp. 26–43). Oxford: Blackwell.
- Klein, P. (1981). *Certainty: A refutation of skepticism*. Minneapolis: University of Minnesota Press.

- Klein, P. (1995). Skepticism and closure: Why the evil genius argument fails. *Philosophical Topics*, 23, 213–236.
- Klein, P. (1998). Knowledge, concept of. In E. Craig (Ed.), *The Routledge encyclopedia of philosophy*. London: Routledge.
- Klein, P. (2004). Closure matters: Academic skepticism and easy knowledge. In Sosa E., & Villanueva, E. (Eds.), *Philosophical issues* (Vol. 14, pp. 165–184). Oxford: Blackwell.
- Kvanvig, J. (2008). Closure and alternative possibilities. In J. Greco (Ed.), *The Oxford handbook of skepticism* (pp. 456–483). Oxford: Oxford University Press.
- Lewis, D. (1996). Elusive knowledge. *Australasian Journal of Philosophy*, 74, 549–567.
- Nozick, R. (1981). *Philosophical explanations*. Cambridge: Harvard University Press.
- Olin, D. (2003). *Paradox*. Montreal: McGill-Queen's University Press.
- Olin, D. (2005). A case against closure. In De Almeida, C. (Ed.), *Perspectives in contemporary epistemology*, a special issue of *Veritas* 50.4, (pp. 235–247). Porto Alegre, Brazil: EDIPUCRS. <http://revistaseletronicas.pucrs.br/veritas/ojs/index.php/veritas/issue/view/178>.
- Priest, G. (1986). Contradiction, belief and rationality. *Proceedings of the Aristotelian Society*, 86, 99–116.
- Priest, G. (2006). *In contradiction: A study of the transconsistent* (2nd ed.). Oxford: Oxford University Press.
- Senor, T. (1996). The prima/ultima facie justification distinction in epistemology. *Philosophy and Phenomenological Research*, 56, 551–566.
- Smullyan, R. (1997). *The riddle of Scheherazade and other amazing puzzles, ancient & modern*. New York: Harcourt.
- Stanley, J. (2005). *Knowledge and practical interests*. Oxford: Oxford University Press.
- Stine, G. C. (1976). Skepticism, relevant alternatives, and deductive closure. *Philosophical Studies*, 29, 249–261.
- Vogel, J. (2004). Skeptical arguments. In Sosa E., & Villanueva, E. (eds.), *Philosophical issues* (Vol. 14, pp. 426–455). Oxford: Blackwell.
- Warfield, T. (2004). When epistemic closure does and does not fail: A lesson from the history of epistemology. *Analysis*, 64, 35–41.