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Hawthorne's might-y failure: a reply to "Knowledge and epistemic necessity"

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Abstract In "Knowledge and epistemic necessity," John Hawthorne gives a defense of what he rightly calls the "standard approach" to epistemic possibility against what he calls a new "competing idea" presented by Dougherty and Rysiew which he notes has been "endorsed and elaborated upon" by Fantl and McGrath. According to the standard approach, roughly, p is epistemically possible for S if S doesn't know that not-p. The new approach has it that p is epistemically possible if p has a non-zero (or perhaps non-negligible) epistemic probability. Both approaches, he notes, would explain the (at least sometime) oddness of CKAs, utterances of the form "p, but possibly not p." However, he offers a number of arguments designed to show that the standard approach has other advantages. In this paper, we undermine Hawthorne's reasons for favoring the standard approach over Dougherty and Rysiew's alternative approach.

Keywords Epistemic modality · Epistemology · Fallibilism · Concessive knowledge attributions

Hawthorne (2012) defends the standard approach to (epistemic) "might" claims, according to which "'Might P' is true in a speaker's mouth only if the speaker does not know that not-P" (493–4). He contrasts this approach with a "competing" approach as presented by Dougherty and Rysiew and endorsed and expanded upon by Fantl and McGrath (493). While Hawthorne's paper is definitely a success in

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raising interesting issues and using them to express the standard approach, we argue that is a failure with respect to the goal of raising significant problems for Dougherty and Rysiew.

In the first section, we set the stage for the debate. In sections two through five, we present arguments that Hawthorne gives in favor of the standard approach and respond to each (in turn) on behalf of Dougherty and Rysiew's approach. We conclude that Hawthorne has failed to provide convincing reason to favor the standard approach over Dougherty and Rysiew's approach.

1 Background

As mentioned above, John Hawthorne defends the standard approach to (epistemic) "might" claims: "'Might P' is true in a speaker's mouth only if the speaker does not know that not-P" (493–4). This approach is contrasted with Dougherty and Rysiew's account of epistemic possibility: "q is epistemically possible for S…just in case q has non-negligible/non-zero[1] probability on S's total evidence" (Dougherty and Rysiew 2011: 396). To make the comparison between these two approaches especially clear, Dougherty and Rysiew's approach can be expressed in terms of a "might" claim. Since "knowing P is…compatible with the epistemic possibility that not-P," utterances of the form "I know P and it might be that not-P" will often be true on Dougherty and Rysiew's account (Hawthorne: 494). Following Rysiew (2001), these sorts of utterances are called "concessive knowledge attributions," or "CKAs" (493). As we will see, CKAs are central to the present debate.

It is agreed that the relevant CKAs sound strange to most people. On one hand, since the standard approach to "might" claims holds that a speaker's knowing P is incompatible with the epistemic possibility that not-P, the advocate of the standard approach will hold that CKAs like "I know P and it might be that not-P" sound strange because they express an explicit contradiction (Hawthorne: 496). Dougherty and Rysiew, on the other hand, attribute this strangeness to a pragmatic conveyance, rather than a semantic one. Specifically, the second conjunct of the relevant CKA— it might be that not-P—pragmatically conveys the proposition *I don't know that P and I don't know that not-P* (Dougherty and Rysiew 2011: 396). This proposition, however, is in conflict with the CKA's first conjunct: I know P. Hence, even though CKAs—on Dougherty and Rysiew's approach—do not always express explicit contradictions, they are often unassertable, because to assert them "would ordinarily mislead an interlocutor to infer that the possibility [that not-P] was an epistemically significant one" (Dougherty 2011: 142).

After outlining these two approaches, Hawthorne claims to give several solid reasons to prefer sticking with the standard approach rather than embrace Dougherty and Rysiew's probabilistic approach. We now turn to Hawthorne's first argument in favor of the standard approach.

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[1] Although we will not consider whether we should accept "non-negligible" over "non-zero" probability, Dougherty and Rysiew (2011) end up preferring the non-zero version along with a pragmatic account of why epistemic possibility claims are often unassertable even if always true.

2 "Might" and "just in case"

Hawthorne begins by contrasting uses of "might" with uses of "in case" or "just in case" in the following four CKAs (496):

1. "I know this isn't going to work. But in case it does…"
2. "I know the operation will be a success. But just in case it isn't…"
3. "I know this isn't going to work. But it might work."
4. "I know the operation will be a success. But it's possible that it won't be."

There are two things we need to keep in mind when considering 1–4: (a) Hawthorne believes that the speaker's use of "know" in each CKA is "half-hearted"[2] and (b) Hawthorne claims that utterances of 1 and 2 "sound a lot better" than utterances of 3 and 4 (496). The standard approach, he tells us, "has a very natural explanation" of why CKAs like 1 and 2 sound better than CKAs like 3 and 4: the conjuncts of 3 and 4 "*explicitly* contradict" one another, while the conjuncts of 1 and 2 do not (496, emphasis in original). Specifically, since each use of "know" is half-hearted, the first conjuncts in 1 and 2 communicate an overly optimistic (or pessimistic) position—more generally, the first conjuncts in 1 and 2 are not to be taken as serious knowledge claims—while the second conjuncts in 1 and 2 provide one's interlocutor with one's "all things considered judgment" (495–6). In other words, when a speaker says, "I know this isn't going to work," he's being overly pessimistic about the situation, and his unjustified pessimism is revealed and tempered by his adding, "but in case it does work…"

Dougherty and Rysiew's approach, Hawthorne thinks, seems to lack "a natural account of the contrast in felicity between" (1 and 2) and (3 and 4) (496). After all, according to Hawthorne, Dougherty and Rysiew's approach seems to treat all four CKAs the same: all four "at best pragmatically communicate something incompatible" (496). So given that there is an apparent contrast in the felicity of (1 and 2) and (3 and 4)—namely that (1 and 2) seem to be felicitous while (3 and 4) seem not to be—and given that the standard approach explains this difference in felicity while Dougherty and Rysiew's approach does not, it seems that we have some reason to favor the standard approach over Dougherty and Rysiew's approach.

Hawthorne's assessment has two problems (apart from the fact that perceiving a difference between the pairs of CKAs often varies by user). First, "just in case" locutions assume "might" claims. So both pairs are logically inconsistent, given the standard account. Second, even if we insist that there is a difference in the felicity of (1 and 2) and (3 and 4), Dougherty and Rysiew's approach has a simple explanation of this difference.

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[2] According to Hawthorne, a speaker is "half-hearted" in her claim to know P if, when an interlocutor presses her on whether or not she *really* knows P, "the speaker will very quickly retract" her claim to know P (495). However, the problem with assuming that CKAs 1–4 all include half-hearted knowledge claims is that a subject's willingness to retract her knowledge claims depends on her temperament, theoretical commitments, etc. For instance, a stubborn person (or a committed fallibilist) will likely utter CKAs like 1–4 and then refuse to withdraw her claim to knowledge, even when pressed. So, contrary to Hawthorne's suggestion, it is not at all clear that CKAs 1–4 include half-hearted knowledge claims.

First, it seems clear that statements of the form "I know ∼P, but in case P…" presuppose "Possibly, P."[3] It would be hard for someone with the relevant concepts and linguistic competencies to miss this. So it seems odd that someone hearing 1–4 through the standard account would distinguish strongly between them. For though Hawthorne is right that (3 and 4) are "explicitly" contradictory and (1 and 2) aren't *explicitly*, the fact remains that on (1 and 2) there is, given the standard account, a contradiction *very close to hand*. But then it should not be, as Hawthorne claims, that (1 and 2) "sound a lot better" than (3 and 4). They are just too semantically close to justify that wide a margin in felicity (for a competent user). This turns Hawthorne's argument on its head, for on the standard account, his disparate reactions seem inexplicable.

Regarding the second problem with Hawthorne's argument: if we are convinced that there is a difference in the felicity of (1 and 2) and (3 and 4), Dougherty and Rysiew's approach *does* have available a natural way to explain this difference. Since probabilities come in degrees, it is reasonable for the advocate of Dougherty and Rysiew's approach to argue that the utterance, "just in case of P" typically pragmatically conveys that P is a fairly *remote* possibility, while "it might be that P" typically conveys that P is a more significant possibility by comparison, apart from context. For example, if a doctor says, "I want to run this test because the treatment might have failed," one is likely to be more unsettled than if the doctor had said, "I want to run this test just in case the treatment failed."

Interestingly, this illustrates a considerable advantage of Dougherty and Rysiew's approach over the standard approach. As we've just said, Dougherty and Rysiew's approach connects (epistemic) possibility claims with probability, so their approach naturally allows for a gradation of possibility claims. This tracks well with ordinary language since ordinary language makes it clear that epistemic possibility claims are graded.[4] Take, for instance, the following token utterances: "it's somewhat possible the train will be late" and "it's very likely that it'll rain today." Without any further context, it would seem that the latter utterance reports an event that is more likely to occur than the event reported in the former utterance. So Dougherty and Rysiew can appeal to pragmatic conveyance and probabilistic framework to explain why certain utterances, like (1 and 2) sound felicitous while (3 and 4) do not. Can the advocate of the standard account make a similar move?

It would seem not. After all, Hawthorne used the standard account to appeal to a semantic difference between (1 and 2) and (3 and 4), but we've shown that 1–4 are semantically in the same boat (either all are contradictory or none are). So how does the advocate of the standard approach explain the difference in felicity if a semantic difference is of no use? It remains to be seen. At the very least, those who hear a

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[3] According to the Oxford English Dictionary, a "case" is "a thing that befalls or happens to any one; an event, occurrence, hap, or chance," while "just in case" means "in the event or contingency that, if it should prove or happen that" (see Simpson 2013a, b, respectively). This means that "just in case P" (semantically) includes that P is a possibility.

[4] This is not to deny that there is a well-defined binary notion in terms of the graded notion: P is epistemically possible if its probability is greater than 0 and is not epistemically possible if its probability is 0.

difference in the felicity of (1 and 2) and (3 and 4) should recognize that this difference causes no problem for Dougherty and Rysiew's approach. Thus, Hawthorne has not yet given us any reason to prefer the standard approach over Dougherty and Rysiew's approach.

3 Some spurious data and one spurious argument

Next, Hawthorne shows that one apparently felicitous CKA offered by Fantl and McGrath "as prima facie evidence against the standard account" should not count as evidence against the standard approach after all (496–7). Hence, if Hawthorne is right, whatever data we glean from Fantl and McGrath's example will be "spurious" (496). Below, however, we'll argue that Hawthorne's argument is irrelevant.

To begin, the CKA in question runs as follows (496):

1. The possibility that not-p is ridiculous and not worth considering. I know that p.

But, as Hawthorne points out, CKAs 2 and 3 are also felicitous (496–7):

2. The possibility that he is not in the pub is ridiculous and not worth considering. He must be in the pub.
3. The possibility that he is not in the pub is ridiculous and not worth considering. He couldn't be anywhere else.

The problem for the alternative approach, Hawthorne thinks, is that nobody is going to argue for compatibility between "must" and "possibly not" in 2 or "couldn't" and "possibly not" in 3 (497). But if CKAs like 1 demonstrate the compatibility of terms like "possibly not-P" and "I know P," then surely CKAs like 2 and 3 demonstrate the compatibility of the terms they each employ (496–7). So since there's "no disputing the fact" that the terms employed by 2 and 3 are inconsistent, we should conclude that the terms used in CKA 1 are inconsistent, and, therefore, CKAs like 1 (despite being felicitous) should not count as evidence against the standard approach (496–7).

First, note that Hawthorne has not offered any evidence (or referred to any) that epistemic "must" is the dual of epistemic possibility. Just because there is some epistemic logic model that treats it this way means very little indeed. Besides, if "must" and "knows" are interchangeable, then obviously Dougherty and Rysiew would deny that "must" is the dual of epistemic possibility, since they deny that "knows" is the dual of epistemic possibility. So Dougherty and Rysiew have options here. They can agree with Hawthorne that "must" and "knows" are interchangeable, but deny that "must" is the dual of epistemic possibility. Or they can deny that "must" and "knows" are interchangeable. As noted above, Hawthorne offers no evidence against the former strategy. And Dougherty and Rysiew might pursue the latter strategy by arguing that "it must be that P" means $Pr(p) = 1$ on one's evidence, whereas "I know that P" is compatible with $Pr(p) \leq 1$. Dougherty and Rysiew (2011: 401) have already noted, there needs to be further discussion of felicitous contradictions and ambiguous statements, but the main point here is just that there are several ways to go here with some having more costs than others. But Dougherty and Rysiew's backs are hardly against the wall.

Furthermore, as Dougherty and Rysiew (2011: 400) have already acknowledged, not all felicitous CKAs support their approach (that is, just because a CKA is felicitous doesn't mean that it's consistent). If Hawthorne wants to undermine Dougherty and Rysiew's approach, he needs to do more than show that *some* felicitous CKAs are inconsistent. It would be well if he could show that all felicitous CKAs have counterparts like 2 and 3 in which terms like "must" and "might not" are used literally. But, first, as Dougherty and Rysiew have already argued, "we are hardly ever literally certain" (2011: 401–2). Thus, they would surely agree, given how casually we use terms like "sure" and "certain," that we hardly ever use certainty locutions literally. When we say "I'm sure you've thought of this, but…" we are *far* from sure! Examples abound. Second, there are cases in which CKAs are both felicitous and consistent (see Dougherty and Rysiew 2009: 130–1). And it would be natural, on Dougherty and Rysiew's account of epistemic possibility, for "must" to be used non-literally frequently, since it expresses certainty, and true certainty is rare.

Alternatively, note that "not worth considering" can be quantified as some probability between zero and some very small (probably vague) threshold k. So the first conjunct—that the possibility that not-p is…not worth considering—says that the probability falls within the interval [0,k]. So the sober assertion of 2 or 3 can simply indicate that a probability of 0 has been assigned to the "possibility"[5] in question, and then 2 and 3 are perfectly consistent. The second conjuncts seem half-hearted, expressing an unjustified optimism (or pessimism depending upon the individual in question). Hence, Hawthorne's felicity argument misrepresents the use of felicitous CKAs and isn't successful at any rate.

4 Cancellation data and data that just don't cancel

Next, Hawthorne claims, "the Dougherty/Rysiew proposal predicts that making it explicit that the chance of [not-P] is insignificant should drastically improve certain speeches in which one admits to a chance of not-P while claiming knowledge of P" (497). He then provides the following example: "'There is a chance that he will not show up, but it's not significant—it is very small indeed. I know he will show up'" (497). This example, Hawthorne thinks, sounds "dicey" at best (497). This leads him to conclude that "matters are not much improved by adding "'but the probability is very small indeed'" when one utters a CKA of this sort (497). Hawthorne cites no empirical data that his judgment is widespread. It is, of course, unsurprising that one firmly committed to the standard view would not like the way this sounds. One hopes for more solid reasons.

Further, in response to Hawthorne's claims, first note that Dougherty and Rysiew have gone to great pains to point out that the mere *mention* of the possibility that not-P can suggest to a listener that the speaker doesn't know whether P (2011, 396–7). A

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[5] Note that the locution "the possibility that P" is very different from locutions like "possibly, P" or "it is possible that P" or "P is a possibility." The latter all tend to express that P is a *remaining* possibility (after evidence gathering). "The possibility that P," by marked contrast, can refer to partitions of an outcome space that has been eliminated. Consider: "The possibility that X has been strictly eliminated".

speaker's mentioning that "possibly not-P" right after she claimed to know P may not always be *fully* cancelable, in effect, and, thus, her interlocutor's doubt may not be fully eliminated by subsequent clarification. In other words, in these cases, what is pragmatically conveyed by a speaker's utterance, "possibly not-P" overwhelms the semantic content of the speaker's subsequent clarifications. Listeners will be stuck with persistent worries, like *but why would S say* "it might be that not-P" *if not-P was so incredibly unlikely*? This doesn't mean that adding "but the probability is very small indeed" won't help at all. All it suggests is that adding "but the probability is very small indeed" may (in some cases) at best *diminish* an interlocutor's doubt that the speaker really knows P. Results will, of course, vary by user and context. Note that the fact that subsequent clarification helps *at all* is something the standard view will have a hard time explaining. Indeed, Lewis (1996) must *implore* fallibilists to try to hear "afresh" expressions of fallibilism as sounding bad. This could not be unless they couldn't hear it. Whose hearing is theory-laden and whose is pure? This may not be a question philosophy is best suited to answer.

Second, those who raise the charge about failure in cancellation to Dougherty and Rysiew's strategy tend to treat implicature and cancellation as a pretty one-sided affair: the speaker "generates" implicatures much like a machine generates widgets. And the speaker can, pretty much unilaterally, cancel an implicature, like shutting off a machine. This is, however, a kind of flat-footed understanding of what Grice said. Neo-Griceans like Kent Bach and advocates of optimality theory (optimality theory is essentially just a fairly well worked out generalization of textbook Gricean theory in a more real-world direction) treat these notions more subtly. Our treatment borrows from these more subtle approaches.[6]

According to this account, roughly, the extra-semantic component of communication is better modeled by default reasoning and heuristics than by simple deductive reasoning, and is largely unconscious. A toy theory goes like this: just as people tend to infer, Hume points out, a nomological connection from constant conjunction of events, people tend to infer (unconsciously) an entailment relation from constant conjunction of utterances. Because the possibilities of error which are consistent with knowledge are so rarely worthy of mention (most examples come from the philosophy room: brains in vats, malevolent demons, and the like) they are rarely mentioned. Almost all the time, "possibly P" is uttered in contexts where it is clear that the possibility is significant in that context. Due to this constant conjunction of the utterance and contexts with significant doubt, the mind tends to attribute a *necessary* connection between "possibly P" and there being a significant chance that P. What's more, because this is unconscious and because the experience feeds into a system governed by nonmonotonic logic, a few counter-examples are not going to significantly diminish the tendency to see a necessary or nearly necessary connection. This is, of course, just a toy theory, but one advantage of such approaches—besides their commonsense appeal—is that they are open to empirical investigation. A full consideration of the application of optimality theory is beyond the scope of this paper, but the toy theory suggests a fruitful research project.

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[6] See van Rooij (2014) for an overview of optimality theory.

So imagine that you are Midwesterner from America visiting Sweden where, according to legend, people tend to stare benignly. Your background experience, however, suggests a strong connection between staring and ill-will. So when the swedes stare at you, you feel very self-conscious. Even when it is explained to you that there are different mores here, you will likely continue to feel self-conscious. Defeating information can't just "cancel" your dispositions. Likewise, when we first meet anomalous cases to the "law" that "possibly P" implies a significant chance that P, the anomalies will be successful in "canceling" the tendency to make the (unconscious) inference from "possibly P" to there being a significant chance that P with varying success.

5 The collapse of complex constructions to a single objection

In this section, we will consider three final objections Hawthorne raises against the Dougherty and Rysiew's approach. We will argue that while it might initially seem that each objection provides an independent reason to reject Dougherty and Rysiew's approach, all three objections collapse into a single objection. Thus— regardless of whether or not advocates of Dougherty and Rysiew's approach can adequately respond to this objection—we must refrain from following Hawthorne in counting this objection multiple times, for they essentially collapse into one.

To begin, in the sixth section of his paper, Hawthorne notes that most people "are perfectly willing to assert 'It couldn't be that P or Q' on the heels of 'It couldn't be that P' and 'It couldn't be that Q'" (498). Further, though the standard approach supports our willingness to grant this inference, Dougherty and Rysiew's approach does not (498). This, Hawthorne thinks, is a strike against Dougherty and Rysiew's approach (498). Call this "Objection A."

Before responding to Objection A, it is important to point out that in the seventh section of his paper, Hawthorne argues that "Dougherty and Rysiew's picture is not multi-premise closure friendly" because "there can be a significant chance that a conjunction is false even though there is no significant chance that one of the conjuncts is false" (499). This too, Hawthorne argues, is a strike against Dougherty and Rysiew's approach (499). Call this "Objection B."

We group objections A and B together because they are really just the same objection run in two different ways. To see why, consider the following proof:

Deduction A

1.	K~p	Premise
2.	K~q	Premise
3.	K~p & K~q	&I: 1, 2
4.	K(~p & ~q)	K2: 3
5.	K~(p v q)	DM: 4

Objection A is that Dougherty and Rysiew's approach undermines the inference to 5 from steps 1 and 2. But Objection B is that Dougherty and Rysiew's undermines the inference to 3 from steps 1 and 2 (i.e. Dougherty and Rysiew's approach undermines multi-premise closure).[7] Yet this doesn't provide us with two separate reasons to reject Dougherty and Rysiew's approach. To see why, note that Hawthorne cannot suspect Dougherty and Rysiew of denying DM. Rather, those who reject multi-premise closure for the reasons that Dougherty and Rysiew do—and so deny that 3 is entailed by 1 and 2 (even if it is a reliable default inference)—would also reject K2 for the very same reasons: they move from a situation where no conjunction falls within the scope of an epistemic sentence operator to a situation where one does, thus inviting failure due to dwindling probability. So in Deduction A, the only reason one couldn't get to 5 is that they couldn't get to 3 (or 4) in the first place, on the deduction model. For this reason, we will focus on responding to the more fundamental of the two objections: Objection B. For clarity's sake, since Objections A and B are intimately related, whatever solution we provide to Objection B will apply just as well to Objection A, *mutatis mutandis*.

First, though, a brief bit of ground clearing is necessary. For it is just false that, if epistemic "must" is the dual of epistemic "can", Dougherty and Rysiew's probabilistic semantics for "can" blocks the inference to 3. For, assuming the duality of the strong and weak modals, epistemic "must" is probability 1. And, clearly enough, if p can't be the case (if its probability is 0), and q can't be the case (its probability is 0), then (p & q) can't be the case (their joint probability is 0). It is pretty easy to describe the operator-based epistemic logic that results from the Dougherty and Rysiew account of epistemic possibility (with the dual). That is just a red herring. The real issue is agglomeration of knowledge and justified belief. We will treat the failure of agglomeration of knowledge, but the same thoughts apply to justified belief.

Given fallibilism, we should deny that knowledge entails evidential certainty, that is, probability 1. But since Hawthorne tends toward a "knowledge-first" epistemology whereby, mirabile visu, all knowledge has probability 1, we should deny that all knowledge has maximal justification or epistemic support or strength or security.[8] So for any belief, x, were we to assign a level of epistemic security to x on our evidence, $Pr(x)$ would be less than 1. This doesn't mean that many (or most) of our beliefs are significantly below a probability of 1. Suppose, just for the purposes of illustration, that the threshold for knowledge is a probability of .95. Further, suppose I know p, where $Pr(p) = .99$ and I know q,

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[7] To be clear, Objection B drops all negations in steps 1–4, but this makes no overall difference.

[8] We will stick with "security," since a very radical "knowledge first" approach would assign all knowledge maximal strength. But it is a *datum* that not all knowledge is equally secure. Our knowledge that humans and chimps share a common ancestor is more secure than our knowledge (if it is knowledge) that no signal can travel faster than the speed of light. For ease of exposition, we will use the probability function "Pr(…)" to measure degree of epistemic security (we do not assume that the property of epistemic security satisfies finite or countable additivity here). Everything that we say here succinctly with the probability language can be said without it but at great addition to words.

where $Pr(q) = .99$. Here, the probability of $(p \& q) = .98$, which is safely above the threshold for knowledge. It isn't until we've added 6 conjuncts with a probability of .99 that the conjunction can drop below the threshold for knowledge. This means that on Dougherty and Rysiew's approach, there are many cases in which knowledge of $(p \& q)$ does follow from knowledge of p and knowledge of q.

In fact, Ram Neta has defined a coherence measure of belief which points out that the greater the quantity of randomly chosen items of knowledge one can conjoin before falling below the threshold of knowledge, the better that person's doxastic well-being.[9] For many ordinary beliefs, there will only be "theoretical doubt" the measure of which cannot even be discerned by the agent. We might represent this without significant distortion by saying that such propositions have a probability of about 99.99 %. It will take a conjunction of 513 such conjuncts to drop below .95. So, plausibly, for the decently well-situated agent in average circumstances, conjunction introduction will almost always work. Yet we must say of this what Hume said of constant conjunction of events in our experience: this common correlation does not amount to a relation of necessity. Any good risk manager knows that risk accumulates and there comes a point when aggregated risks over a collection of quite safe plans yields an unsafe plan. So the Dougherty and Rysiew approach validates the tendency to reason in the pattern Hawthorne indicates without elevating it to the level of totally unrestricted usage.

Finally, Hawthorne considers a "related worry" to Objection B (which we'll call "Objection C"): that "logically more complex constructions" that are "in the vicinity of 'I know that P and I know that Q but it might be that Not (P or Q)'" cause problems for Dougherty and Rysiew's approach as well (499). So consider the following schema for a quasi-CKA: "I know that P and I know that Q but it might be that Not (P or Q)" (499). We'll symbolize this as 'Kp, Kq, but $\Diamond \sim (p \vee q)$.' Since this is contradictory on the knowledge-based account of modality, Hawthorne, and others who endorse the standard account, have a handy explanation of the alleged infelicity if the utterance. Further, Hawthorne claims (499) that Dougherty and Rysiew's explanation of the infelicity of standard CKAs does not generalize to cover this example.

As an example of one such CKA, Hawthorne gives the following case: "'I know of each of the people on the list that they are innocent but it might be that one of them is guilty,'" which he claims sounds infelicitous (499). This example should be symbolized as 'Kp, Kq, $\ldots K\Phi$, but $\Diamond(\sim p \vee \sim q \ldots v \sim \Phi)$,' (where, for example, 'Kp' stands in for "I know that person$_1$ is innocent" and so forth). Note that, oddly, Hawthorne's example differs in form from his schema. This difference has important implications. Here's why.

Again, suppose that the threshold for knowledge is a probability[10] greater than or equal to .95 on a subject's evidence. We will assume independence throughout the

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[9] Neta presented this material in a talk titled "Coherence, the Preface, and the Lottery" given in May 2007 at the University of Edinburgh.

[10] Where, not to beg any questions, we harmlessly treat the probability as a measure of epistemic stability.

following example to make it tidy. Suppose, then, that p has a probability of .99 on my evidence and q has a probability of .98. From this, we know that Pr $\sim$(p v q) = .0002 but Pr($\sim$p v $\sim$q) = .0298. What does this demonstrate? That CKAs of the form "Kp, Kq, but $\Diamond\sim$(p v q)" (that is, those CKAs that follow the form of Hawthorne's schema) are harmless to Dougherty and Rysiew's approach, while CKAs of the form "Kp, Kq, but $\Diamond(\sim$p v $\sim$q)" may not be. After all, with respect to the first type of CKA, Pr $\sim$(p v q) will *always* be less than (or equal to) the lesser of Pr($\sim$p) and Pr($\sim$q), for $\sim$(p v q) is logically equivalent to ($\sim$p & $\sim$q), and the probability of a conjunction is less than the probability of either of the conjuncts.[11] So, in cases like this, if the fallibilist is comfortable asserting, "I know P but it might be that not-P," she should be even *more* comfortable asserting, "I know P and I know Q, but it might be that not (P or Q)," because "not (P or Q)" is even *less* likely than "not-P." Recall, this is treating the strong modality as expressing knowledge. It will not do so on the Dougherty and Rysiew account. And, as we've already discussed in our initial comments on *Deduction A*, even if we replace "must" for cognates of "knows" then there is still no problem.

What about CKAs of the form, "Kp, Kq, but $\Diamond(\sim$p v $\sim$q)"? These are to be taken more seriously, it seems. But—on the standard account—these CKAs are logically equivalent to "Kp, Kq, but $\sim$K(p & q)." And yet, this is no different than Objection B. That is, by arguing that this schema must be rejected because it conflicts with conjunction introduction, he is expressing his commitment to "Kp, Kq, so K(p & q)," which has already been considered. So, on one interpretation, Objection C fails to undermine Dougherty and Rysiew's approach, while on the other interpretation, Objection C, fails to provide any new evidence against Dougherty and Rysiew's approach.[12]

In response, however, Dougherty and Rysiew must still explain the infelicity of the relevant CKAs (granting for the sake of argument that these CKAs are infelicitous). Given Dougherty and Rysiew's approach, the following is true: inferring "K(p & q)" from "Kp" and "Kq" doesn't always work. Call this fact 'F.' F is revealed by Objection B. Hawthorne anticipates that advocates of Dougherty and Rysiew's approach can reasonably argue that people are typically ignorant of "the fact that insignificant chances add up to significant ones" (498). That's one way to go, but as we've argued above, inferring "K(p & q)" from "Kp" and "Kq" does work in many cases. So it seems reasonable to conclude that given that this inference is often successful and given that people are often ignorant of F, they are typically willing to assert "K(p & q)" given "Kp" and "Kq." As we pointed out (in our discussion of optimality theory in Section IV) there is a tendency of the human mind to, in Hume's terms, go from constant conjunction to necessitation. Hume was thinking of nomological necessitation but the same leap occurs with logical necessitation. There is a reliable inference from someone saying "Possibly P" to

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[11] When they are non-identical, and both are defined, and neither are 0.

[12] And if there are any other types of troublesome CKAs in the vicinity of Hawthorne's schema, he neglects to mention them.

their neither knowing P nor knowing ∼P. It is surprising when this doesn't hold.[13] Likewise, there is a reliable inference from Kp and Kq to K(p & q). Much of our knowledge is far enough above the minimum threshold of epistemic status for knowledge that it would take a large string of conjuncts to get the conjunction to drop below the threshold. Nevertheless, at some point they will. So while it will be very surprising when the inference fails, that surprise should not be mistaken for a good case that there is a relation of logical necessitation present. And so Objection C—namely, that it sounds weird when "∼K(p & q)" is conjoined with "Kp and Kq"—is explained by and rooted in Objection A, and Objection A is explained by and rooted in the facts revealed by Objection B. Thus, two of Hawthorne's objections to Dougherty and Rysiew's approach (Objections A and C) are ultimately grounded in Objection B, and, therefore, Objections A and C shouldn't count as independent evidence against Dougherty and Rysiew's approach. And Objection B has been sufficiently dealt with.

6 Conclusion[14]

Hawthorne raises a number of objections to Dougherty and Rysiew's approach, each of which fails to produce convincing evidence in favor of the standard approach over Dougherty and Rysiew's approach. Thus, Hawthorne has failed to provide us with reason to accept the standard approach over Dougherty and Rysiew's approach.

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[13] These issues are closely tied to the issues we are discussing, and so it is unsurprising that it turns up yet again in connection with Hawthorne's example. For we are accustomed to thinking of knowledge as eliminating possibilities. In ordinary life, knowing ∼P makes practically rational setting aside P for consideration. But this "ruling out" is a practical matter which some epistemologists mistake for a semantic phenomenon. For only absolute certainty could rule out an option in every logically possible scenario. So the first conjunct—e.g. "I know that P and I know that Q"—practically conveys that we should set aside all the possibilities in the set. But then along comes the second conjunct—e.g. "but it might be that not P or not Q"—and says that there remain some possibilities. So the WAM procedure is intact: the infelicity is the result of the fact that one conjunct extrasemantically conveys something in contradiction with the other conjunct. True, in the ordinary CKA it is the second "possibility conjunct" that does the work and here it is the first "knowledge conjunct," but that hardly matters.

[14] In the final section of his paper, Hawthorne argues that the "impulses that lead one to think that knowledge is not good enough for epistemic necessity or maximal epistemic probability are dangerous impulses" because "there may be no cogent conception of a condition that satisfies the demands that such impulses invite" (501).

In so arguing, Hawthorne considers four accounts, each of which ground epistemic necessity in something other than knowledge (499–501). Among the four accounts he considers is Dougherty and Rysiew's account of epistemic necessity, but even though Hawthorne critiques the other three accounts, he fails to offer any critique of Dougherty and Rysiew's account, so it is unclear why he thinks that their account fails to provide a "cogent conception" of epistemic necessity (499–501).

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