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A Puzzle About Stalnaker's Hypothesis

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Abstract According to Stalnaker's Hypothesis, the probability of an indicative conditional, $\Pr(\varphi \rightarrow \psi)$, equals the probability of the consequent conditional on its antecedent, $\Pr(\psi|\varphi)$. While the hypothesis is generally taken to have been conclusively refuted by Lewis' and others' triviality arguments, its descriptive adequacy has been confirmed in many experimental studies. In this paper, we consider some possible ways of resolving the apparent tension between the analytical and the empirical results relating to Stalnaker's Hypothesis and we argue that none offer a satisfactory resolution.

Keywords Conditionals · Probability · Stalnaker's Hypothesis · Triviality arguments

1 Introduction

Robert Stalnaker (1970) proposed the following as an adequacy condition on any semantics for indicative conditionals:[1]

$$\Pr(\varphi \rightarrow \psi) = \Pr(\psi|\varphi), \quad \text{for any } \varphi, \psi \text{ such that } \Pr(\varphi) > 0. \tag{1}$$

That is to say, the probability of any indicative conditional equals the probability of its consequent conditional on its antecedent (provided the latter is defined). This thesis, which is now commonly called "Stalnaker's Hypothesis," is generally found to have considerable intuitive force. Thus Bas van Fraassen (1976: pp. 272–273): "[T]he English statement of a conditional probability sounds exactly like that of the probability of a conditional. What is the probability that I throw a six if I throw an even number, if not the probability that: if I throw an even number, it will be a six?" More strikingly still, in the past ten years or so, various experimental psychologists have subjected (1) to empirical testing, and they invariably found that their participants' judgments of the probabilities of conditionals closely matched those participants' judgments of the corresponding conditional probabilities.[2]

2 Triviality

What makes these experimental results particularly striking is that they seem to run counter to David Lewis' and others' so-called triviality arguments.[3] These set out to show that, loosely stated, (1) cannot hold generally, and in effect can hold only for very special, "trivial," probability functions, which have various features that make them unrealistic as representations of people's states of graded belief.

For present purposes, it is unnecessary to review these triviality arguments in detail. However, to provide some

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[1] For the sake of brevity, we will refer to indicative conditionals as *conditionals* throughout.

[2] See, for instance, Hadjichristidis et al. (2001), Evans et al. (2003), Oaksford and Chater (2003), Oberauer and Wilhelm (2003), Over and Evans (2003), Evans and Over (2004), Weidenfeld et al. (2005), Evans et al. (2007), Oberauer et al. (2007), Over et al. (2007), and Douven and Verbrugge (2010).

[3] See Lewis (1976, 1986), Hájek (1989, 1994), Döring (1994), Hall (1994), and Etlin (2009).

idea of how these arguments proceed, and also for later reference, it will be useful to briefly go through Lewis' (1976: pp. 136–137) first, very simple triviality result. Take any Pr for which (1) holds, and let φ and ψ be such that both $\Pr(\varphi \wedge \psi) > 0$ and $\Pr(\varphi \wedge \neg\psi) > 0$. Then by the law of total probability,

$$\Pr(\varphi \to \psi) = \Pr(\varphi \to \psi|\psi)\Pr(\psi) + \Pr(\varphi \to \psi|\neg\psi)\Pr(\neg\psi), \tag{2}$$

which, using (1), can be rewritten as

$$\Pr(\varphi \to \psi) = \Pr(\psi|\varphi \wedge \psi)\Pr(\psi) + \Pr(\psi|\varphi \wedge \neg\psi)\Pr(\neg\psi), \tag{3}$$

which further simplifies to

$$\Pr(\varphi \to \psi) = 1 \times \Pr(\psi) + 0 \times \Pr(\neg\psi).$$

That is to say, for all Pr, φ, and ψ that satisfy the aforementioned conditions, it holds that $\Pr(\varphi \to \psi) = \Pr(\psi)$. This can hardly be true for every real person's belief state. Consider, for instance, that many would agree that it is rather unlikely that Chelsea will not do at least fairly well in next year's Premier League, but that it is not so unlikely that they will not do fairly well if they lose their six top players. Apart from (1), the assumptions of the above argument appear entirely uncontentious. So, unsurprisingly, the standard response to the argument has been to jettison (1).

3 Van Fraassen on the triviality results

But how, then, are we to make sense of the empirical data apparently supporting (1)? The situation is quite puzzling indeed: we have lots of empirical support for a thesis that, it seems, simply cannot be true. One attempt to argue that the tension between the analytical and the empirical results is only apparent starts by pointing at an unstated premise of the argument, to wit, that the triviality arguments take for granted the assumption that the semantics of "$\to$" is independent of people's belief states. To see how crucial this assumption is, consider again the above argument, and note that, without that assumption, the move from (2) to (3) would be illegitimate. Rather than on (1) simpliciter, the move relies on the following generalization of (1):

$$\Pr(\varphi \to \psi|\chi) = \Pr(\psi|\varphi \wedge \chi), \quad \text{for any } \varphi, \psi, \chi \text{ such that } \Pr(\varphi \wedge \chi) > 0. \tag{4}$$

However, those who want to leave open that the interpretation of the conditional is relativized to belief states can consistently hold onto (1) while denying (4). For the following claims are obviously compatible with each other: (i) $\Pr(\varphi \to \psi) = \Pr(\psi|\varphi)$; (ii) where $\Pr'$ comes from Pr by conditioning on χ, there is a conditional operator "$\to'$" such that $\Pr'(\varphi \to' \psi) = \Pr'(\psi|\varphi)$, and hence such that $\Pr(\varphi \to' \psi|\chi) = \Pr(\psi|\varphi \wedge \chi)$; (iii) it is not the case that $\Pr(\varphi \to \psi|\chi) = \Pr(\psi|\varphi \wedge \chi)$ (because "$\varphi \to \psi$" is meaningless in the scope of $\Pr'$, and hence in the scope of $\Pr(\cdot|\chi)$).[4]

This was first observed by van Fraassen in his (1976). In fact, he did much more in that paper to challenge the triviality results. In it, he showed that if the semantics for "$\to$" is relativized to epistemic states, then there do exist nontrivial probability functions satisfying (1), where "$\to$" may be considered as logically well-behaved.

While he acknowledged van Fraassen's point, Lewis (1976:138) dismissed the possibility that the interpretation of the conditional varies with people's belief states on the grounds that it would preclude disagreements about conditionals. However, Lewis' dismissal of van Fraassen's "tenability result" is rash. Consider the sentence

There was a lot of snow in Amsterdam last winter. (5)

People may disagree about what exactly counts as a lot, or how exactly Amsterdam is to be delineated from the rest of the world, or when winter begins and when it ends, and therefore may interpret the above sentence in somewhat different ways. By itself, that would seem no obstacle to genuine disagreement about the sentence. As long as they roughly agree on the said issues, one person affirming (5) and another person denying it would seem to be in disagreement. More generally put, if one person affirms a given sentence which another person denies, then it may be enough that their interpretations of that sentence are roughly the same to justify the assertion that they are in disagreement. Now, for all Lewis has said, the difference between "$\varphi \to \psi$," where "$\to$" is relativized to belief state Pr, and "$\varphi \to' \psi$," where "$\to'$" is relativized to belief state $\Pr'$, may be no greater than that between interpretations of (5) differing in the way just considered; in fact, it may hold for any pair of belief states that a given conditional has roughly the same meaning, whether it is interpreted relative to one member of the pair or relative to the other. So, for all we know, it is a mistake to claim that if van Fraassen is right, no disagreement about conditionals could occur.[5]

[4] This observation affects all extant triviality results except the one presented in Hájek (1989). However, the latter assumes people to have states of belief representable by probability functions with finite range. How restrictive this assumption is should not be overlooked (*pace* Bennett (2003: p. 74), who contends that it is innocuous). For instance, it would disallow us to model probabilistically the belief state of a scientist who thinks the value of a certain parameter is to be found in some interval of the reals.

[5] See Hájek and Hall (1994:96) for a similar response to Lewis' assessment of the relevance of van Fraassen's result.

To be sure, van Fraassen makes no attempt to show that interpretations of conditionals relativized to belief states are in general so related that apparent disagreements about a conditional can reasonably be said to be genuine. (Nor is the onus on van Fraassen to show this, it seems to us; but we leave this question aside here.) Nonetheless, the data about probabilities of conditionals obtained by Evans, Over, and others might seem to provide some indirect evidence for thinking that interpretations of conditionals must be related in the designated way. After all—some might say—that supposition would best explain those data together with the data showing that people sometimes appear to disagree about a given conditional. Be this as it may, it would be overly optimistic to conclude that van Fraassen's tenability result has salvaged (1). In the following we present a new argument against (1) that is compatible with the assumption that conditionals have their interpretations relative to belief states.

4 A new argument against Stalnaker's Hypothesis

The new argument assumes *probabilistic centering*, according to which

$$\Pr((\varphi \rightarrow \psi) \wedge \varphi) = \Pr(\varphi \wedge \psi), \qquad \text{for all } \varphi \text{ and } \psi. \tag{6}$$

It is worth stressing right away that this principle also follows from the conditional logic underlying van Fraassen's result. In fact, it seems that (6) is hard to deny for anyone committed to (1), for, given (1), (6) is equivalent to the principle

$$\Pr(\varphi \rightarrow (\varphi \rightarrow \psi)) = \Pr(\varphi \rightarrow \psi), \qquad \text{for all } \varphi \text{ and } \psi,$$

which would seem compelling on any account that permits the nesting of conditionals.[6] The crucial observation about our premises is that jointly they imply the probabilistic independence of a conditional from its antecedent, that is,

$$\Pr(\varphi \rightarrow \psi | \varphi) = \Pr(\varphi \rightarrow \psi), \quad \text{for all } \varphi,\ \psi \text{ such that } \Pr(\varphi) > 0. \tag{7}$$

For, assuming (1) and (6), we can derive for all φ such that $\Pr(\varphi) > 0$:[7]

$$\Pr(\varphi \rightarrow \psi | \varphi) = \frac{\Pr((\varphi \rightarrow \psi) \wedge \varphi)}{\Pr(\varphi)} = \frac{\Pr(\varphi \wedge \psi)}{\Pr(\varphi)} = \frac{\Pr(\varphi)\Pr(\psi|\varphi)}{\Pr(\varphi)} = \Pr(\psi|\varphi) = \Pr(\varphi \rightarrow \psi). \tag{8}$$

It is to be noted that this derivation does not hinge on (4), nor is it otherwise open to the objection that it assumes conditionals to have fixed interpretations across belief states.

Van Fraassen (1976:278) remarks that (7) "might provide a new fulcrum for the application of a philosophical critique or defence of [(1)]" (*ibid.*). To our knowledge, so far no one has examined this possibility. This is surprising, considering that (7) is deeply problematic. For, probabilistic independence being a symmetric relationship, from (7) it follows that

$$\Pr(\varphi | \varphi \rightarrow \psi) = \Pr(\varphi), \quad \text{for all } \varphi, \psi \text{ such that } \Pr(\varphi \rightarrow \psi) > 0,$$

so that (1) together with (6) entails that the probability of a factive sentence is unaffected by the supposition of any conditional that has that sentence as its antecedent—which seems plain wrong.

To see this, consider the following: Harry sees his friend Sue buying a skiing outfit. This surprises him a bit, because he did not know of any plans of hers to go on a skiing trip. He knows that she recently had an exam and thinks it unlikely that she passed. Then he meets Tom, another friend of Sue's, who is just on his way to Sue to hear whether she passed the exam, and who tells him: "If Sue passed the exam, her father will take her on a skiing vacation." Recalling his earlier observation, Harry now comes to find it more likely that Sue passed the exam. There seems nothing wrong with Harry's response. Indeed, it seems the natural response in the circumstances, if only because Sue's having passed the exam would explain, in light of Tom's testimony, why she bought the skiing outfit, something that, by itself, had appeared puzzling to Harry. In our view, this is a clear-cut case where, under the supposition of a conditional, one can rationally assign a probability to the conditional's antecedent that differs from its unconditional probability.

Of course, this example only challenges (1) on the assumption of (6), but, as we said, that is implicitly assumed in van Fraassen's tenability result as well. So, however ingeniously that result may work around the triviality arguments, it has not effectively salvaged (1). More generally, assuming that (6) is indispensable as a constraint on the probability of embedded conditionals, the above example shows that "→" will not really behave like a conditional on any account of conditionals that keeps to (1).

Some might have qualms that our example involves conditionalization on conditionals. As we see things, however, dynamic intuitions on conditionalization are not meant to be conceptually prior to static intuitions regarding

[6] Given (1), the principle is equivalent to $\Pr((\varphi \rightarrow \psi) \wedge \varphi) \div \Pr(\varphi) = \Pr(\varphi \wedge \psi) \div \Pr(\varphi)$, that is, (5).

[7] See Hájek and Hall (1994:86).

[8] For forceful arguments in favor of the view that intuitions on conditional probability are primitive, see Hájek (2003).

conditional probabilities.[8] In general, assessments of the first type are just a convenient way of reformulating assessments of the second type, exceptions being cases in which conditionalization cannot be soundly applied.[9] When putting our example in dynamic terms, we assumed that conditionalization would be safe in cases like that one. But nothing hinges essentially on this assumption. Alternatively, we could have formulated the example in purely static terms of conditional probability. If Harry were asked the question, "Supposing that, in fact, Sue's father agreed to take her on a skiing vacation if she passed the exam, would you remain as doubtful as you are about Sue's having passed the exam?" then there would be nothing odd or problematic in Harry's answering "no"—or so we suggested above, just in other words.

5 Escape routes?

There are more radical ways than van Fraassen's to respond to the triviality arguments. One is to side with Adams (1975) in holding that conditionals do not express propositions and therefore do not embed in other conditionals or in the scope of logical connectives, and that meaningful natural language sentences with the apparent logical form of embedded conditionals have in fact the logical form of unembedded simple conditionals, that is, conditionals whose antecedent and consequent are both factive.[10] This allows one to stick to a version of (1) with all variables restricted to factive sentences, provided one reads "$\Pr(\varphi \to \psi)$ " as measuring some other quantity than probability of truth (like assertability or acceptability). The resulting thesis is commonly known as "Adams' Thesis." To see how it avoids our argument against (1), it is enough to note that, on Adams' account, (6) and (7) are both ill-formed, because the terms on the left-hand side of the respective equations are not defined.

We have two remarks about this. First, as Vann McGee (1989: p. 485) notes, because of the mentioned restriction to factive sentences and its inability to make sense of Boolean combinations of conditionals, Adams' Thesis has been generally found to be of limited significance. However, it is hard to go beyond Adams' Thesis while steering clear of the problem we uncovered for van Fraassen. Indeed, McGee's own attempt to strengthen the thesis by offering rules for computing probabilities for certain forms of embedding conditional can be readily seen to fall victim to the above argument as well. McGee's result crucially hinges on the *import–export principle*, which, again with all the variables restricted to factive sentences, amounts to this:

$$\Pr(\varphi \to (\psi \to \chi)) = \Pr((\varphi \wedge \psi) \to \chi).$$

As an instance, we get

$$\Pr(\varphi \to (\varphi \to \psi)) = \Pr((\varphi \wedge \varphi) \to \psi).$$

By the strengthened version of Adams' Thesis that McGee assumes, this yields

$$\Pr(\varphi \to \psi|\varphi) = \Pr(\psi|\varphi),$$

that is,

$$\frac{\Pr((\varphi \to \psi) \wedge \varphi)}{\Pr(\varphi)} = \frac{\Pr(\varphi \wedge \psi)}{\Pr(\varphi)}.$$

Thus,

$$\Pr((\varphi \to \psi) \wedge \varphi) = \Pr(\varphi \wedge \psi).$$

And this is enough, we saw, to place the argument of the previous section in position.[11]

Second, while many philosophers take Adams' Thesis to be descriptively correct (even if limited in scope, as noted), experimental results presented in Igor Douven's and Sara Verbrugge's (2010) show that it is *not*: people's acceptability judgments about conditionals do not generally match their judgments of the corresponding conditional probabilities.[12] In their experiments, Douven and Verbrugge consider so-called inferential conditionals—conditionals in which there is an inferential connection between antecedent and consequent—and they differentiate among three types of such conditionals, to wit, those in which the inferential connection is deductive, those in which it is abductive, and those in which it is inductive. Douven and Verbrugge find that especially for the conditionals of the inductive type, the match between acceptability and conditional probability is very poor; the two are not even moderately correlated. This should give pause to those attracted to (1) who, under pressure of the triviality arguments, consider a retreat to Adams' Thesis: while there is,

[8] For forceful arguments in favor of the view that intuitions on conditional probability are primitive, see Hájek (2003).

[9] See, e.g., Howson (1997).

[10] A sentence is *factive* if it is not itself conditional in form and does not contain a conditional.

[11] Dietz and Douven (2010) provides still another argument against (1) on the basis of probabilistic constraints for embedded conditionals taken from McGee (1989).

[12] A reference to personal communication with Adams in Hájek and Hall (1994:77) makes it clear that Adams prefers to think of $\Pr(\varphi|\psi)$ as measuring the acceptability of $\varphi \to \psi$. Many authors state Adams' Thesis explicitly in terms of acceptance or acceptability. See, e.g., Mellor (1993:233): "The Adams thesis ... is that my *degree of acceptance* of a conditional 'If P, Q ' is equal to my *conditional credence* in Q given P," where by "conditional credence" Mellor means conditional subjective probability. See also Arló-Costa (2001, Section 3).

as said, a welter of evidence in favor of the former, there is strong evidence *against* the latter.

As an aside, we note that Douven and Verbrugge compared probability judgments with acceptability judgments for a set of conditionals and found that, in general, the two differed significantly. (In fact, they found that people's probability judgments for conditionals do closely match their corresponding conditional probability judgments, thereby offering further support to (1).) This rules out what otherwise might have helped reconcile the empirical results pertaining to (1) with the triviality arguments, to wit, the possibility that the participants in the various experiments about (1) interpreted the questions about the probabilities of the conditionals as really asking for those conditionals' degrees of acceptability.

Finally, we mention that other frameworks that supply sufficient means for circumventing the triviality results are also committed to (6). In particular, we have in mind Stalnaker's and Jeffrey's framework presented in their (1994) (where the semantics of "$\rightarrow$" depends on epistemic states) and, more generally, conditional event algebras (see, e.g., Milne 1997). As a result, these also face the counterexample to (1) presented above.[13]

6 The approximation reply

At least some of the experimental psychologists working on conditionals have been aware of, and have commented on, the tension between the data about (1) and the triviality results of Lewis and others. Most notably, Evans and Over (2004) have argued that the triviality results are not really incompatible with their experimental data. Specifically, they claim that while the former show that (1) cannot hold *strictly*, it does not follow from these results that (1) could not still hold *approximately*. They even think that, supposing conditionals to be propositions, and to have the truth conditions assigned to them by Stalnaker's (1968, 1975) semantics—according to which $\varphi \rightarrow \psi$ is true iff ψ is true in the "closest" possible world in which φ is true, where "closest" is supposed to mean "most similar to what we take to be the actual world"—"people might often judge [a conditional's] probability to be *close to* the conditional probability" (Evans and Over 2004: p. 29), so close, in fact, that it is "hard to find a significant difference between the two probabilities in an experiment" (*ibid*.).

However, it is a mistake to think that the approximate version of (1) that Evans and Over envision is free from triviality. Consider again Lewis' first triviality argument stated in Sect. 2. Even assuming only the approximate version of (1), with "=" replaced by "$\approx$," we get that, for any pair φ, ψ such that $\Pr(\varphi \wedge \psi)$ and $\Pr(\varphi \wedge \neg\psi)$ are both positive, $\Pr(\psi|\varphi)$ cannot diverge widely from $\Pr(\psi)$. That would seem problematic enough, inasmuch as this condition cannot plausibly be thought to hold generally for people's graded beliefs. To slightly modify an earlier example, one may think it *very likely* that Chelsea will do well in next year's Premier League, yet think it *very unlikely* that they will do well on the supposition that they will lose their six best players. Admittedly, this is a rather loose argument, which does not make precise the notion of approximation or that of wide divergence. But no such shortcoming attaches to Hájek's and Hall's (1994: pp. 102–105) argument, which shows in a rigorous fashion that, "unless the standards of approximation are so weak as to make the claim trivial" (p. 104), the "approximate version" of (1) is no more tenable than the strict one.[14]

Even more relevantly, the observation that the data only support the approximate version of (1) does nothing to mitigate the force of our counterexample presented in Sect. 4. To see this, first note that assuming just the approximate version of (1) would require that we replace the last (and only the last) equality sign in (8) by "$\approx$." So, we would still have that $\Pr(\varphi \rightarrow \psi|\varphi) \approx \Pr(\varphi \rightarrow \psi)$. Second, it is readily seen that, like strict probabilistic independence, approximate probabilistic independence is symmetric; that is, for all Pr, φ, and ψ such that $\Pr(\varphi)$ and $\Pr(\psi)$ are both positive, $\Pr(\varphi|\psi) \approx \Pr(\varphi)$ iff $\Pr(\psi|\varphi) \approx \Pr(\psi)$. In other words, supposing a conditional to hold should at most somewhat affect one's confidence in the conditional's antecedent. And it is perfectly reasonable to assume that, in our example, after learning from Tom that Sue's father will take her on a skiing vacation if she passes the exam, Harry becomes much more confident that Sue passed the exam (even if, perhaps, he would still be rather doubtful that she passed).

But perhaps *something* along the lines of Evans' and Over's response will work. In particular, it might be pointed out that the triviality results all suppose that the Pr function occurring in (1) (as well as in the approximate version thereof) is a probability function in the strictest sense. However, in the Bayesian community it is generally acknowledged that no real human being has degrees of belief that are representable by a probability function so understood. It is presumably already false that the propositions (or, if one prefers, sentences) to which we assign degrees of belief form what mathematicians call a field, but even if they do, the degrees of belief we assign to them will

[13] As an aside, we note that the example also puts pressure on the material conditional interpretation of conditionals; see Douven and Romeijn (2010, Sect. 2).

[14] Hájek and Hall (1994: p. 104) actually present two triviality results militating against the approximate version of (1): what they call the "Strengthened Lewis Result" and the "Perturbation Result." The reader is referred to their paper for the details. For a third triviality result against the approximate version of (1), see Morgan (1999).

in general not be probabilities. For instance, we will not assign probability 1 to every tautology. Also, an entailment may not be evident to us, and as a result it may happen that we believe the entailing proposition to a higher degree than the entailed proposition. Presumably most of us should expect still further inconsistencies in our degrees of belief (inconsistencies from a probabilistic perspective, that is). The best we can do, realistically speaking, is to have degrees-of-belief functions that *approximate* probability functions. Even though it may be hard to make the relevant notion of approximation precise in a motivated way, it might be said to be at least intuitively clear. It might thus be thought that, rather than claim that their participants had probability functions that approximately satisfied (1), Evans and Over should insist that these participants, like other ordinary mortals, had degrees-of-belief functions that were only approximately probability functions and that satisfied (or at least approximately satisfied) an analog of (1) for precisely such functions, to wit,

$$\mathrm{DB}(\varphi \rightarrow \psi) = \mathrm{DB}(\psi|\varphi), \tag{9}$$

where DB stands for a degrees-of-belief function of a non-ideal rational person and that approximates a probability function. It might further be noted that, in the context of the kind of experimental work at issue here, this analog of (1) rather than (1) itself is of interest: we knew all along that the latter does not hold, for we knew all along that real people (as opposed to ideal Bayesian reasoners) do not literally assign probabilities to the various propositions expressible in their language.

Unfortunately, this will not fly. Consider once more the triviality argument stated in Sect. 2. If it is correct that real people's degrees-of-belief functions at best approximate probability functions, then presumably we may not have an equivalent of the law of total probability for non-ideal rational degrees of beliefs. Much less can we generally assume that $\mathrm{DB}(\psi|\varphi \wedge \psi) = 1$, or that $\mathrm{DB}(\psi|\varphi \wedge \neg\psi) = 0$. Rational persons are not logically omniscient, and not all instances of, for example, $\psi \wedge (\varphi \wedge \neg\psi)$ may be easily recognizable as being logically inconsistent. Note, however, that this does little to blunt the force of the said triviality argument. For while the foregoing makes it reasonable to hold that we do not have *in general* that $\mathrm{DB}(\varphi \rightarrow \psi) = \mathrm{DB}(\psi)$, we still do have this for each particular conditional $\varphi \rightarrow \psi$ for which an equivalent of the law of total probability does hold on our degrees-of-belief function and for which we can easily recognize that $\varphi \wedge \psi$ entails ψ (such that, one may suppose, we believe ψ to a degree of 1 conditional on $\varphi \wedge \psi$) and $\varphi \wedge \neg\psi$ is inconsistent with ψ (such that, one may suppose, we believe ψ to a degree of 0 conditional on $\varphi \wedge \neg\psi$). More exactly, whether an analog of the triviality argument of Sect. 2 (or, for that matter, of any of the extant triviality arguments) can be run in terms of our non-idealized degrees of belief *for a particular conditional* depends only on a fragment of our degrees-of-belief function. If the relevant fragment can be extended to a probability function—whether or not that coincides with our degrees-of-belief function—it follows from (9) that one's degree of belief in the conditional equals one's degree of belief in the conditional's consequent. It is reasonable to think that, for many conditionals, the aforementioned condition holds. However, it is equally reasonable to think that of many of those we will be more or less confident than we are of their consequent. The example about Chelsea doing or not doing well in next year's Premier League may again be a case of point (at least for many rational people).

7 Concluding remarks

In light of the foregoing negative results, the question of what we are to make of the experimental data apparently supporting (1) has only become more pressing. Must they be reinterpreted, as showing something not about people's probabilities for conditionals but about some other quantity related to conditionals? (But then, which quantity could it be? Not acceptability, as we said.) Or should they be taken at face value, supplying a reason to reconsider the triviality arguments as well as our argument from Sect. 4 and to see whether there are not still some assumptions underlying these arguments that should be given up? We are not currently able to answer these questions. However, it seems fair to say that we have presented a puzzle that should be of interest to philosophers and psychologists alike, and that, we think, deserves much more attention than it has hitherto received.

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