Vagueness, conditionals and probability

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This paper explores the interaction of well-motivated (if controversial) principles governing the probability conditionals, with accounts of what it is for a sentence to be indefinite.

The conclusion can be played in a variety of ways. It could be regarded as a new reason to be suspicious of the intuitive data about the probability of conditionals; or, holding fixed the data, it could be used to give traction on what sorts of philosophical account of indefiniteness are acceptable. Regarded in the latter way, the project here is a direct descendent of Robert Stalnaker's (1970).

The paper outlines the various options, and shows that popular theories of indefiniteness such as orthodox supervaluationism and many-valued theories are incompatible with the results. An appendix compares the results obtained here with the 'impossibility' results descending from Lewis (1976).

1 The basic argument stated

Suppose we have a deck of black cards and red cards. There are only seven red cards. Of these, only five are diamonds. I am about to draw a card at random. Consider the sentence:

(S) If I draw a red card, it'll be a diamond

I claim:

Thesis 1: The probability of *S* being true is 5/7

Thesis 2: In cases where I draw a black card, *S* is neither definitely true nor definitely false.

The first thesis says that the probability of this conditional is equal to the conditional probability of the drawn card being red, given that it's a diamond. This is pretheoretically a very plausible claim—and is supported by empirical studies of folk judgements of the probabilities of conditionals (Evans and Over, 2004). The second premise is also very enticing. If the draw is truly random, and has not been held yet, how could there be a fact of the matter about the truth of this conditional?

Let us add to the example, by specifying that the total size of the pack of cards is 100, of which 93 are black. Consider the sentence:

(S*) It not definitely the case that, if I draw a red card, it'll be a diamond

On the basis of thesis 2, we move to:

Thesis 3: The probability of S^* is at least 93/100

Putting this all together, the probability of *S* is just over half (5/7). But it's very very likely that *S* is indefinite (93/100).

*Thanks. . .

2 Clarifications

1. The argument is stated in terms of 'probability'. But this could be taken to mean many things. It could, for example, be taken to mean *subjective probability*: the level of confidence you have in the conditional (and in turn, we could distinguish the subjective probabilities one *ought* to have in the circumstances, from a claim on what level of confidence an individual *actually has*. It could be taken to mean *evidential probability*: the likelihood of the proposition given the available evidence. It could mean the *objective chance* of the proposition holding. No doubt their are other interpretations.

   I think the thesis is plausible under at least the subjective probability and evidential probability interpretations.

2. The argument case is underdescribed in many ways. What is particularly important here is that the draw is *random*: the draw would take place through some chancy mechanism which gives each card an equal chance of being drawn. I take it that ordinary processes like rolling a dice, or even taking a decision of which card to pick from amongst an array laid out on a desk are examples of appropriately chancy processes.[1] I will take it the decision procedure doesn't proceed by 'narrowing down' the options: if in fact one takes card C, then it should not be the case that had one not taken card C, one would have taken a card adjacent to it (the dice rolling way of deciding is a good one here).

   Likewise, one might start imagining scenarios with odd sorts of interventions. Perhaps there is an invisible genie who is disposed to magically change any card I draw into a diamond. If such a world were actual, then thesis 2 would seem wrong. I ask you to rule out such outlandish scenarios: the scenario we're concerned with is one where I am rightly certain that if I decide to randomly draw the card, I do so without undue interference.

3. The argument is stated in absolutist terms: it is claimed that the subjective probability of S is 5/7, in the original setup. Perhaps this isn't quite right: perhaps the probability of this conditional is only approximately 5/7. The main conclusions of this paper survive translation into the approximate setting.[2]

3 The generalized argument stated

The numbers involved in the basic argument seem incidental. We can state the premises in a more general form. Suppose we have a deck of n cards, of which m are red (and $n-m$ black). Suppose that k out of the m red cards are diamonds. In this context:

Thesis 1+: The probability of 'S' being true is k/m.

Thesis 2+: In all the cases where one draws a black card, 'S' is neither definitely true nor definitely false, and so:

Thesis 3+: The probability of S^* is at least $(n-m)/n$

[1] I'm taking the chances here to be of the sort that quite generally as ascribed to macroscopic processes by statistical mechanics. See **?** for discussion of the philosophical issues in play here. If one wished to do so, surely one could specify a case where some radioactive decay process was involved in selecting the card, bringing more fundamental physical chances into play.

[2] Typically, we will be able to derive an approximate version of our results. Some cases of the generalized result may be given up altogether: but the approximate results that remain are enough to do damage.

For particular choices of n, m and k, this scheme seems exactly as good as the original. Notice two aspects. By making m large and setting $k = m - 1$, we can make the probability of S arbitrarily high. And by choosing n large in relation to m, we can make the probability that S is indefinite arbitrarily high.

By the laws of classical probability, if two sentences are each invested with arbitrarily high probability, their conjunction must take arbitrarily high probability too. Thus, by suitable choice of n, m, k we have a recipe for finding a p which makes the following true for any choice of δ, however small:

Probability('$p \wedge$ it is not definitely the case that p')=1-δ

I would be interested in this consequence of our theses even if I did not personally endorse them. But obviously the more seriously you take them, the more interesting they will be.

4 Thesis 1

The basic reason for liking thesis 1 and 1+ is their strong intuitive appeal. The link between something like a probability of a simple conditional and the corresponding conditional probability is a centrepiece of many accounts of the indicative conditional; clearly many philosophers have found it compelling enough to build theories around it (or some surrogate). The cases under consideration in thesis 1 and 1+ are particularly paradigmatic: so even if one thought the thesis admitted exceptions, one should hesitate to drop these cases.

If one doesn't personally feel the pull of thesis 1, one might be tempted to dismiss this data as the product of the theoretical corruption of the intuitions of philosophers. But that doesn't hold water. Empirical studies back up the claim that in these paradigmatic sorts of cases, the probability of conditionals are judged in accordance with the conditional probabilities.[3]

Of course, one could take the intuitions to be faulty in these cases, or the population at large to be systematically in error when judging such matters. But we should avoid such error theories where we can. Rather than wishing the data away, we need to *explain it* (I mention some possibilities below). But the straightforward reaction to the fact that commonsense agrees with thesis 1 and 1+, is to think—pro tem—that thesis 1 and 1+ are correct.

Why might you move away from this default position? You might be in favour of one of the famous revisionary theories of the indicative conditionals; such as the theory on which it expresses a material conditional (Jackson, 1987), or expresses no proposition at all (Edgington, 1986). Such theories will deny at least one of our premises. My reasons for not liking such theories are not particularly original: both, I think, struggle to give a satisfactory account of the usage of the conditional in natural language (particularly when it comes to quantificational and compound contexts such as "no student passed if he goofed off; but every teacher was proud if he got fifty per cent through."). The debate that ensues can be found in good survey papers, and I have little to add here.[4]

Second, you might be in favour of some less revisionary theory. For example, some modal accounts of indicative conditionals agree that 'if a red card is drawn, it'll be a diamond' expresses a proposition (and isn't the one formed from the material conditional), but holds that it'll be a proposition whose probability isn't equal to the conditional probability. Examples of such views include the variably strict conditional approach of Nolan (2003); the contextually strict conditional approaches of von Fintel (e.g. 2001) and Gillies (e.g. 2007); and the modal restrictor approach of Kratzer (1986). Such theories can't be accused of ignoring or leaving

[3] Evans and Over (2004, p.XX).

[4] Bennett (2003) is an excellent starting point.

unexplained the wider linguistic data concerning conditionals. However, they do face an outstanding challenge: to explain why folk judgements of the probability of conditionals are so out of step with the predictions of their theory.

Some of these approaches take on this burden directly. For example, Kratzer suggests that 'It's 5/7 likely that if a red card is drawn, then it's a diamond' has the logical form '[5/7 likely: red card is drawn](a diamond is drawn)' and is predicted by her theory to express the relative proportion of *situations where a red card is drawn* where a diamond is drawn. So the net effect is that these locutions match the conditional probabilities. Likewise, Gillies' account of the interaction of modals and conditionals predicts that the logical form 'if a red card is drawn, [5/7 likely](a diamond is drawn)' will come out true due to the shiftiness in the context with respect to which the operator in the consequent is evaluated. But it's not so clear that such logical forms are plausibly attributed to claims of the form:

> There's a 5/7 likelihood that the following sentence is true: If a red card is drawn, it's a diamond.

But the empirical data backing thesis 1 specifically backs such formulations. Prima facie, given there is a proposition expressed by the latter sentence (e.g., on the Kratzer view, with the 'if' clause restricting a covert epistemic modal 'must'), it'd be deeply mysterious that intuitions in these sort of carefully constructed locutions don't track the probability of the proposition expressed.[5]

The other thing that must be checked if one wants to explain away the apparent data in terms of interactions between the 'if' and the probability operator, is whether the theory generalizes to slightly more complex cases in the right way. Quantified conditionals are a natural next step. Consider:

> it's highly likely that on every occasion a card is drawn, the card's a diamond if it's red.

It's not straightforward to see how the two approaches mentioned above can assign the right truth conditions to this sentence.[6]

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[5]The Kratzer story gives an elegant account of locutions like: 'if you crash the car and kill Harry, you may crash the car and kill Suzy'. The Kratzer reading would be: (There is a permissible situation x: in x you crash the car and kill Harry)(You crash the car and kill Suzy). This renders the sentence false, as it should (crashing with fatal results isn't permissible). The same reading is offered for: 'you may crash and kill Suzy if you crash and kill Harry'.

But notice what happens when we parallel the somewhat artificial locution above: 'it is permissible for you to make true the following sentence: if you crash the car and kill Harry, you crash the card and kill Suzy'. This seems fine; just by giving Harry and Suzy a lift and driving safely you can make that true. (I owe this example to Brian Weatherson, who used it in a slightly different context.)

Similarly, 'if you're male and unmarried, you're necessarily a bachelor' is naturally read as false (your bachelorhood isn't essential to you). But famously there's an alternative reading of such locutions, which we communicate to first year philosophy students, seemingly unambiguously, by "The following sentence is necessarily true: if you're male and unmarried, you're a bachelor". It would be deeply mysterious if probability operators alone among modals didn't pattern this way.

[6]That 'likely' can scope over 'every occasion' is important. Suppose that the probability of diamond given red is high for each occasion where a card is drawn; but there are so many such occasions that it becomes highly likely that at some point a red non-diamond is drawn. There's a reading of the displayed sentence where it is false in such circumstances.

But can we still apply the Kratzer trick to get the right results in this case? If 'likely' has wider scope than 'every occasion', and this in turn binds 'the card' in the if-clause, it's hard to see how the if-clause could be a restrictor on 'likely'. Likewise, on the von Fintel/Gillies approach it's hard to see how we could take the context relevant to assessing the probability to be updated by the if-clause. But unless we can get the if-clause to interact with the

There's a final, and much more general reason to be distrustful of the data. Didn't David Lewis (1976) (and the literature that followed his publication, in particular Hájek and Hall (2004) and Hájek (2004)) show us that things like Thesis 1+ just couldn't be sustained, however intuitive they are? The hypothesis that, quite generally, the probability of a conditional is the corresponding conditional probability is sometimes called CCCP; and Thesis 3+ might be viewed as a *restricted* form of CCCP. In an appendix to this paper, I sketch some main lines of this literature and explain why the hypotheses here are still worth investigating.[7]

5 Thesis 2

Supposing that thesis 1 or 1+ is accepted, can we resist the argument by denying thesis 2 or 2+?

One way of arguing for thesis 2 is to appeal to particular theories of the indicative conditional. Closest-world modal theories are a case in point: given that the antecedent 'I draw a red' is actually false, these theories tie the truth value of S to the truth value of the consequent at the *closest* world where the antecedent is true.[8] But the setup is carefully designed so that it is implausible that worlds where different red cards are drawn are any closer or further away than each other: none are more similar to actually than each other; none involve features are known or believed or presupposed not to obtain; no other factors differ amongst the worlds that plausibly contribute to closeness.[9] But if none of factors that determine whether one world is closer than another differentiates among a range of worlds, plausibly it is indefinite which is closest.[10] But if it is indefinite whether a diamond or a non-diamond world is closest, then the conditional itself is indefinite. Many theories of conditionals (especially those still in the running once Thesis 1 is taken on board) are already committed to Thesis 2.

It is interesting, however, to see if we can find arguments for Thesis 2 independently of any particular theory of indicative conditionals. It looks is if it is possible to do so.

In light of earlier clarifications, we note something interesting about all the non-red cases. At the time at which the conditional is evaluated, it is supposed to be an objectively chancy (and unbiased) matter which card would be drawn. So no red card is only more objectively likely than any other. Worlds where a black card is drawn may vary in many ways—which black card

probability, we can quickly find counterintuitive results (hint: consider cases where the probability of drawing red at all is very low, but the probability of drawing a red non-diamond is orders of magnitude lower).

Perhaps with some fancy footwork we can extend the ideas to this case: but it's not clearly a *straightforward* generalization.

[7]To anticipate: many of the results in the literature simply do not apply to restrictions of CCCP. Nor, I think, should the failure of the general version be seen as undermining the intuitive data that support Thesis 1, since intuitive data never supported the problematic generalizations. The outstanding remaining challenge is that of Hájek (2004), who develops an argument against even single instances of CCCP. Hájek's argument, however, has to make assumptions about how belief updating functions—it is *dynamic* rather than *static* result. And this, I think, gives wriggle room for the friend of instances of CCCP.

[8]See Stalnaker (1968); Weatherson (2001), Nolan (2003), Davis (cite), Williams (mingb).

[9]Williams (minga) argues that in some superficially similar cases, there are in fact features to which we can appeal to push worlds further away. If certain worlds are objectively *atypical* relative to the objective chances, they can be pushed further out. This is intended to show why certain non-zero probability events do not undermine the truth of ordinary counterfactuals. The current setup is not of the form that is amenable to this tactic (allowing sheer *improbability* to play this role is not a viable option: see Lewis (cite, appendices)).

[10]The alternative is to declare the worlds *equally* close, and, analogously to Lewis (1979), regard the conditional as false unless the consequent is true at all the closest antecedent-worlds. This is the line taken by Nolan (2003), and extension to the current case has S being false whenever the antecedent fails to hold. Such a theory is already in violation of Thesis 1.

The indefiniteness line is taken explicitly by Stalnaker (cite) and Weatherson (2001). Nolan explicitly flags the possibility of taking this line as a variation on his theory (much of which is independent of this particular issue.

is drawn, whether I scratch my nose immediately beforehand, and so forth—but none of these look like the sort of thing that could plausibly make the difference to the conditional being true or false.[11] Let's emphasize this feature of the intended setting:

> **Irrelevance** The (non-zero probability) non-red possibilities differ from each other only in ways that are intuitively irrelevant for the truth-status of the conditional.[12]

If the non-red situations vary only in ways that seem intuitively irrelevant to the truth-value of the conditional, then one would naturally assume that the truth-status of the conditional is the same throughout these cases. This is of course something stronger than the IRRELEVANCE, namely:

> **Uniformity**. Given that IRRELEVANCE holds, either:
>
> 1. *S* is true in the non-red cases which are live possibilities, or
> 2. *S* is false in all these cases, or
> 3. *S* is indefinite in all these cases.

IRRELEVANCE, I think, supports but does not entail UNIFORMITY, which supports but does not entail THESIS 2. Let us consider the moves in more detail.

Suppose UNIFORMITY is accepted. Thesis 2 is simply the third disjunct. So we can argue for Thesis 2 if we can argue against the other two disjuncts. At first, this might seem straightforward. For recall at this stage of the dialectic we are holding fixed Thesis 1: so it must be that the overall probability of *S* is 5/7. According to option (1), the non-red cases are all true. But all the probability invested in non-red cases would often be invested in cases where the conditional is true, and we're back in a situation where the probability of the conditional is far greater than 5/7. According to option (2), the non-red cases are all false. But all the probability invested in non-red cases are invested in cases where the conditional is false, and we're back in a situation where the probability of the conditional is far lower than 5/7. So neither option looks likely.

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[11] A possible worry. We have to this point formulated the key principles metalinguistically. So even granted that the non-red cases do not differ in ways intuitively relevant to what one might call 'conditional facts', we might also wonder whether they vary in ways relevant to whether a certain conditional sentence *expresses something* which is true.

Perhaps the non-red cases differ over patterns of usage of conditional statements among the wider community (albeit in minor ways). Should such minor variations in usage be counted as relevant to the truth-value of *S*? It is extremely important to Williamson's epistemicist that such minor variations in usage do generate differences in what propositions sentences express, and so (sometimes) what truth-value they have. But I take it the incredulous stare that Williamson's view generates are due to the fact that such minor variations in usage are *intuitively* irrelevant to the question of what proposition they express. But that may just be an instance of the fallacy underlying sorites reading: we all know that big changes in usage are relevant to what propositions sentences express; and a big change can be nothing but many small changes added together. So it is simply *wrong*, however appealing, to think that minor changes is usage are irrelevant in this respect.

Nevertheless, in the case at hand, we should stick to our guns. Variation in usage (slightly more people calling a conditional false, for example), might be relevant to whether all non-red worlds are worlds where a conditional *S* is true. But it's hard to see how variations in usage could be such as to be relevant to the question of whether a world where I scratch my nose, rather than one where I don't, is a non-red world where *S* is truth. So IRRELEVANCE can be defended, even if broader principles are more dubious.

[12] This is stated in a strong form. On the subjective probability interpretation, it requires that any possibility one has non-zero credence in, be one that is in this way irrelevant. One could defend this by refining (and idealizing) the setting so that the subject can rule out with certainty any sort of difference that might be relevant to the truth of *S*. Or one might weaken the principle so that it requires only that the vast majority of probability be invested in possibilities that differ only irrelevantly, and ultimately argue for a slightly weakened form of thesis 2.

However, this overlooks the possibility of the *Agnostic*. According to the agnostic, we are (epistemically or subjectively) uncertain over which of (1) or (2) obtains (we are presuming that the Agnostic is certain of Uniformity itself).[13] Prima facie that seems reasonable enough: it would not be a shock if the right way to represent folk beliefs about conditionals was to present them as *unopinionated* about delicate matters of theory. Moreover, if someone is uncertain *in exactly the right kind of way*, then they may still satisfy Thesis 1. For example, if they give 5/7 probability to (1), and 2/7 probability to (2), then overall their probability in S can be 5/7.

There are reasons to think that the Agnostic stance is unstable, however. The agnosticism needs to be too finely tuned to the details of the original case. For example, consider the conditional S': 'if I draw a red, I draw a heart'. The probability of this should be 2'. But treating this in a fashion parallel to S, to get a result that assigns probability 2/7 to S', the Agnostic would have to invest 2/7 probability in (the analogue of?) (1), and 5/7 in (the analogue of?) (2): exactly the opposite of what is needed for S. Similar tailoring would be needed for each case. so while ordinary folk would be represented as *uncertain*, their uncertainty would be tailored in an incredibly fine way to each individual conditional. Agnosticism would seem appealing if one could think of it as uncertainty between a rule that told us that *generally* conditionals like S are false when their antecedents don't hold; and one that told us that *generally* such conditionals are true in such circumstances. But nothing like this sort of uncertainty can be involved. So although UNIFORMITY does not entail Thesis 2, once we've accepted UNIFORMITY and Thesis 1, there doesn't look any principled way to resist taking the further step.

What of UNIFORMITY itself? This was motivated on the basis of a particular feature of the case at hand, namely IRRELEVANCE. Picture the ideal agent, with full knowledge of the non-conditional facts about the world, attempting to assess the truth-value of the conditional. What everyone should admit is that—given INDIFFERENCE—it's hard to see how even such an ideal agent could be justified in assigning different truth-values in the varying non-red cases.

But this is an observation about (idealized) epistemology, and one might resist drawing a metaphysical moral from these intuitions about epistemology. Perhaps conditionals do have differing (determinate) truth values in the various cases, in exactly the proportion to render true thesis 1. That is a suggestion that while IRRELEVANCE holds, UNIFORMITY does not. We can call this position 'Molinist'.

Notice that this Molinist may, but need not think that conditional facts are *sui generis* in that they fail to supervene on the non-conditional facts. For all we have said, they may still hold that there can be *no difference* in the truth values of the conditional statement or proposition, without corresponding variation in non-conditional facts (my scratching my nose or not, for example).[14]

There is clearly something unattractively 'brutalistic' about the Molinist position, even if they retain supervenience. Here is an attempt to articulate this:

[13] We might also consider an extended Agnostic, who is uncertain between options (1), (2) and (3). I set this aside for now.

It is a nice issue how to represent the Agnostic's evidential probability or credences, for it is consistent with this view that all the non-red situations are *in fact* cases where the conditional is true. Two options suggest themselves: having some kind of higher-order uncertainty over probability distributions; or enriching a single probability space with an extra element corresponding to different possible meanings of the indicative conditional.

[14] At one point, I thought that phenomena close to those that Hájek (cite) points to could be used to argue that the Molinist was committed to the non-supervenience of the conditional truths on non-conditional truths. The rough idea is that variants of theses 2 should hold in highly idealized settings where there were just not enough no-draw worlds that differed in their non-conditional character, to represent the probability functions of the kind required by thesis 1.

I have come to believe that this will not work, for it would be question-begging to represent the subjective or evidential probabilities of the Molinist via a space that contained only metaphysical possibilities: the Molinist should be allowed to be uncertain about whether a conditional is true or false (while maintaining there is a brute fact of the matter) even when they know exactly what the non-conditional facts are.

Brutalism There are a pair of arbitrarily similar cases c and c', which differ only in ways that are intuitively irrelevant to whether ϕ is true. Of that pair, it is determinately the case that ϕ is true in c and not in c'.[15]

It's pretty plausible that the Molinist will agree to this claim. But if an argument is needed, one is provided in the notes.[16]

One does not get to deny Thesis 2 just by being a Molinist, however. *In addition* to holding that there are brute matters of fact about whether S is true or false in the relevant cases, one must *also* deny it is indefinite. The epistemicist (Williamson, 1994) notoriously thinks there are brute facts of the matter about the truth-values of vague sentences of all kinds, and that indefiniteness is exactly *ignorance* about such matters. From the epistemicist perspective, the Molinist position is grist to the mill. Moreover, whereas the Molinist's brutalism about conditional sentences might seem an ad hoc response to the puzzle from which we begun, the epistemicist embeds it within a wider brutalism over semantic facts, which they argue is *independently* motivated by the phenomenon of the vagueness of natural language.

The Molinist might still resist the invitation to declare themselves an epistemicist—perhaps because they genuinely are committed to a range of *sui generis* conditional facts; or perhaps because they are less happy with brutalism over semantic facts, than they are with brutalism over conditional facts. But in the present dialectic, Molinism is introduced simply as a response to the puzzles outlined above, and not part of a rich theory which motivates regarding conditional facts as irreducible to non-conditional facts. So the vast majority of those tempted by Molinism, I think, will have every reason to see this as a special instance of the epistemicist's brutalism over semantic facts of all kinds.

Absent wheeling in an elaborate theory of the sui generis nature of conditionals, then, I contend that once one accepts thesis 1, one should not deny thesis 2. And once we have thesis 2, thesis 3 seems irresistible. Further, the argument we've just gone through can be repeated for each instance of Thesis 2+, at least so long as thesis 1+ holds *precisely*. (If it holds only

[15] The formulation here could do with some tightening up. For example: we shouldn't allow variation in *conditional* facts to be a relevant respect of dissimilarity: it is similarity as regards *non-conditional* facts that we are concerned with.

Notice that Brutalism is substantially stronger than anything we could get by vanilla vagueness. It is true that using classical logic alone and a vague predicate like 'tall', sorites reasoning gives us a pair of arbitrarily similar cases, where the first is tall and the second is not. But notice (i) for all we have said, the truth of this existential generalization is compatible with there being no fact of the matter about which pair satisfies this description (cf. supervaluationist treatments of vagueness); (ii) the pair in paradigmatic cases of vagueness do differ in respects which are intuitively relevant to the satisfaction of the predicate. In the case at hand, we have also that the differences are intuitively *irrelevant*; and this gives extra force to the claimed bruteness.

[16] To begin with, suppose that the Molinist thinks that the conditional has a determinate truth value in each of the non-red cases. (In principle one could adopt the mixed position which combines Molinism with the denial of this—but it seems pretty ad hoc to do so.)

As a recipe for finding such a pair witnessing BRUTALISM, take two non-red worlds C and C', such that the conditional is true at C, but false at C'. By IRRELEVANCE, the first element of the brutality condition is met. But they may differ markedly in what happens in other ways (perhaps I scratch my nose and jump around a bit in one, and stay perfectly still in the other). Now construct a series of non-red cases which differ only minutely from each other, whose first member is C and whose last member is C', such that any adjacent pair in the series differs only minutely in the underlying non-conditional facts (consider worlds where I jump around less and less, scratch my nose for briefer and briefer periods of time). Since every pair differs only in ways that are intuitively irrelevant to the conditional, and by construction the differences are minute, the only way they can fail to be a case of the kind needed is if they have the same truth-value. But if this goes for every pair in the series, it follows that the initial two cases we picked must have the same truth-value, contradicting our initial assumption.

Effectively, what we have done is to construct something very like a sorites series. It differs from a sorites series only in that the minor premises are not *obviously* true and false respectively. Rather, the premises are provided by the Molinist assumption.

approximately, then in certain cases the argument for an instance of thesis 2+ will lapse, though we'll still get enough cases to run versions of the arguments to follow. For simplicity, we'll set aside this possibility.)[17]

6 The theses are incompatible with supervaluationism

Given Theses 1+ to 3+, the upshot is that we must invest arbitrarily high probability in a sentence of the form:

> *p* and it is not definite that *p*

We can use this result to get argue against various philosophical accounts of indefiniteness.

Let us start with a popular theory of indefiniteness: *supervaluationism*. The key thought of supervaluational treatments of indefiniteness is that there is not one but a *cluster* of classical interpretations appropriate to a given natural language ('precisifications'). A sentence is true (supertrue) if is true according to each; and false (superfalse) if it is false according to each. Sentences which are true on some precisifications and false on others are neither true nor false: they are *indefinite*. Applied to the current setting, the claim will be that 'if a red card is drawn, it will be a diamond' will be neither true nor false in cases where the card drawn is black.

One can think of the orthodox supervaluationism as proposing that 'it is definite that *p*' can be glossed as 'it is true that *p*'. That means we should render:

> *p* and it is not definite that *p*

as

> *p* and it is not true that *p*

If the initial version sounded surprising, the supervaluationist gloss sounds as close to analytically false as one could get.

The intuitive repugnance of this result might lead us to wonder whether the substance of the supervaluationist proposal would be better glossed in other ways. It is helpful therefore to have a more principled argument, leaning less on glosses, and more on detailed aspects of the orthodox supervaluationist treatment of definiteness.

A key virtue claimed by supervaluationists is that they can accommodate truth-value gaps without sacrificing the most distinctive theses of classical logic. Every precisification of the

[17] The major dialectical changes occurs when there is a very high conditional probability of drawing diamonds when one draws reds (and mutatis mutandis for the case where the conditional probability is very small). For in those sorts of cases, the departures from the instance of thesis 1 required by making the conditional true in all the non-red cases, are correspondingly small. For example, suppose that the thesis 1 holds only approximately: we can only say that the probability of the conditional is within 0.05 of the conditional probability. Now consider a case where the conditional probability of diamonds on reds is already very high: say 0.95. Then, consistently with this approximate version of thesis 1+, one may say that all non-red cases are cases where the conditional is true (in violation of thesis 2+). No matter how dominant black draws are over red, at the limit, the probability of the conditional will be 1, which is within 0.05 of the conditional probability.

Nevertheless, approximate versions of thesis 1+ allow us to argue for many instances of Thesis 2+. One might think that this itself is inductive support for the general version. But even if not, suppose that the last instance of thesis 2+ that is established is where the probability of the conditional is below 0.95. Since we still have all the relevant instances of thesis 2 and 3+, we can make 'it is indefinite whether *p*' as high as we like. Overall, we still have an argument that an instance of this form is 0.94 probable. For our purposes, this will be sufficient. Playing with approximations, then, we can expect to weaken the conclusion we can obtain; but we still get striking results.

language is classical, and so on every precisification, classical tautologies will be true. Thus, classical tautologies are guaranteed to be supertrue, no matter what the range of precisifications of a langauge happen to be. Every classical tautology is a supervaluational tautology. But notoriously the result has limitations. The key result is that the orthodox supervaluationist regards instances of the following schema as logical contradictions:

$p \wedge \neg$ definitely, p

The standard characterization of logical consequence is as guaranteed truth preservation. For the orthodox supervaluationist, that gives us:

Global consequence
$A \models B$ iff Every model on which A is true is one where B is true

But it is widely accepted in the literature that 'p' and '$\neg$ definitely p' can never be both supertrue in the same model (for the latter to be supertrue, we need a precisification where p is false, and that will undermine the supertruth of p). And that is enough to ensure that each instance of '$p \wedge \neg$ definitely p' will be a supervaluational contradiction.[18]

Given this result, orthodox supervaluationism must be logically revisionary. For example, rules such as conditional proof, reductio, and argument by cases have to be given up.[19] It is normal in the literature to object at this point that such principles are entrenched in ordinary practice, and that giving them up is a major blow against supervaluationism. In my view, too much has been made of this thought. For every classical principle that has been given up, the supervaluationist can endorse a weakened variant that demonstrates the reliability of exactly the inferential moves the classicist thinks are valid—she only has to give up instances where the reasoning involved is supervaluationally but not classically valid. So even if orthodox supervaluationism is revisionary of logical *theory*, despite its reputation it has, I think, no revisionary implications for the *practice* of reasoning.[20]

But the observation that $p \wedge \neg Dp$ is a supervaluational contradiction does have direct impact in the current setting. For given our theses about conditionals, we are forced to assign arbitrarily high probability to instances of this scheme—even an approximate version of the basic theses 1-3 will force us to assign significant probability to something of this form. By the orthodox supervaluationist's lights, we must assign high probability to a *contradiction*. Not only does this sound bad, is flies directly in the face of a central theoretical link between probability and logic:

Probability-logic link If $A \models B$, then the probability of B should be no lower than the probability of A.

Such principles are central to spelling out one aspect of the epistemological significance of logic: it is the way in which logical relations provide constraint on what *coherent* subjective

[18]See Fine (1975), Williamson (1994, ch 5.). Williams (mingc) argues that the reasoning will be successful only if we presuppose that the logic of 'definiteness' is S5, and we treat 'definitely' as a logical constant.

There is a well-known way of defining a formal 'consequence relation' for the language that avoids this result: so called local consequence. For arguments against this being truly a consequence relation, see Williamson (op cit) and Williams (op cit).

[19]If we have these principles, then it will be straightforward to argue from the law of excluded middle to the conclusion that there is no indefiniteness. Suppose for reductio there is indefiniteness: $\neg Dp \wedge \neg D\neg p$. Then suppose p. A contradiction follows ($p\neg Dp$); so by reductio, $\neg p$. But a contradiction again follows ($\neg p \wedge \neg D\neg p$). So we have an overall reductio of our starting point.

[20]I discuss this case in Williams (op cit).

probability distributions can look like. If we do not have the probability-logic link, then one would suspect that one or other of the things one is calling 'logic' or 'probability' does not deserve the name.

A consequence of the probability-logic link is that logical contradictions should have probability 0, and tautologies should have probability 1. So even our very first result, following only from thesis 1-3, shows us that our thinking about conditionals is in directly in tension with orthodox supervaluationism, given the probability-logic link. (More generally, this suggests that the best way of attacking the logical revisionism of orthodox supervaluationism is *not* to try to argue that it undermines inferential practice—as explained earlier, this is easily resisted—but rather to draw out its consequences for the representation of partial belief via the probability-logic link.[21] The current argument is just one instance in which revisionism, so amplified, causes trouble.)

To summarize. The key thesis of the orthodox supervaluationist is the identification of truth with supertruth. Once we have made that identification, the consequence of theses 1+-3+ is that we must assign high probability to a conditional, while simultaneously assigning high probability to it not being true. That sounds unsustainable. But we don't have to rest content with the way it sounds if we are allowed to appeal to another of the characteristic features of the orthodox supervaluationist package—its logical revisionism—together with the central link between probability and logic. Our theses about conditionals are incompatible with orthodox supervaluationist treatments of indefiniteness.

7 Other options

The incompatibility between theses 1-3 and orthodox supervaluationist demonstrates the power of these theses in giving traction on the general debate over the treatment of indefiniteness. There remains the question of *how much* traction it can give us. In the end, this must be pursued on a case by case basis by each theory of indefiniteness.

Some other treatments of vagueness look equally unpromising. For example many treatments of indefiniteness incorporate a non-classical, many-valued logic.

There are complex interpretational issues that must be faced before we can evaluate what such a nonclassicist would and should say about our situation. Let us start, to begin with, with the sort of nonclassical view explored in recent work by Hartry Field. On Field's interpretation, our primary concern should be with the logic that governs vague sentences. The model theory (which can be presented in many-valued terms) features assignments of 1, 0.5 and 0 to sentences, governed by the 'strong Kleene' truth-functional tables. Field urges us *not* to interpret these as 'true' 'half-true' and 'false' respectively—model theory is essentially a tool for specifying a logic, not a system modelling the 'intended interpretation' of the language.

Logical truths, for Field, are those sentences which get semantic value 1 on every model. Inconsistencies are those that always take semantic value below 1. An argument is valid if it preserves value 1 on every model.

[21] It is easy to show that the probability-logic link is incompatible with supervaluationist probability behaving in a classical way. In particular, it looks hard for the supervaluationist to avoid assigning probability 1 to a disjunction, each of whose disjuncts are probability 0. There is a non-classical treatment of probability that has this feature: the theory of Dempster-Shafer functions (for an accessible introduction, see Halpern (1995). The main focus of the question of the tenability of orthodox supervaluationist logic, I suggest, should not be over its treatment of patterns of reasoning allegedly entrenched in practice such as conditional proof, reductio et al; but its implications, via the probability-logic link, for the structure of probabilities such as rational degrees of belief and evidence. It is here we find truly non-classical behaviour, and perhaps can get traction one way or other on the orthodox supervaluationist conception of indefiniteness.

Rather than trying to make sense of this in terms of truth-preservation (or something similar), one should look directly at the logic that is thus specified. An attractive result is that contradictions will be inconsistent, just as the classicist maintains. But some classical logical truths, such as the law of excluded middle, will be dropped. That corresponds to what seems fairly intuitive about paradigm cases of indeterminacy—if Harry is borderline bald, then 'either Harry is bald or Harry is not bald' seems like something we should reject. Field's approach lets us do so (it is no longer a tautology) and indeed encourages us to regard the rejection of excluded middle for A as characteristic of taking A to be indeterminate. (Here we can understand 'rejection' as 'assigning credence close to zero).[22]

What should this view say about our conditionals? Well, if we regard them as indeterminate, then on the Field view we should characteristically reject an instance of excluded middle for the conditional. But how could we rationally have higher credence in the conditional than in a sentence one of whose disjuncts is the conditional itself? More formally, assuming the probability-logic connection mentioned earlier, and since $A \models A \vee \neg A$, if we assign credence zero to excluded middle, we need to assign credence zero to A. The probability of the conditional, in the relevant sense, will be 0. Clearly, therefore, we cannot be both have high confidence in the indeterminacy of the conditional, and maintain a high level of confidence in conditional itself, as required by our earlier argument.[23]

So we have strong negative results: given our theses (and in particular, thesis 1) we can argue against two prominent theories of indefiniteness: standard supervaluationism, and the Field-style presentation of many-valued theories. The degree-theoretic interpretation of indeterminacy, at least as elaborated by Smith, looks more

There are three reactions to this.

1. **The impossibility result reaction**. First, one can regard the result as a new way of arguing against the (approximate) identification of conditional probabilities and probabilities of conditionals. Like some previous results (Hájek, 2004), the results we draw out require only individual instances of CCCP, not the fully general version. But unlike these previous results, we need assume nothing about rational belief *change* to get our result. Instead, the needed supplementary premises concern the nature of indefiniteness: a commitment to standard supervaluationism, or to many-valued treatments of indeterminacy, will force one to give up even the most paradigmatic instances of that identification. (Of course, such a reaction will face the burden of explaining the systematic judgements that apparently support thesis 1).

2. **The epistemicist reaction** A different kind of reaction would treat the above considerations as the basis for an inference-to-the-best-explanation argument for the Molinist-epistemicist package described earlier. The epistemicist identifies indefiniteness with (a particular kind of) ignorance about the truth-value of a sentence, while maintaining that there is always a determinate truth-value to be ignorant of. There are independent motivations to think that sometimes, you can be confident of something and confident that you don't know it. Few would take themselves to know that their lottery ticket is a loser;

[22] It is crucial for this that one does not identify *rejection* with *acceptance of negation*. The negation of the law of excluded middle will be equivalent to an explicit contradiction, and so need to reject it.

[23] One should distinguish the Field view absolutely from the 'degree theoretic' interpretation of the many-valued setting. Here, we do take the semantic values assigned by the model seriously—the idea being that a sentence assigned value d on the intended model should be regarded as 'true to degree d'. Rather than the two classical truth-values, truth and falsity, one gets intermediate *degrees* of truth: perhaps just one intermediate degree; perhaps infinitely many. I discuss these settings below.

but most would be highly confident that it will not win. The Molinist-epistemicist will contend that we have exactly such a case here.

By appealing to the plausibility of thesis 1 and 1+, we might argue that epistemicism is the only major player in town that can accommodate these results. Of course, epistemicism is the sort of theory that generates incredulous stares, and one cannot expect arguments of this kind to immediately win it converts by itself. But what is impressive is that epistemicism—a theory devised primarily for explaining the phenomenon of vague language and the sorites paradox—should also seems to be what is called for to deal with the non-soritical indefiniteness of conditionals. And this explanatory success may well increase our confidence in the epistemicist programme.

3. **The missing option reaction** I have not pretended to cover all possible analyses of indefiniteness (though we may fairly claim to have covered representatives 'big three': epistemicism, the most popular 'semantic indecision' theory (orthodox supervaluationism), and one reading of a many-valued treatments). Rather than rejecting the apparent data, or treating it as evidence for the radical semantic brutalism involved in epistemicism, one might take the moral to be that the correct treatment of indefiniteness is not to be found in the standard array of options.[24]

8 Conclusion

We have seen how, from certain widely discussed hypotheses about the probability of simple indicative conditionals, we can argue, first, that a range of conditionals are indeterminate; and second, that one of the standard accounts of what the vagueness of a conditional might consist is incompatible with the hypothesis. Of course, the interest of these results depends on how seriously one took the hypothesis in the first place. There are reasons to be wary of it—what it consists in is an appeal to certain instances of the notorious CCCP—-the identification of the probability of a conditional with the corresponding conditional probability. In an appendix, I briefly set out why I think the individual instances needed here are still worth exploring.

Depending on one's starting point, one might regard these results as another nail in the coffin of taking data about the probabilities of conditionals at face value (the outstanding challenge is then to explain *why* the apparent data arises); or, taking the probability judgements at face-value,

[24] One interesting option is a degree theory of vagueness, where the truth values are not exhausted by truth and falsity, but rather come in a variety of intermediate degrees. This can be developed in truth-function terms ('fuzzy logic'), or in non-truth-functional terms Kamp (1975); Lewis (1970); Edgington (1997).

We would have to supplement each with a story about what the appropriate attitude to have to indeterminate sentences are. It won't help very much in either case if our credences should be our credence of the sentences being perfectly true—but there may be alternatives. For one approach to this issue in the truth-functional setting, see Nick J. J. Smith (cite). Smith basic idea is to treat our degrees of belief in sentences as 'expected truth values'. (Cite also here Jeffrey/Stalnaker?)

The non-truth functional setting is similar in spirit to supervaluationism, but instead of identifying a set of precisifications for our language, we posit a measure over the classical interpretations of our language the captures the degree to which they are 'the intended interpretation'.

One natural suggestion in this setting is that the degree to which a sentence is determinate should act as an expert function for probabilities (so that if one is certain that S is true to degree k, one's credence or evidential probability of S should be k). If one combines this with a reading of 'indefinite' as it figures in thesis 1 as 'not degree-1 definite', then if one knows a conditional is determinate to a degree k less than 1, one should invest probability k in the conjunction of the conditional with the claim that the conditional is not perfectly determinate.

This is no more than a sketch of a possible view, and much more work would be needed to defend its details. As in the epistemicist case, it is striking that such a degree theory could be motivated by consideration entirely distinct from the sorites paradox.

one can take these results to constrain acceptable accounts of the indeterminacy of indicative conditionals—supporting, for example, an epistemicist treatment over its supervaluationist or many-valued rivals.

Appendix: The impossibility results

When outlining some reasons to be suspicious of Thesis 1 and 1+, we mentioned the impossibility results arising from Lewis (1976) and the subsequent literature. We here quickly sketch the outlines of this debate, locate Thesis 1+ within the dialectic, and sketch how the considerations of the current paper fit in.

I take it that the impossibility results that arising in the literature that follow Lewis's original paper are of two kinds: static and dynamic. The static kind argue that even within a single probability function, there are some conditionals whose probability is not equal to corresponding conditional probability. The dynamic kind show that (in precisely defined senses) the equation of probabilities of conditionals and conditional probabilities are unstable under various ways of updating ones credences. (Hájek and Hall, 2004, give an excellent survey.)

Most of the static results are not obviously relevant the evaluation of Thesis 1, since there aren't any static no-go results of this kind that ban instances of CCCP for the conditionals involved in Thesis 1. Indeed, there are tenability results (due to van Fraassen) that assure us Thesis 1 is at least consistent.[25]

One might think, however, that the static no-go results undermine the appeal of Thesis 1 and 1+ indirectly. If the equation of probabilities of conditionals with conditional probabilities can't be sustained in general, one might think it would be ad hoc to retain it in restricted cases.

This would be a powerful concern, I think, if the data about probability judgements concerning conditionals supported CCCP in general: for we'd then know that such intuitions have to be classed as unreliable indicators of the probability of conditionals, and we would *anyway* have to search for some way of explaining away the intuitions.

But the data about probability judgements does *not* support CCCP in general: indeed, it's hard to see a firm case for it for anything other than paradigmatic simple conditionals (for which we know the instances of CCCP are satisfiable by van Fraassen's result). This is one moral we can take from Vann McGee's putative 'counterexamples to modus ponens' (McGee, 1989). Suppose that you see a creature flapping around on the beach. The likelihood is that it's either a dolphin or a salmon. However the conditional 'if it's a fish, then if it has lungs it's a lungfish' seems extremely probable: exactly as probable as the simple conditional 'if it's a fish with lungs, it's a lungfish'. But the probability of 'if it has lungs it's a lungfish' conditionally on 'it being a fish' will be low. (The probability of the simple conditional is obviously extremely low, and it's conjunction with 'it's a fish' will be no higher. We obtain the conditional probability by dividing this the probability of this conjunction by the probability that it's a fish. If the latter is 1/2, the conditional probability is double the probability of the conjunction. But double something very tiny is still very tiny.) So it's just not true to say that the intuitive data supports full CCCP.

Given the standard static results don't undermine Thesis 1, directly or indirectly, attention turns to the dynamic results. I take it that the most powerful dynamic result for our purposes is that of Hájek (2004). He shows that any single instance of CCCP is unstable under a wide array of Bayesian belief update methods, updating on a wide array of propositions. That is, if one takes a probability distribution that vindicates an instance of CCCP, and updates on selected propositions by Bayesian methods (e.g. conditionalization, or Jeffrey conditionalization), one is left with a probability distribution that doesn't vindicate that instance of CCCP.

To use these considerations to undermine Thesis 1, we'd need to add premises about the stability of Thesis 1 on receipt of various pieces of information, and we'd have to buy into some Bayesian story about belief-updating. Furthermore, updating on *any* old propositions is not

[25] A different kind of static result is given by Hájek (cite). It shows that CCCP can't be satisfied in a finite probability space. This does have some impact in the current setting, raising delicate issues about the space over which evidential or subjective probabilities are defined. We don't have space to go into these issues here.

guaranteed to undermine an instance of CCCP—it needs to be appropriately chosen.

The second and third premises here, in particular, seem to me to be hostages to fortune in the argument. There really is a gap, it seems to me, between buying into the probabilistic representation of partial belief at any given time, and the full Bayesian or quasi-Bayesian package of the dynamics of partial belief. And even if we do have a updating-story of the required kind, it seems to me no part of the Bayesian picture that for some arbitrary bit of information A, (no matter how easy and natural to describe) agents can find themselves in situations where they should conditionalize on the information that A. But this sort of issue may well be crucial to the arguments under discussion here.

Suppose, for example, that A is a proposition conditionalization on which would disrupt an instance of CCCP. Now suppose one could systematically describe a proposition A^*, conditionalizing on which would give exactly the same results as conditionalizing on A for all non-conditional propositions; and such that conditionalizing on A would not disrupt the instance of CCCP. Then to turn the observation that conditionalizing on A would disrupt the instance of CCCP into an argument against our hypothesis, we'd need to support the claim that A, rather than A^*, is at least sometimes the thing we should conditionalize upon.

The upshot is that Thesis 1 and 1+ aren't knocked out the water by the standard impossibility results. That's one reason why it's still interesting to trace their implications. But what we have seen here is that even these highly restricted instances of CCCP are incompatible with certain popular theories of indefiniteness. And so we have a new static argument against these paradigmatic instances of CCCP—if we buy into those standard treatments of indefiniteness.

The current argument therefore does not require us to endorse the theories of belief updating needed to make the dynamic impossibility results damaging—in the terminology above, it is a static argument. True, it has its own philosophical hostages to fortune—several of them. But these are of a radically different kind, involving issues about the nature of semantics facts and of indeterminacy, on which we can hope to get independent traction. So, for one unconvinced or agnostic about some of the assumptions of Hájek's dynamic argument, the present considerations potentially give us a new angle on the (un)tenability of highly restricted instances of CCCP.

Even one convinced by Hájek's dynamic arguments against instances of CCCP should, I think, also be interested in this one—from their perspective, it will look like arguments from radically different sorts of premises point in the same direction—and that can considerably raise ones confidence in a conclusion even if in fact one already had an argument for the conclusion whose premises you endorsed.

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