A Reply to the Report of Reviewer #2 on *Formal Qualitative Probability*

May 26, 2019

When I first read the report from the reviewer designated as "Reviewer #2", I found no criticism of worth. None-the-less, I thought it important to make a more careful reading, to be sure that I did not overlook something that might help me to improve my paper. In order to compel myself to consider the whole review carefully, I wrote a response to each part of it.

Herein, I will quote the report in *italicized* blocks, in a way that doesn't fully preserve the reviewer's structure of itemization. A copy of the report as it was provide to me is available on-line.

> *This paper aims to develop a new axiomatic theory of qualitative probability and to illustrate its significance for various applications—including representing uncertainty, updating doxastic states, and making statistical inference. Qualitative probability judgments, unlike quantitative probability judgments, merely involve making probabilistic comparisons among events (or propositions, etc.)—for example, merely judging that one event is more probable than another event. Although qualitative probability was widely studied among notable figures of the early 20th-century history of probability, most of the philosophy of probability literature of the last several decades has focused on quantitative probability. The author of the present paper aims to study qualitative probability on its own terms and, very broadly, to demonstrate that it can illuminate various issues that quantitative probability cannot.*
>
> *The project of the present paper is a worthwhile one, and the author seems to have some original ideas to contribute to it. However, many of the author's sweeping claims are not justified by what the author says,*

Further remarks by the reviewer that attempt to support this contention are addressed as they appear. He or she was everywhere mistaken.

> *and overall the paper strikes me as too large in scope.*

Attempting to present this work as a collection of $n+1$ papers would be like attempting to reach the top of a mountain by throwing one's individual organs towards the summit, in the hope of reassembling them once they were all there. Chances of survival would be minimal.

> *Additionally, many parts of the paper are very unclear or involve idiosyncratic language or terminology that make the paper unacceptably difficult to read.*

There is absolutely *no* idiosyncratic language in the paper. *Every* term is used either as it is defined in everyday English or following the existing literature. However, as will be seen, the reviewer often assumes, for unstated reasons, that I am using a term in a peculiar and undefined manner, and consequently declares a passage to be unintelligible. There *is* an idiosyncratic formal *notation* for the binary relations of probability, for the very good reason that the more common notation uses symbols that have a different meaning in the theory of *preference*, and this paper is part of a larger project that involves that theory.

> *More detailed comments follow.*
>
> *1. The paper establishes far less than what it purports to establish. From the abstract and introduction, one would have thought that the paper would contain at least three things: (a) a detailed argument that qualitative probability is a more general concept than quantitative probability,*

The point that qualitative probability is a more general concept is a piece of trivial mathematics. Complete preörderings are a proper subset of preörderings more generally, so a theory that does not require that the preördering be complete (nor require that it be incomplete) is more general. It is not my responsibility in the paper to teach the concept of *generalization*, nor the basic mathematics of ordering. Indeed, it would be an abuse of many readers to include such discussion.

> *(b) a development and motivation of a new theory of qualitative probability,*

To *some* extent, yes.

> *(c) a systematic development of applications of this new theory of qualitative probability.*

In the case of more familiar theories of probability, systematic development of applications was something that took place over many decades. The expectation that advocates of qualitative probability should refrain from publication until they have such applications is absurd.

> *In fact, the paper only contains (b).*

To *some* extent, yes.

> *As for (a), the author does cite some historical authors (e.g., Keynes and Koopman on p. 2) who have thought that qualitative probability is more general than quantitative probability—i.e., that there are some cases in which probability does not lend itself to numerical measurement. However, the author then goes on to assume that this is indeed the case throughout the rest of the paper without acknowledging that it is a controversial thesis and with- out engaging with the relevant literature in philosophy of probability. (A few papers relevant to the so-called 'comparativism'debate in philosophy of probability include Stefánsson's "What Is 'Real' in Probabilism?", 2017, Australasian Journal of Philosophy; Meacham & Weisberg's "Representation Theorems and the Foundations of Decision Theory", 2011, Australasian Journal of Philosophy; and Eriksson and Hájek's "What Are Degrees of Belief?", 2007, Studia Logica.) Instead of claiming (without argument) that qualitative probability is more general than quantitative probability and then claiming to develop a more general conception of probability, it would have been more appropriate for the author merely to claim to develop a conception of qualitative probability and argue that such a conception is worth developing on independent grounds.* \

The reviewer confuses the claim that qualitative probability is more general with a claim that the greater generality has real-world application. I would be saddened to learn that many purported experts in the philosophy of probability make this same confusion, but it doesn't strike me as worth investigating the possibility of such confusion, as it wouldn't bear upon my research. It is a trivial point of mathematics that complete preörderings are just a special case of preörderings; there is no possibility that qualitative probability is *not* more general. I made it explicit that my axiomata would hold across multiple interpretations, and that some of these interpretations force the preördering to be complete.

The question of whether the theory allows greater real-world application is important, but it was not part of the stated or implied purpose of the paper to explore that question carefully.

> *As for (c), the discussion of applications of the author's own theory of qualitative probability is very underdeveloped. For example, the author states an alleged generalization of Bayes' theorem— namely, (14) on p. 18—but doesn't explain why this is indeed such a generalization (or, for that matter, merely why it is a qualitative analogue of Bayes' theorem). There's just a lot of symbol manipulation in this section without much conceptual discussion. (Indeed, a general problem with the paper is that it often has too much symbol manipulation and not enough conceptual discussion.)*

The reviewer mistakenly believes that the *conceptual* as such requires natural language; it doesn't. Symbols, natural or formal, may be defined or undefined.

Every symbol that I used, natural or formal, had a prior standard definition within the relevant literature or was explained with natural language, or was given a definition in terms of formal symbols that had themselves been defined.

Now, let's consider whether (14), which will be presented below, is a generalization of Bayes' Theorem; it is purely a matter of mathematics. Bayes' Theorem is a *formal* relation.

$$prob\,(X_1 \,|\, X_2\,) = prob\,(X_2 \,|\, X_1\,) \cdot \frac{prob\,(X_1)}{prob\,(X_2)} \ .$$

No one who is conversant in the theory of probability fails to get the *conceptual* content of this formula, though there is no natural language here. One may state it thus

$$prob\,(X_1 \,|\, X_2 \wedge c\,) = prob\,(X_2 \,|\, X_1 \wedge c\,) \cdot \frac{prob\,(X_1 \,|\, c\,)}{prob\,(X_2 \,|\, c\,)} \ ,$$

to offer some prior context c. (In the paper, I provided some discussion of that issue.) Now,

- if $prob\,(X_1 \,|\, X_2 \wedge c\,) > prob\,(X_2 \,|\, X_1 \wedge c\,)$ then $prob\,(X_1 \,|\, c\,) > prob\,(X_2 \,|\, c\,)$, and *vice versa*;
- if $prob\,(X_1 \,|\, X_2 \wedge c\,) = prob\,(X_2 \,|\, X_1 \wedge c\,)$ then $prob\,(X_1 \,|\, c\,) = prob\,(X_2 \,|\, c\,)$, and *vice versa*; and
- if $prob\,(X_1 \,|\, X_2 \wedge c\,) < prob\,(X_2 \,|\, X_1 \wedge c\,)$ then $prob\,(X_1 \,|\, c\,) < prob\,(X_2 \,|\, c\,)$, and *vice versa*.

That's just high-school algebra. Using just introductory-level symbolic logic, we can express those if-then statements with

$$\left[\begin{array}{c} \overline{prob\,(X_1 \,|\, X_2 \wedge c\,) \geq prob\,(X_2 \,|\, X_1 \wedge c\,)} \\ \vee \\ \overline{prob\,(X_1 \,|\, c\,) \geq prob\,(X_2 \,|\, c\,)} \end{array}\right] \forall\,(X_1, X_2, c) \ . \qquad \text{(A)}$$

If the theorem has somehow instead been

$$prob\,(X_1 \,|\, X_2 \wedge c\,) - prob\,(X_2 \,|\, X_1 \wedge c\,) = prob\,(X_1 \,|\, c\,) - prob\,(X_2 \,|\, c\,) \ ,$$

it would still be true that if $prob\,(X_1 \,|\, X_2 \wedge c\,) > prob\,(X_2 \,|\, X_1 \wedge c\,)$ then $prob\,(X_1 \,|\, c\,) > prob\,(X_2 \,|\, c\,)$, and so forth. The formula (A), being implied in either case and not restricted to either case is a *generalization* of either case.

These relations are qualitative, albeit with an underlying quantitative relation. The quantative claim is a *restricted* case of the qualitative claim. If we remove that restriction (thereby *generalizing*), then (using the notation of the paper)

- if $(X_1 \,|\, X_2 \wedge c\,) \rhd (X_2 \,|\, X_1 \wedge c\,)$ then $(X_1 \,|\, c\,) \rhd (X_2 \,|\, c\,)$, and *vice versa*;

- if $(X_1 \,|\, X_2 \wedge c) \boxdot (X_2 \,|\, X_1 \wedge c)$ then $(X_1 \,|\, c) \boxdot (X_2 \,|\, c)$, and *vice versa*; and
- if $(X_1 \,|\, X_2 \wedge c) \lhd (X_2 \,|\, X_1 \wedge c)$ then $(X_1 \,|\, c) \lhd (X_2 \,|\, c)$, and *vice versa*.

No more or less than these three relations are captured by a formula found in my paper:

$$\left[\begin{array}{c} \overline{(X_1 \,|\, X_2 \wedge c) \trianglerighteq (X_2 \,|\, X_1 \wedge c)} \\ \vee \\ \overline{(X_2 \,|\, c) \trianglerighteq (X_1 \,|\, c)} \end{array}\right] \forall\, (X_1, X_2, c) \ .$$

This formula is a thus generalization of Bayes' theorem. (Anyone with a degree in philosophy should be able to comprehend that expression, which uses nothing but standard symbols and those defined in the paper; and one could not have comprehended the previous expression in the paper if one could not understand this expression.) That formula, in turn, is (as noted in the paper) a special case (under the restriction that $c_2 = c = c_1$ and $Y_1 = X_2$ and $Y_2 = X_1$) of

$$\left[\begin{array}{c} \left(\begin{array}{c} \overline{(X_2 \,|\, X_1 \wedge c_1) \trianglerighteq (Y_2 \,|\, Y_1 \wedge c_2)} \\ \vee \\ \overline{(X_1 \,|\, c_1) \trianglerighteq (Y_1 \,|\, c_2)} \\ \vee \\ [(X_1 \wedge X_2 \,|\, c_1) \trianglerighteq (Y_1 \wedge Y_2 \,|\, c_2)] \end{array}\right) \\ \wedge \\ \left(\begin{array}{c} [(X_2 \,|\, X_1 \wedge c_1) \rhd (Y_2 \,|\, Y_1 \wedge c_2)] \\ \vee \\ [(X_1 \,|\, c_1) \rhd (Y_1 \,|\, c_2)] \\ \vee \\ \overline{(X_1 \wedge X_2 \,|\, c_1) \rhd (Y_1 \wedge Y_2 \,|\, c_2)} \end{array}\right) \end{array}\right] \forall\, (X_1, X_2, Y_1, Y_2, c_1, c_2) \ , \qquad (14)$$

So there should be no question that formula (14) is a generalization of Bayes' theorem. Perhaps the reviewer didn't know high-school algebra or cannot handle basic symbolic logic; but, given the confusion that he or she manifested concerning the greater generality of qualitative probability, it seems most likely that the reviewer simply did not understand the concept of *generality*. In any event, my paper should not be expected to explain high-school algebra, the basics of formal logic, nor the nature of *generality*.

> *Additionally, in the next section, the author has some discussion of updating qualitative probability, but the author never explicitly states a general update rule for qualitative probability (which one would have expected from a section with the word 'Updating' in its title). The author merely states a special case of updating—namely, (15) on p. 19—but does not explain the conceptual significance of this case.*

No. The familiar formula for updating is a special case of the familiar form of Bayes' theorem, and the general formula for updating is a special case of the generalization of Bayes' theorem; in each case, the rule of updating is a special case of the theorem, which does not make the rule of updating a special case of itself. The reviewer did not follow the discussion of Bayes' theorem and updating leading to (15), and therefore did not recognize (15) for what it were.

> *Moreover, the author defends this update principle by saying (on p. 20) that it merely follows from the axioms of the author's theory. However, this defense only makes sense if the author is implicitly assuming that the axioms of their theory are diachronic (rather than synchronic) constraints of probability. Nonetheless, as far as I can tell, the author never argues for this substantial assumption anywhere in the paper.*

There was no good reason for me to *restrict* the propositions so that they were not diachronic, and no good reason to note that I'd not made such a restriction. Even without a concern for updating, there would be little use for a purely synchronic theory of probability, as not only collection of evidence but thought itself are processes that take place *through time*. The results, for updating, of imposition of synchrony on the assumptions are not interesting. Had the reviewer attended to what I wrote in the first paragraph of the section on updating

> The point that unconditional probabilities correspond to cases in which conditions are taken to be known with perfect certainty comes to bear if we consider the usual process of Bayesian updating in which, upon the introduction of new information I_n, $prob\,(X \mid I_n\,)$ is computed and then *assumes the rôle* and names of $prob\,(X)$; I_n disappears into the subsequently unacknowledged background. If the presence of previously new information were not suppressed, then instead of something such as "$prob\,(X)$", one would write something such as "$prob\,(X \mid \bigwedge_{i=1}^{n} I_i\,)$" (or such as "$prob\,(X \mid \bigcap_{i=1}^{n} I_i\,)$").

then he or she would have realized that concerns over diachronic or synchronic interpretation are an artefact (within standard convention) of probabilities corresponding to new information "*assum[ing] the rôle* and names of" probability without that information. There are two probabilities, rather than one evolving probability; *the two underlying probabilities are themselves timeless*. What changes with time is not the probabilities but which of the probabilities we wish to use — what part of the relational graph we occupy — because what changes with time is what information we hold.

> *Another note to make about (c) is that the author's own theory of qualitative probability does not seem necessary to develop the applications of qualitative probability that the author wants to develop. In particular, as far as I can tell, one could have begun the development of these applications in a similar manner to how the author has begun to do so merely using Koopman's theory of qualitative probability (which the author criticizes), as Koopman's theory has a similar form to the author's theory and the author does not seem to*

> *have appealed to any unique features of their theory in discussing the applications.*

Elsewhere, the reviewer asserts that the value of my paper is in its critique of Koopman, and objects that I did not present it as such; here, the reviewer writes as if it were inappropriate for me to criticize Koopman's work given that I don't show distinctive application for mine. However, it is in the reviewer's mind and not in mine that the focus of the paper was or should be in its criticism of Koopman. My discussion of applications is not part of a campaign against his work, nor do I need to surrender to that work if I will not seek every possible opportunity to attack it. To the extent that I criticize Koopman, it is primarily based upon the point that his system can be developed from a simpler system. (I also offer some propositions for which he has no corresponding propositions, but his purpose was different from mine, as I noted in my paper.)

> *If this is indeed the case, then it would seem more appropriate to develop these applications in a more general context of qualitative probability, without presupposing the author's own theory. Coupled with the fact that the author's discussion of applications is very underdeveloped, the section on applications strikes me as inappropriate for the present paper. Instead, it could be developed into an entire paper on applications of qualitative probability.*
>
> • *The chief contribution of the present paper appears to be (b)—namely, the development and motivation of a new theory of qualitative probability. However, much of this part of the paper is very unclear or insufficiently explained.*

An examination of the report will show that the reviewer has not made a case for a lack of clarity or sufficiency in my explanation.

> *My next comments concern this point.*

Hence I will reiterated and expand on some of what I've said.

> *2. The bulk of the paper (pp. 4–14) is the author's development and motivation of a new theory of qualitative probability—*

It would be more accurate to say that the *kernel* of the paper is that development and motivation.[1] For good or for ill, the reviewer offers almost no comment on anything beyond that kernel.

> *specifically, a new theory of qualitative conditional probability.*

As I noted in the paper, unconditional probabilities are equivalent to conditional probabilities whose conditions are perfectly certain; hence, the use of the term "conditional" is somewhat superfluous.

[1] The pagination used by the reviewer corresponds to the formating requested by the journal, which is different from that of the working copy of the paper.

> *Qualitative conditional probability is generally taken to be the quaternary relation of an event A, given event B, being more probable than (or at least as probable as) an event C, given event D. Theories of qualitative conditional probability have been developed by several authors, but the one most relevant to the author's purposes is Koopman's theory. Koopman's theory is notable because, unlike most other such theories, it doesn't require complete comparability. That is, given arbitrary events A, B, C, D, it doesn't require the following: A, given B, is more probable than (or at least as probable as) C, given D.*

That much is quite true.

> *The author's own theory of qualitative conditional probability is largely developed in response to shortcomings that the author believes Koopman's theory in particular to possess (though the author doesn't emphasize this point upfront).*

I don't emphasize that point because, while I ultimately profitted greatly from poring over Koopman's work, I didn't begin by pondering that work; in some cases, I had arrived at similar propositions independently. When I began comparing what I'd done with Koopman's work, I sometimes found him to have arrived at better propositions, and sometimes at worse propositions.

> That said, much of the author's motivation and development of the theory are very unclear or insufficiently explained. Some examples:
>
> *The author axiomatizes two primitive relations of qualitative conditional probability—'strict supraprobability' and 'equiprobability' (to use the author's terminology). The former is the quaternary relation of A, given B, being strictly more probable than C, given D. Equiprobability is the quaternary relation of A, given B, being exactly as probable as C, given D.*

The terminology is not my *invention*; it is found in the prior literature. And neither *strict supraprobability* nor *equiprobability* is a *quaternary* relation. There are expressions in this paper of forms such as "$(X_1 \,|\, c_1)\, R\, (X_2 \,|\, c_2 \vee c_3)$" which is a probability claim about as many as *five* propositions, yet that doesn't make R a *quinary* relation. The implied quinary relationship would not be R, but $(\_ \,|\, \_)\, R\, (\_ \,|\, \_ \vee \_)$. The primitive relations are *binary*, notwithstanding that the *relata* in the paper of these relations are dyads, just as indeed those dyads are *dyads*, though one or both of their elements may be compounds. In calling the primitive relations "quaternary", the reviewer executes a graceless attempt at an intellectual *pirouette*.

> *The author's approach contrasts with the approach of many theories (e.g., Koopman's) that just axiomatize 'weak supraprobability'—namely, the quaternary relation of A, given B, being at least as probable as C, given D. (Such theories generally define strict supraprobability and equiprobability in terms of weak supraprobability.) The*

> *reason the author provides for doing so is that the latter approach "allows a mathematical elegance but has fostered some confusions of interpretation" (p. 4). However, the author does not explain what these confusions are. So, the reader is left wondering why the author has gone through all of the extra complications of axiomatizing two primitive relations instead of one.*

I identified those confusions in the discussion of mistaking *weak supraprobability* for a positive state of belief. As will be noted below, the reviewer is so trapped in that confusion that he or she did not even comprehend the confusion when it was identified *in plain English*.

> *It is unclear what the author takes the relata of the qualitative probability relations to be. For example, the author says "[a]ny event corresponds to a proposition that the event has occurred, and any proposition corresponds to the event that the proposition is true" (p. 5) and then seems to allow that the relata of qualitative probability may be either events or propositions. However, it is unclear what difference the author has in mind for events and propositions, since 'event' is often used in the probability literature merely as a placeholder for whatever the relata of probability are supposed to be (e.g., propositions, sets of outcomes, sentences, or something else).*

The fact that, in the literature, the word "event" is sometimes used *loosely* to include propositions doesn't license the reviewer to treat me as being unclear or incoherent when I do not use that word loosely. In the very next sentence, I stated 'Hereïn, I will treat the X and c of $(X \mid c)$ as themselves propositions, and an expression such as "$(X_1 \vee X_2 \mid c_1 \wedge c_2)$" as meaningful.' After a parenthetical remark, I stated 'Were these instead events, the equivalent expression would simply be "$(X_1 \cup X_2 \mid c_1 \cap c_2)$". Likewise, an expression such as "$c \Rightarrow X$", representing an eventuality as logically implied by a context, would have an equivalent "$c \subseteq X$".'

> *Much of the author's idiosyncratic language makes the author's discussion very difficult to follow. For example, the author argues that "[the relations of strict supraprobability and equiprobability] cannot each describe a positive state of belief" (p. 6).*

The reviewer misquotes me. What I wrote was "The formal relations above may be interpretted to fit the various interpretations of fundamental probability mentioned in the introduction, but it is noteworthy that these relations cannot each describe a positive state of belief". *Six* formal relations had been presented at that point, *four* of them were explicitly discussed in a footnote attached to that sentence, and the remaining two were found in the mathematical expression that followed in the main text. Yet the reviewer interprets "The formal relations above" to be equivalent to just "the relations of strict supraprobability and equiprobability".

> *But what does the author mean by 'positive state of belief' ? Just a belief that something is the case? The attitude of being more confident than not that something is the case? The author does not clarify.*

There's no idiosyncratic language here. I made the *clarifying* point "*states of belief* may entail what is believed, what is rejected, and what is neither believed nor rejected." For example, concerning Santa Claus, one may believe in him, or reject his existence, or have no opinion; each state is, in some general sense, a state of *belief*, but rejection is obviously *negative*, rather than positive, and agnosticism is a state of belief in which there is neither positive belief nor rejection.

> *(Additionally, I could not follow why the author is making such a point in the first place.)*

The reviewer could not follow because he or she had not recognized that those subjectivist who treat *weak supraprobability* as if it is a positive state of belief do so in error, and this confusion is fostered by treating *weak supraprobability* as primitive within a subjectivist framework.

> *However, even if the author did clarify what was meant, the author's argument for this claim is too symbol-heavy; there are no clear conceptual explanations of what is going on.*

In order to complain that there is too much formal mathematics, the reviewer is misrepresenting the use of formalisms, as opposed to natural language, as a lack of *clarity*; there would be no more conceptual content attached to my writing "*weak supraprobability*" as opposed to "$\trianglerighteq$" or "and" as opposed to "$\wedge$". In fact, natural language would result in expressions that were either far more complex or far more ambiguous.

In reading the review, it becomes rather evident that the reviewer ignored the *formulæ* as much as he or she hoped would be practicable when trying to understand a paper on *formal* qualitative probability.[2] Were one to agree that the paper should somehow have avoided formulæ significantly more than it did, the reviewer would remain *grossly derelict* in not attending to those formulæ. Some reader might perhaps be offended by an author's frequent use of *Greek*, but it would be unethical for the reader to pretend that he'd understood something when he'd ignored what that author had written, or that the author had not written something because she'd written it in a language with which the reader were uncomfortable. Nor should the response to mathematic formalism of an

[2]Years ago, I heard an economics professor describe the evolution of how graduate students in that major field would attempt to read papers. The early students would attempt to understand papers by reading the prose but ignoring the formulæ as much as possible. But, as students developed, they increasingly paid attention to the formulæuntil, in the end, they tried to understand papers by reading the formulæ and paying as little attention to the prose as they could. While I don't advise ignoring my prose, doing so would be less violent than ignoring the formulæ.

alleged philosopher of science be a dismissive "*Græcum est; non legitur.*" Such formalism is very much part of the language of science.

> *Slightly later on this page, the author uses the phrase 'self-alienated' in a way that I could not understand at all.*

Here's what I wrote "In the particular case of subjectivism, it seems self-alienated to *conclude* merely that $(X \mid c) \trianglerighteq (Y \mid d)$." A subjectivist believes that a probability is no more nor less than a state of belief. Here, a hypothetical subjectivist has concluded no more than that he or she believes that $(X \mid c)$ is more probable than $(Y \mid d)$ or he or she believes that they have the same probability; that's *not* like saying "I'm sure that either the Christians or the Atheists are correct, though I don't know which"; instead, it's like saying "I know that I'm a Christian or that I'm an Atheist, though I don't know which I am." It would represent a strange *alienation from oneself.* Remarkably, many subjectivists have failed to recognize this problem, and they've done so because in their choice of formal structure they've traded an acknowledgement of the nature of *weak supraprobability* as a *union* for the mathematical elegance of using it as formally undefined. Resisting *analysis* of *weak supraprobability* as the union of two relations, the reviewer then couldn't make sense of the point nor of the argument leading to it.

> *Also later on this page, the author refers to a '*pure *frequentism' and a '*pure *combinatoric interpretation of probability' (author's own italics). However, the author never explains what these terms mean in the present context. These are just a few examples on p. 6. The paper is filled with such quasi-technical terms that are never defined.*

The reviewer chose to read "pure" as if it were used in some peculiar sense when it was not, and chose to ignore the parenthetical remark "(An *impure* frequentism would be one in which $\trianglerighteq$ and $\trianglelefteq$ were always about beliefs about frequency; similarly for an impure combinatoric interpretation.)".

> *(A particularly confusing unexplained term was 'Ockham's Razor' on pp. 10–11, which did not seem to mean what it usually means.)*

What was and is formula (7) says that, given any context, as complications are conjoined to the description of a potential outcome, that outcome becomes less probable. I refer to that as "a variation on Ockham's Razor", which it rather plainly is.

> *Coupled with the fact that the author does define some technical terms, it is often extremely difficult to know what exactly the author is saying or arguing.*

I defined the terms that I used in unusual ways (though, even then, my use was not *idiosyncratic*); I did not define the terms that I used in ordinary ways, but the reviewer chose not to read some of those terms as used in ordinary ways.

> *The reader should not have to work so hard to understand the paper.*

This reviewer created a lot of work for him- or herself. So perhaps he or she shouldn't indeed have to work so hard, but I'm inclined to believe that the reviewer would not have worked hard enough even had he or she spared him- or herself some of this ill-spent effort.

> *Much of the author's idiosyncratic notation makes the paper difficult to follow as well. For example, the strictly more probable than and at least as probable as relations are usually denoted by '$\succ$' and '$\succeq$', respectively, in the literature. By contrast, the author uses the symbols '$\lhd$' and '$\unlhd$', respectively. Additionally, the exactly as probable as relation is usually denoted by '$\approx$' (or '$\sim$') in the literature. By contrast, the author uses the symbol '$\boxdot$'. The paper would be considerably easier to read if the author adopted notation and terminology that are more standard in the contem- porary literature on qualitative probability (and the contemporary philosophy of probability literature more generally).*

First, I use "$\rhd$" and "$\unrhd$" respectively (not "$\lhd$" and "$\unlhd$", respectively) where most of the literature uses "$\succ$" and "$\succeq$" respective. Second and more importantly, the symbols "$\succ$", "$\succeq$", "$\sim$", and "$\approx$" are *also* the most commonly used when in referring to the relations of *strict preferability*, *weak preferability*, and *indifference*. My probability paper is the second of a research programmer, the first paper of which was about preferences, and the third of which will be about the interrelationship of preferences and probability. However difficult the reviewer may imagine it to be to accept "$\rhd$" and "$\boxdot$" for probability relations, it would *miserable* to have to read "$\succ$" one way in the first paper, another in the second, and then to accept some further change in notation in the third. And the reviewer should not have been so presumptious as to imagine that I were using a distinctive notation without sufficient cause.

> *On the section 'On Consistency and Completeness of the Axiomata' (pp. 14-15). The author argues that their theory is at least as consistent as Kolmogorov's theory of probability because the axioms of the author's theory 'conform to the Kolmogorov axiomata'.*

Again the reviewer misrepresents what I wrote. I said

> As noted in the Introduction, all the axiomata here, with the possible exception of (A6) are themselves axiomata or theoremata in the systems of the major popular interpretations, and (A6) obtains under at least some of those systems. The reader can trivially verify that, while what I offer as axiomata are not sufficient to imply the Kolmogorov axiomata, his axiomata imply (A1) through (A5) and (A7) through (A13), and that (A6) *conforms* to the Kolmogorov axiomata.

A consistent system produces no contradictions amongst its theoremata, so (A1) through (A5) and (A7) through (A13), are as consistent as the Kolmogorov axiomata because they are implied by those axiomata. My claim about conformance was exactly and only about the remaining axiom (A6).

> *However, the author does not explain what 'conform' means here.*

Again, I didn't use the word in some peculiar sense. Axiom (A6) says two things. First, that if a context c implies X, then X given c is maximally probable; second, that if X given c is maximally probable, then c implies X. That axiom isn't an implication of the Kolmogorov axiomata, but it doesn't contradict any of them; it merely conforms, but it conforms.

> *I assume the author has something like the notion of 'representation' (familiar from the measurement theory literature) in mind. However, there are a few such concepts (e.g., 'almost representation' vs 'full representation') that are important to distinguish, and the author does not clarify which concept is meant.More importantly, however, this representation-theoretic point is not directly related to the consistency of the author's theory. To demonstrate that the author's theory is consistent, a model of that theory needs to be exhibited. The author has provided no such model. Thus, at best, the author's argument here only establishes that, if the author's theory is consistent, then any model of the author's theory 'conforms' (in some way) to some model of Kolmogorov's theory. (Another point should be noted here. When one encounters the phrase 'consistency and completeness' in the context of the logical properties of a given theory, one generally expects to hear about the consistency and semantic completeness of the theory. However, by 'completeness' here, one eventually re- alizes that the author only means that the author's theory is more general than Koopman's theory. This is still another example of confusing, non-standard terminology used by the author.)*

There's just a lot of useless work here — for the reviewer in writing it and for anyone reading it — because the reviewer did not attend to what I *actually* said about the axiomata other than (A6), and then, once again, chose to take me as using a word in some peculiar, unprovided sense, when I'd written ordinary English. The reviewer objects elsewhere to my use of formal notation, and yet we see that ordinary English repeatedly baffled this particular reviewer; I question whether there are any means by which the ideas of this paper could be communicated to him or to her.

> *3. The author seems to have some original criticisms of Koopman's theory— and some ideas about how to improve Koopman's theory—*

Well, that much is true.

> *but it is difficult to understand and appreciate these ideas when the paper as a whole has the aforementioned problems.*

Rather, it's difficult to understand and appreciate these ideas when one engages in careless readings and sloppy imputations.

> *Overall, the paper needs to be more focused, more tightly argued, and much more clearly written. My recommendation to the author would be to make this paper much more limited in scope—namely, as a paper that carefully develops the author's own theory of qualitative conditional probability and argues for this theory over alternative such theories (including Koopman's and others). Such a paper would have independent interest and, if written in a philosophically rigorous way, could potentially be publishable.*

The pair of reviewers at the previous journal thought that the paper contained little or nothing of interest as it stood; this reviewer and the other at this latest journal want to see a paper that makes a more modest contribution. Neither pair paid the attention to the paper that it was owed.