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, and overall the paper strikes me as too large in scope. Additionally, many parts of the paper are very unclear or involve idiosyncratic language or terminology that make the paper unacceptably difficult to read. 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, (b) a development and motivation of a new theory of qualitative probability, (c) a systematic development of applications of this new theory of qualitative probability. In fact, the paper only contains (b).

    - 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 without 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.

- 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.) 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. 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.
- 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. 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. My next comments concern this point.

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

   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 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.
- 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).
- 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). 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. (Additionally, I could not follow why the author is making

such a point in the first place.) 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. Slightly later on this page, the author uses the phrase 'self-alienated' in a way that I could not understand at all. 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. (A particularly confusing unexplained term was 'Ockham's Razor' on pp. 10–11, which did not seem to mean what it usually means.) 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. The reader should not have to work so hard to understand the paper.

- 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 '$\trianglelefteq$', 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 contemporary literature on qualitative probability (and the contemporary philosophy of probability literature more generally).
- 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'. However, the author does not explain what 'conform' means here. 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 realizes 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.)

3. The author seems to have some original criticisms of Koopman's theory—and some ideas about how to improve Koopman's theory—but it is difficult to understand and appreciate these ideas when the paper as a whole has the aforementioned problems. 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.