

Comment on Article by Robert

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First of all, I would like to express my gratitude to Professor Robert for initiating a discussion of my book in *Bayesian Analysis*. I say this without slightest amount of hypocrisy. To have one's book discussed in a professional journal is an honor and a privilege. Of course, I would be a hypocrite if I did not express my dismay with the overall tone of Robert's review and most of his specific opinions.

I will not try to summarize the main claims of my book. Larry Wasserman's review contains a crystal clear account of my theory—I could not provide a better summary myself. Incidentally, this provides an answer to one of Robert's claims:

The statement that “the above philosophical claims are incomprehensible to all statisticians” (p.9) could extend to *The Search for Certainty* as well.

I was amazed by how well Larry Wasserman understood my book.

After reading the first version of Robert's and Gelman's reviews I sent them a letter that Robert kindly posted on his blog [Robert \(2010a\)](#). I said, among other things:

On page vii of [my book], I clearly state my three intellectual goals: (i) Criticism of von Mises and de Finetti, (ii) Presentation of my scientific theory of probability, and (iii) Education of scientists about philosophical theories of probability.

Robert replied in [Robert \(2010b\)](#):

So, just as clearly, I am not interested in (i) (philosophical) criticisms of von Mises and de Finetti philosophical bases, in (ii) a new scientific theory of probability, or in (iii) educating scientists about philosophical theories of probability.

Would you trust a review of a Chinese cookbook if the reviewer declared publicly that he was not interested in Chinese cooking?

I am not afraid that my scientific laws of probability (L1)-(L5) will be rejected. They are embedded like a rock in every undergraduate probability textbook. Authors of such textbooks briefly discuss frequency and subjective interpretations of probability in the first chapter. These are quickly forgotten. The rest of any textbook indoctrinates students that probabilities are objective scientific quantities, like mass and temperature. There is no hint of a suggestion that probabilities should be assigned to events only if the

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corresponding experiments or observations are repeated or repeatable. Robert suggests in Sect. 2 of his review that my laws (L1)-(L5) belong to the nineteenth century. Prose is a very old concept. Nevertheless Robert's review is written in prose whether he likes it or not. All undergraduate probability textbooks are based on (L1)-(L5), no matter which century these laws belong to.

I disagree with almost every statement in Robert's review so I will have to limit my reply to a handful of issues that I find either important or amusing. I believe that the fundamental obligation of every reviewer is to present the contents of the book in an accurate manner. Of course, the reviewer may disagree with the opinions or claims made by the author. Robert consistently refers to my laws (L1)-(L5) as axioms. I consistently called (L1)-(L5) "laws" in my book. I explained the difference between axioms and laws in Sections 11.4 and 11.5. Even if Robert disagrees with my analysis of the difference between probabilistic axioms and probabilistic laws, he should have presented (L1)-(L5) as laws, not axioms.

Robert writes in Sect. 2:

As often the case in modern debates about the nature of induction, the author calls for the scholarly authority of Karl Popper, "the champion of the propensity theory of probability" (p.43), and of his falsifiability criterion [...] to support his own arguments. In my opinion, this is the extent of the theoretical support for the criticisms contained in the book about the failure of both von Mises' and de Finetti's theories. *The Search for Certainty* contains no philosophical background other than this call to Popper [...] (This is furthermore acknowledged as a "repackaging of Popper's idea for general consumption", p.8.)

I used Popper in a very selective way. His idea of falsifiability is embodied as (L5), the key element of my system (L1)-(L5). Robert writes "At last, (L5) is formulated in a psychological wording rather than in the mathematically unambiguous constraints $P(\emptyset) = 0$ and $P(\Omega) = 1$." Hence, Robert completely missed the fact the (L5) is a philosophical and scientific idea, not a mathematical formula. My critique of von Mises' and de Finetti's theories is based on two ideas, that they are very weak and their key concepts of a *collective* and *consistency* are unusable in practice. These arguments have nothing to do with Popper; they are general arguments that could be used by Catholic theologians, Marxist philosophers and ordinary people. My personal philosophy of science, outlined at the beginning of Chapter 11, represents science as a communication system and hence it is much different from Popper's epistemology. Finally, and crucially, Popper had (at least) two significant proposals in the area of philosophy of probability. One of them is falsifiability embodied in my (L5). I did not adopt his other idea saying that probability is a physical property. Robert did not recognize that (L5) is Popper's idea but saw Popper's ideas in the rest of my arguments, where they are absent. Robert did not notice the difference between axioms and laws in my book. Robert questions the depth of my epistemology in Section 1 of his review. No matter how shallow my epistemology is, I do not have any evidence that Robert penetrated even this little

depth.

Section 4 of Robert's review is titled "Narrow focus". My book is indeed narrowly focused. It is an attempt to establish (L1)-(L5) as the standard scientific laws of probability. The rest of the book is constructed around these laws. The laws (L1)-(L5) are inconsistent with the philosophical theory of von Mises and with the philosophical theory of de Finetti. Hence, I devoted much of the book to an argument showing that the latter theories are useless from the scientific point of view. I did not discuss the logical philosophy of probability (except for a very brief review in Section 2.2) because (L1)-(L5) seem to be more orthogonal than inconsistent with the logical philosophy. Also, the logical philosophy of probability seems to be almost completely unknown to and almost completely ignored by statisticians and other scientists, so I do not consider it much of a competition for (L1)-(L5). On the scientific side, I concentrated on showing how (L1)-(L5) can be used to support both branches of statistics, frequency and Bayesian, in their current shape. I am not aware of any elements of the science of statistics that would contradict (L1)-(L5). I did not find anything in Robert's review that would suggest otherwise. Hence, I do not see any need to defend (L1)-(L5) against any other potential competition at this point.

In Section 3, Robert repeatedly says that I do not understand the purpose of statistics but he fails to tell us what this purpose is. I have to assume that the potential readership of his review, that is, statisticians, do not have to know what the purpose of statistics is according to Robert because there is a general and well established consensus among statisticians on this issue (this was sarcasm, in case you missed it).

I find it annoying and highly objectionable how Robert quotes me out of context and without explanation. I will give two examples of such quotes. At the beginning of Section 5 he quotes my claim that "The expected value is hardly ever expected". Without any further explanation, this sounds like "Red roses are hardly ever red", suggesting that I lost my mind. In section 4.1.1 of my book I give an example of a fair die. The expected number of dots is 3.5. I do not write a formula in the book but any scientist would understand that what I mean is that if X stands for the number of dots then $P(X = EX) = 0$. My more general claim is that for most random variables X encountered in practice, $P(X = EX)$ is either 0 or close to 0. The idea of saying that "the expected value of X is μ " when $P(X = \mu) = 0$ is plain ridiculous and we fail to see this only because of the long tradition of using this expression. Why not use the term "mean" which is not so absurd? "Rape" is now mostly called "canola". "Nuclear magnetic resonance imaging (NMRI)" is nowadays called "magnetic resonance imaging (MRI)". "Calcutta" is now called "Kolkata". I do not see any reason why we could not switch from "expected value" to "mean".

In Section 6, Robert quotes me as saying "the above philosophical claims are incomprehensible to all statisticians" out of context, suggesting that I think that all statisticians are feeble-minded. I ask the reader to see pages 1 and 2 of my book (the first two pages of the Introduction) to see how Robert manipulated my statement. The Introduction to my book, together with the Preface and Contents are posted online for free at my personal Web site and at the publisher's Web site. By the way, Robert

incorrectly states that the quote comes from page 9 of my book, while in fact it appears on page 2.

The following two items are insignificant but they indicate the level at which Robert operates in his review. First, I ask the reader to ponder the expression “Hofstadter’s vulgarisation book” used by Robert in Section 2. It refers to *Gödel, Escher, Bach: An Eternal Golden Braid*.

At the end of Section 5, Robert discusses the cover of my book. A two dimensional, static and highly artistically modified image cannot uniquely represent anything—it is a Rorschach test. When I look at it, I think about playing with dice in the context of a board game. And I see the event “seven sixes.” After reading Robert’s review, I have realized that the cover may represent objects falling into a black hole—a cover appropriate for a book on relativity theory. I invite the reader to apply this Rorschach test to himself. I wonder how many readers will have the same associations as Robert.

Finally, let me discuss briefly a very general aspect of Robert’s review. According to Robert, everything is wrong with my book, from the choice of material and lack of innovative ideas to style and book cover. If the book is worthless and irrelevant then why did Robert bother to write his review? If this is habit-forming then Robert may spend the rest of his life writing reviews of worthless and irrelevant books. I also wonder why *Bayesian Analysis* decided to publish a review of a worthless and irrelevant book. Perhaps, after all, the book is not totally worthless and irrelevant.

References

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