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Comment

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In the introduction to this admirably written paper, Professor Good states that his focus is on influences that Poisson's work has had on statistics and probability "interpreted in a broad sense." The author then highlights three topics: (i) the law of large numbers and the distinction between kinds of probability, (ii) the Poisson summation formula, and (iii) the Poisson distribution. In what follows I shall direct my comments to (i), mainly because this topic is of current interest to me. However, before doing this, I would like to give the following additional information pertaining to Poisson's work on statistics and probability, which appears to have escaped Professor Good's mention, but which may be of historical interest to many of the readers of this journal.

According to Sheynin (1981) it is Poisson who introduced the concept of a *random quantity* and a *cumulative distribution function.* Poisson's influence on Chebychev, the originator of the Russian school of probability (whose most prominent representatives are Markov, Voroni, Lyapunov, Steklov, and Kolmogorov), is beyond any question. It is also of interest to note that Poisson qualitatively connected his law of large numbers with the existence of a stable mean interval between molecules (Gillispie, 1963, p. 438). If the above is true then is it possible that it was Poisson who paved the way for Einstein and von Smoluchowski (see Maistrov, 1974, p. 225) to develop in 1905, probabilistic arguments for a theory of Brownian motion? If such be the case then a proper eponymy for Brownian motion could be *Poissonian-Brownian motion.* After all, it was only 1827, 17 years after Poisson (as Editor for *Mathematics of the Bulletin of the Philomatic Society*) was involved in probability theory (see Bru, 1981), that the English botanist Robert Brown observed the phenomenon named after him. Another noteworthy aspect of Poisson's interest in statistics and probability, and one which appears to have escaped Professor Good's notice (also see Good, 1983a, Part V), is his use of the calculus of probability to clarify Hume's notion of *causality* (see p. 163 of Poisson, 1837). Incidentally, Bru (1981) regards the material on page 163 of Poisson (1837) as a "strengthening of the 'philosophical probability' of the theory of chances and its applications to nature." By "philosophical probability" I take it to mean *logical probability* or *credibility*, and if this be so, then Bru's view would lend support to Professor Good's interpretation that Poisson's concept of probability was that of logical probability.

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In Section 2 of the paper under discussion, Professor Good states that "The empirical evidence that gives some support for the existence of logical probabilities, or at least multipersonal probabilities, is that, for many pairs (A, B) the judgments of $P(A \mid B)$ by different people do not differ very much." Recognizing that the existence of logical probabilities is controversial, I would all the same, like to add a supplement to the above statement. With the recent work by DeGroot (1974) on reaching a consensus, and by Lindley et al. (1979) on the reconciliation of probability judgments, it appears to me, by analogy with Good (1983a, p. 197), that *insofar as logical probabilities can be measured, they can be done only in terms of subjective probability.*

Professor Good's remark about quantum mechanics and Einstein's statement that "God does not play dice" prompted me to do some searching about the physicists' view of probability, and what may have prompted Einstein to make the above, now famous, comment. For this I found the book by Pagels (1983) most informative and fascinating to read. My understanding of the material there, particularly that in

Chapters 3 through 8, gives me the impression that it may have been the *interpretation of probability* that could have been responsible for the disagreement between Einstein and the founders of the quantum theory, Bohr, Born, Dirac, and Heisenberg. [Schrödinger, it appears, never accepted indeterminism. He is said to have remarked that he would not have written his famous paper had he known the consequences of Born's (statistical) interpretation of his wave theory.] According to Popper (1968, p. 198), Bohr, Born, Dirac, and Heisenberg subscribed to the relative frequency view of probability, whereas Einstein's view of probability, at that time, appears to have been subjective. [However Popper (1968, p. 208) claims that much later Einstein adopted, at least tentatively, a frequency interpretation of quantum theory.] In his reaction to Max Born's comment that "If God has made the world a perfect mechanism, He has at least conceded so much to our imperfect intellect that in order to predict parts of it, we need not solve innumerable differential equations, but can use dice with fair success," Einstein says "But an inner voice tells me it [the quantum mechanics] is not yet the real thing. The theory says a lot, but does not really bring us closer to the secret of 'the Old One'."

Professor Good's interpretation that "de Finetti's theorem really proved only that it is not *essential* to assume that physical probabilities exist, not that they *cannot* exist" is novel and thought provoking; it is certainly one that I have not seen before. He further goes on to say that "The assumption that physical probabilities exist is, in some contexts, not even theoretically self-contradictory in my opinion, and in such contexts their use can be regarded as an exemplification of pragmatism rather than fictionalism." The expression "not even theoretically self-contradictory" raises a question in my mind, especially in the light of the fact that Professor Good has made reference to quantum mechanics, for an example of physical probability. My question arises because of the *uncertainty principle* enunciated by Heisenberg, which says that every physical measurement (such as the toss of a coin) involves an exchange of energy between the object measured and the measuring apparatus (which might be the observer). Any such exchange of energy will alter the state of the object which after being measured will be in a state different from before. Indeed the uncertainty principle pointed out the notion of the observer-created reality. It says that the world just "isn't there" independent of our observing it; what is "there" depends in part on what we choose to see—reality is partly created by the observer. This means that it is theoretically impossible to repeat an experiment (even once) under identical conditions, making the cornerstone of the definition of the relative frequency and physical theories of probability unsound. Furthermore the impersonal, public, or objective characteristic attributed to physical probability cannot be conceptually justified either. I am eager to hear Professor Good's comments to the above concerns.

As a closing comment, I would like to state that I have always found a reading of Jack Good's papers a pleasurable experience, and one that leads me through a process of self-interrogation and enlightenment. He always takes us from the simplistic banalities of pragmatism to the euphoric confusion of idealism. As a personal query, would Jack care to comment once and for all, in simple English, whether he is a card-carrying Bayesian, or a noncard-carrying solipsist (he claims not to know any, and by this I assume that he includes himself)?; or would Jack prefer to be called just another Doogian?.

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