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Comment

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1. INTRODUCTION

The present paper is the latest manifesto in Lindley's long crusade to wrest the Holy Land of Statistics from the infidels. In it he has given a new name to this heathen host: *Berkeley*, eponymously named after the Bishop with whom Thomas Bayes had his own disagreements, but also after the campus of the University of California, which "has perhaps the best department broadly holding to that [non-Bayesian] view." This seems a bit unfair to my long-time colleagues Blackwell and Dubins, both enthusiastic Bayesians, who are untainted except through such guilt by association.

As a Berkeleyan, both geographically and in Lindley's ideological sense, I shall take this opportunity to comment on some of my agreements and disagreements with the orthodox Bayesian view presented by Lindley. Of course these are only my personal opinions; Berkeleyans are no more unified in their formulations than are Bayesians.

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2. ROLE OF THE SAMPLE SPACE

This is the topic of Sections 1.3 and 1.4 of Lindley's paper and is mentioned by him as a major point of disagreement. He notes that the sample space is often difficult to specify; I fully agree (see, for example, Lehmann, 1988). Lindley refers to Jeffreys' characterization of the sample space X as "the class of observations that might have been obtained but weren't" and (rightly) declares this class to be an artificial construct. "The practical reality," Lindley writes, "is the data x (not X), the parameter-space Θ and the likelihood function $p(x \mid \cdot)$ for fixed x and variables θ."

However, the sample space is of course only the beginning of Berkeley's violation of this dictum. Specifying a probability distribution (or class of distributions) assigns not only possible values to X but also the possible probabilities of all these values.

The idea that the actual data set is only one of many possible such sets that might have been obtained under the given circumstances is central to the concept of a probability model which underlies the theories of Fisher, Neyman, Pearson and Wald. Since Bayesians also specify such models (although with a different interpretation of probability), they must also at some stage contemplate alternative values of x. To be more specific: near the end of Section 1.4 Lindley considers the case that "observations have been made of n normal quantities" and complains that Berkeleyan inference utilizes the fact that n is fixed; but it equally utilizes the assumption of normality. So why pick out the sample space? There is a good reason: it is the one aspect which does not make an appearance in the Bayes' solution.

The second element of Lindley's "practical reality" is the parameter space Θ. However, in Section 2.2 where it is explained how parameters are introduced to simplify the specification of the model, Lindley states toward the end that "θ is a construct, and not an ingredient in the original, practical problem." I agree with this latter view (in this connection it is interesting to read Fisher, 1922, which introduced the idea of parameter models as the basis for theoretical statistics). And where does that leave the third ingredient $p(x \mid \cdot)$?

It seems to me that the requirement to shun all considerations of alternative values x leads inevitably to the discarding not only of the sample space but of probability models altogether, and thus to a form of data analysis which takes data at their face value and asks what they have to tell us without any additional assumptions. From this perspective the Bayesian philosophy—particularly in the more modern versions of reform Bayesians such as Jim Berger, George Box, Persi Diaconis, James Dickey, Don Rubin and Adrian Smith—appears to have more in common with Berkeley than with a data analytic approach which tries to dispense with models altogether. And in its reliance on models, Berkeley is in a slightly stronger position than Bayes. For it requires one specification less (that of the prior) and can seek refuge from the unreliability of models in robustness, nonparametrics and semiparametrics, a much more difficult task for Bayesians.

3. A MORE FUNDAMENTAL DIFFERENCE

Having in the preceding section argued that B and B have perhaps more in common than Lindley would like to acknowledge, I turn in conclusion to my strongest disagreement with Lindley's philosophy. It is with his insistence on a unique correct answer, on all-embracing coherence, a normative approach which brooks no deviation; it is the attitude which van Dantzig in 1957 (reviewing Jimmy Savage's book) in an unforgettable phrase labeled "Statistical Priesthood."

Lindley concedes that Berkeley's sample space and the Bayesian's prior "both have elements of arbitrariness" and that "there is often little reason for choosing a specific form for $p(x \mid \theta)$." An architect building on such shaky foundations will surely take whatever additional protection is available. Two important protective measures in the present case are: (i) to make the analysis under a variety of different models and priors (for a study that uses such a multiple approach see for example Mosteller and Wallace, 1984); (ii) to evaluate the risk functions or operating characteristics of the resulting Bayes' procedures. Both these auxiliary measures have been recommended by some of the neo-Bayesians mentioned earlier.

Lindley's Bayesian theory is a beautiful structure which provides many important insights. However, its application requires adaptations and compromises, and these bring it much closer to the more pragmatic, less idealistic view of Berkeley.

Lindley complains that the Bayesian paradigm "has not achieved the success it theoretically derives" and predicts that Statistics Departments will "fade away" unless they embrace it. I see the future somewhat differently. A greater danger at the moment seems to me that the profession will turn away from theory altogether (be it Bayes or Berkeley) because of the unrealistic and arbitrary assumptions on which so much of it is based. However, a data-analytic approach, without any possibility of assessing the reliability of its conclusions and of comparing different procedures, in the long run will not be satisfactory either, and an alliance between the three approaches will have to be forged (in this connection, see Lehmann, 1985). In the mean time, we can enjoy the beauty of Lindley's utopia as long as we don't take it too seriously.

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