of θ to be "unbiased," or require an advance specified upper bound to the probability of error of the first kind. That such requirements could lead to absurdities such as randomised "conclusions," assertions with only 90% confidence that a real number lay between $-\infty$ and $+\infty$, etc., was noted by several of the older Berkeley's associates; but the energy, courage, generosity of spirit, brilliant wit, and human warmth of N's character so impressed all those who came into contact with him that the inherent impossibility of the task N had set himself was not stressed, and old Berkeley grew into the over-rigid system which Lindley so mercilessly attacks. Of course, as with the somewhat similar attempts by von Neumann, Birkhoff, and others to "pure-mathematicise" quantum physics, N's programme produced many insights and valuable results in spite of its ultimate failure.

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Comment

James O. Berger

1. INTRODUCTION

There are many reasons to adopt the Bayesian paradigm. Professor Lindley emphasizes the foundational and axiomatic rationales in this paper. Having followed that route to Bayesianism myself, I am particularly appreciative of the job Lindley has done in illuminating the route. I only regret that this paper was not around when I started studying the issues.

I emphasize the foundational nature of Lindley's paper for two reasons. First, it is a common misconception that the arguments for Bayesian statistics are all theoretical, as opposed to practical. To the contrary, an extremely strong case for Bayesian statistics can be made purely on the pragmatic grounds that it is much easier to understand and yields sensible answers with less effort. Lindley has reasonably concentrated on the foundational side, but it is important to note the existence of these very pragmatic rationales. Of course, I completely agree with Lindley that foundational issues can have a profound effect on practice.

The second reason for mentioning the foundational nature of the paper is that, in foundational matters, virtually everyone disagrees in some respect, even (or perhaps especially) Bayesians. Thus the bulk of my discussion focuses on the foundational differences that I have with Lindley, primarily the issue of specification of unique prior probabilities. While this is perhaps a significant issue foundationally, it is much less of an issue in terms of Bayesian statistical practice. Hence, my disagreements with Lindley are actually quite minor from the perspective of statistics in general. Indeed, my motivation for raising the issue (in Section 3) is mainly to argue that uncertainty in probability specifications can be incorporated into the Bayesian paradigm without any major changes being necessary.

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2. FREQUENTIST BAYESIANISM

As I read Section 1 of the paper, I agreed with virtually all of the points raised but felt uneasy at the conclusion that coherence is missing from the Waldian paradigm. After all, admissibility is at the heart of the paradigm and, in a sense, admissibility is just a frequentist version of coherence.

Would Wald have disagreed that the correct solution to the mixture problem is to choose a procedure that is Bayesian? Perhaps not. Indeed, there have existed frequentists who consider themselves coherent Bayesians, in the sense that they agree with the use of Bayes' rules, and even utilization of prior information, but still want to base their evaluations of accuracy on frequentist (Bayesian) measures of performance. Such statisticians would presumably disagree with Lindley's statement that "only the Bayesian attitude is coherent . . . Consequently the sample space is irrelevant." They would agree with the first part, but disagree with the second because of their insistence that only frequentist measures are meaningful.