the suggestion that θ has the same sort of reality as x, the observation.

This article has served to put into sharp contrast the Bayesian and Berkeley schools of statistics. Perhaps it is appropriate to close by remarking on a point of agreement between them.

By and large, all statisticians agree on the use of probability to model uncertainty. Perhaps we should unite on this agreement and look outside mainstream statistics. There we would notice a growth industry in *ad hoc* uncertainty modeling: fuzzy sets, possibility theory, varieties of belief representations, inexact logics, ... While we debate the niceties of priors versus sample spaces, there are many out there developing alternatives to our tools for inference and decision. Moreover, their alternatives, despite so many flaws obvious to us, are apparently far more attractive to those who award research and development funds. Many projects are building decision support systems and inference engines with what I can only describe as "inbuilt irrationality." Is it right that we stand idly by, waiting for their comeuppance? Professor Lindley is one of the few explaining carefully and patiently the flaws of these alternatives to probability modeling. It might be wise for us to forget, at least for the time being, some of the disagreements within statistics and put our energies into the wider debate of the value or otherwise of nonprobabilistic modeling of uncertainty.

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Comment

Joseph B. Kadane

I want to supplement Lindley's admirable overview of Bayesian Statistics with some references and speculations about how modern computing may both influence Bayesian thought and be useful in accomplishing the agenda that Lindley, and before him Savage and others, have set out. The simplest Bayesian analyses, using exponential family likelihoods and stated priors in the conjugate form, do not require computing at all. Raiffa and Schlaifer (1961) give a still rather complete treatment of the computation of posterior distributions under these conditions. Modern Bayesian thought goes beyond these ideas in several respects. The important dimensions of generalization are:

a) The prior may not be stated, but may instead have to be elicited.
b) The likelihood may not be in the exponential family, or the prior may not conjugate with it.
c) The problem may not be the computation of a posterior distribution (or some functional of it) but rather a design problem.
d) Robustness may be of special concern.

I give some brief comments on each in turn.

Joseph B. Kadane is Leonard J. Savage Professor of Statistics and Social Sciences. His mailing address is Department of Statistics, Carnegie Mellon University, Pittsburgh, Pennsylvania 15213.

1. ELICITATION

The idea of elicitation is to discover a prior that models the user's opinions well. Unfortunately this very important problem has not received the attention it deserves from the Bayesian computing community. For example, in Goel's (1988) survey of Bayesian programs, only two of the more than thirty listed concern elicitation, and neither of those was ready to be released. Nonetheless, this is a natural area for computation, particularly of the interactive sort. An early attempt of my own is given in Kadane, Dickey, Winkler, Smith and Peters (1980). For some more recent work in elicitation see Chaloner and Duncan (1983) and Gavaskar (1988). A very interesting recent work by DuMouchel (1988) uses graphical methods in the elicitation of a generalized ANOVA model.

As I have already remarked, I consider elicitation to be a very fruitful area for future work. One would think that the flexibility offered by modern devices such as mice would be useful in permitting users to express their views. While to date all the work reviewed here has assumed a given, known likelihood function, future elicitation work will, I believe, deal with the fact that likelihoods, as well as priors, are subjective and hence subject to elicitation (Bayarri, DeGroot and Kadane, 1988). Perhaps Lindley's work reported here will be the basis for future computer work in elicitation.

2. COMPUTING POSTERIOR MOMENTS AND MARGINAL DISTRIBUTIONS WITHOUT CONJUGACY

This is an area of rapid progress in the last few years. There are several methods under investigation. The simplest method relaces an integral by a summation over a grid, which works well when the dimension of the parameter space is small. However, grids are exponential in the dimension, so they rapidly become infeasible as the dimension becomes even moderate. One response to this problem has been the development of methods asymptotic in the sample size. Some work along these lines are Mosteller and Wallace (1964), Lindley (1961, 1980), Leonard (1982), and Tierney and Kadane (1986). Kass, Tierney and Kadane (1988) give a review of this literature. The most recent programs are those of Tierney (1989). The current advantage of this work is that it gives a second-order-accurate approximation to posterior moments the computation of which is quadratic in the dimension of the parameter space. The methods are no more complicated than computing maximum-likelihood estimates and their covariance matrices. A second line of response is to use classical polynomials in an iterative way, as suggested in Naylor and Smith (1982) and Smith, Skene, Shaw, Naylor and Dransfield (1985). The set of programs developed at Nottingham based on these ideas are a strong competitor to the asymptotic methods. Yet a third line being aggressively pursued is based on Monte Carlo sampling with an importance function (Geweke, 1990; Stewart, 1985; Zellner and Rossi, 1982; van Dijk and Kloek, 1985). Another variant leads to a sample from the posterior distribution (Tanner and Wong, 1987). There is recent and very interesting work on Gibbs sampling by Gelfand and Smith (1990) along these lines. It is my opinion that after a period of exploring the advantages and disadvantages of each of these methods in different situations, attention will be directed to hybrid methods that will seek to combine the advantages of each. While such a development is several years ahead, progress has been swift in this domain and Bayesians can now compute many of the posterior quantities they need.

3. DESIGN

An almost completely neglected area of Bayesian computing, the design of experiments is a very rich area for the application of Bayesian methods, and nearly ready, scientifically, for programs to be written. The analysis of designs puts great strains on the posterior calculations described above, since in principle to measure the worth of a design, it is necessary to anticipate each of the data sets that might ensue, its analysis and hence worth, and its probability. Consequently most work in optimal designs, at least at first, will concentrate on tractible families in which the integration over the sample space can be done analytically. Even in the ad hoc way that classical statisticians do designs now, they have to ask themselves or their clients about the size of effects they expect to see. Surely Bayesian programs with formal prior elicitation to guide design can be a great advance over this informal method. In fact, classical statisticians may find Bayesian design more appealing, at first, than Bayesian analysis of data after the design has been implemented. The recent work of Chaloner and Larntz (1988, 1989) along these lines is very welcome.

4. ROBUSTNESS

Another rich topic for Bayesian computing is the issue of sensitivity and robustness. Virtually any Bayesian program can be run again under different assumptions, but it is often possible to speed the analysis of the consequence of a different prior, likelihood, dropping single data points, etc. Some recent efforts along these lines are Gelfand and Smith (1990), Kass, Tierney and Kadane (1989), O'Hagan (1988), Polasek and Potzelberger (1988), but I expect that there will be many more.

5. CONCLUSION

This fast and idiosyncratic review of Bayesian computational work is intended to supplement Lindley's find paper. There's lots going on, and much remains.

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Comment

E. L. Lehmann

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.

E. L. Lehmann is Professor Emeritus at the University of California, Berkeley. His mailing address is Department of Statistics, Statistical Laboratory, University of California, Berkeley, California 94720.

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