Finally, a query: In DFb, Diaconis and Freedman use the past tense in describing themselves as subjectivist and classical Bayesians, respectively. How do they describe themselves now?

REFERENCES

BERGER, J. O. (1984). The robust Bayesian viewpoint. In *Robustness of Bayesian Analyses* (J. Kadane, ed.). North-Holland, Amsterdam.

BOX, G. E. P. and TIAO, G. C. (1973). *Bayesian Inference in Statistical Analysis*. Addison-Wesley, Reading, Mass.

CLAYTON, M. K. (1985). A Bayesian nonparametric sequential test for the mean of a population. *Ann. Statist.* **13** 1129–1139.

DE FINETTI, B. (1975). *Theory of Probability* **2**. Wiley, New York.

DIACONIS, P. and FREEDMAN, D. (1986a). On inconsistent Bayes estimates of location. *Ann. Statist.* **14** 68–87.

DIACONIS, P. and FREEDMAN, D. (1986b). On the consistency of Bayes estimates. *Ann. Statist.* **14** 1–26.

SERFLING, R. J. (1980). *Approximation Theorems of Mathematical Statistics*. Wiley, New York.

DEPARTMENT OF STATISTICS
UNIVERSITY OF WISCONSIN
MADISON, WISCONSIN 53706

A. P. DAWID

University College, London

The mathematical beauty and tractability of the Dirichlet prior render it almost irresistibly seductive. But beware! Rocks and shipwreck await the poor Bayesian navigator captivated by its siren song. Brown (1976) shone a little light on these murky waters. Now Diaconis and Freedman deserve the gratitude of all explorers for illuminating some of the more treacherous obstacles to a smooth passage.

Beyond these specific warnings, what broader morals are to be drawn? In view of the fact that, generically, the pair (θ, μ) is inconsistent, it is not really surprising that the authors can find such a pair. What I find far more surprising is the existence of priors μ (e.g., tail-free) which *are* consistent at each θ. Perhaps this is only possible because of the rather weak definition of consistency employed. Nevertheless, it is an important property, and one which demands further characterization.

Choosing a prior for an infinite-dimensional parameter space is always going to be problematical, and any accessible prop (such as consideration of imaginary results) should be grabbed. For example, any two different priors are, generically, mutually singular, and so involve incompatible world views of what is even possible. This can be expected to lead to diverging inferences from the data. The mere possibility of consistency, in the problem considered, is therefore an unexpected bonus.

Diaconis and Freedman have only considered i.i.d. observations with unknown distribution. Now given any prior, and data $(X_1, X_2, \ldots, X_n)$, we can construct the predictive distribution for X_{n+1}. Consistency implies that, as $n \to \infty$, the discrepancy between this predictive distribution and the "true" distribution of X_{n+1} will, in a suitable sense, approach zero. This property can be extended to apply to much more general models for the data sequence, involving stochastic dependence and varying marginal distributions, where it has been termed "prequential consistency" (Dawid, 1984). (Note that the "counterexample" in Theorem 1 of Diaconis and Freedman (1986) does in fact yield prequential consistency, and so need not be regarded as especially troubling. However, the location model with a symmetrised Dirichlet can be prequentially inconsistent.) In these more general models, is consistency attainable *at all*, for sufficiently large parameter spaces? For example, can one consistently estimate, prequentially, a process known only to be stationary? The arguments of Dawid (1985) strongly suggest that, in general, prequential consistency will *not* be attainable by any method, be it Bayesian or not. It would be extremely valuable to characterize problems which allow consistency at all. I conjecture that, in any such problem, there will exist a consistent *Bayesian* analysis.

Diaconis and Freedman seem to imply that their results cast a shadow over the use of Bayesian methods, because these can be inconsistent. But so too can ill-chosen non-Bayesian methods. Conversely, for the problem considered of estimating a distribution, there do exist consistent methods, both classical and Bayesian. The moral to be taken away from their analysis is, not that Bayesian methods should not be used, but that great care is needed in selecting such a method. However, exercise of this care when considering priors less tractable than the Dirichlet, whose implications are correspondingly less transparent, is likely to pose serious problems of implementation.

Acknowledgment. I am grateful to Mark Schervish for discussions on these points.

REFERENCES

BROWN, P. J. (1976). Remarks on some statistical methods for medical diagnosis. *J. Roy. Statist. Soc. A* **139** 104–107.

DAWID, A. P. (1984). Statistical theory. The prequential approach (with Discussion). *J. Roy. Statist. Soc. A* **147** 278–292.

DAWID, A. P. (1985). The impossibility of inductive inference (comments on "Self-calibrating priors do not exist", by David Oakes). *J. Amer. Statist. Assoc.* **80** 340–341.

DIACONIS, P. and FREEDMAN, D. (1986). On inconsistent Bayes estimates of location. *Ann. Statist.* **14** 68–87.

DEPARTMENT OF STATISTICAL SCIENCE
UNIVERSITY COLLEGE, LONDON
LONDON WC1E 6BT
ENGLAND