and provided any procedure is interpreted under an adequate (conditional) probabilistic setup.

Thus I agree with Professor Lindley that the calculus of probability is a privileged instrument to operate with and to represent uncertainty, but I am not convinced that only probabilities conditional on available information are finally relevant. Sampling probabilities, such as significance levels, may also convey useful information precisely for being conditional on unobservable parameters; but they should clearly be properly interpreted and, in particular, not be confused with posterior probabilities; this later issue has been aptly argued in Berger and Delampady (1987)

In conclusion, I very much enjoyed reading Professor Lindley's fascinating exposition. I nevertheless stick to the idea that developing a proper understanding of both the sampling theory and the Bayesian paradigm is more appropriate than overdeveloping one and ignoring the other. I believe that this attitude is likely to be more fruitful for statistical practice and for understanding within the statistical community.

ADDITIONAL REFERENCES

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Rejoinder

Dennis V. Lindley

I am most grateful to the editors for inviting so many fine statisticians to comment on these lectures and to the discussants for raising so many important and interesting points. Where a point is mentioned by two or more, it appears under the first. This has the apparent difficulty that later ones appear to deserve a shorter reply.

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2. The Fisherian tradition (also mentioned by Barnard) is usually better suited to the treatment of scientific and technological data than that of Wald. The latter was discussed because the Wald lectures, on which this article is based, were given in the States, where Wald's ideas are more commonly encountered than in Britain. A critique of the Fisherian view has been given by Basu (1988).

3. A common objection to the Bayesian view is "where did you get that prior?" (Section 1.3); hence the emphasis on elicitation. A careful reading of Jeffreys will show that he often does not use flat priors (for example, in hypothesis testing). More recent work has shown multivariate, flat priors to be unsatisfactory. Why should personal judgment be left qualitative? The failure to quantify can lead to imprecision and vagueness.

4. Cox agrees with de Finetti in not liking axioms. The key question is surely our attitude to uncertainty: how are we to appreciate an incompletely understood world? A basic assumption is not that all probabilities are comparable, but that all uncertainties are. This assumption seems reasonable until someone can produce criteria that divide uncertainties into two or more types. So far as I am aware, this has not been done.

6. Temporal coherence has received relatively little attention. Some additional assumption seems called for. One approach is to recognize that probability statements typically contain *three* arguments and can be written $p(A \mid B:C)$, read as the probability of A conditional on knowing B and supposing C. The distinction between B and C is that the former contains known events, the latter events supposed to be true. Thus, in the distribution over sample space, a parameter would usually belong to C since its value is unknown. Temporal coherence requires an extra axiom, $p(A \mid B:CD) = p(A \mid BD:C)$. This says that as D passes from merely supposed, to experienced or known, the probability does not alter. With the axiom one can write $p(A \mid B:C)$ as $p(A \mid BC)$ and the distinction between supposition and fact is irrelevant. (Mouchart, in his contribution, reminds us that Pratt, Raiffa and Schlaifer, 1964, similarly felt the need for an extra axiom.) If the probability does change in practice, this is because more was experienced than merely D and that this additional experience was not part of the conditioning in the original probability statement.

The example is similarly handled using conditioning. The original probability over 10 and −20 was