procedures which provide sensible answers when the prior specification is in doubt, very much as non-parametric statistics provide answers when the model is in doubt.

Moreover, as Lindley emphasizes in Section 5.5, "it is the practical test of usefulness which will eventually establish the paradigm," and I know of precious little Bayesian applications, including his more recent work (see Lindley, 1988), which does not use, even if only in the last step of a hierarchical structure, some form of reference prior.

To conclude, I would like to reemphasize a point which Lindley has very often made: real problems are *always* decision problems; only a decision theoretical perspective is a sure guide in any real problem to identifying the relevant uncertainties, and the kind of data one might be able to use to reduce them, thus defining the relevant 'statistical' problem; moreover, only a decision framework provides a solid foundation for the solution of those 'statistical' problems; but, as we all know, the solution must then be Bayesian.

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Comment

David R. Cox

It is a pleasure to have the chance of congratulating Dennis Lindley on this lucid article which reviews important material and also gives new results; I particularly liked the treatment in Sections 6.3–6.7 of personal probability assessments.

Such a wide range of material is covered that it is hard to know how best to comment, but in essence there are two key questions for consideration. First, just how important and relevant is the personal probability approach for direct quantitative use in applied statistical work in various fields? Secondly, given that personal probability is under study, is the present approach, strongly in the tradition of F. P. Ramsey and de Finetti, entirely satisfactory?

The following brief comments address these issues.

1. *Terminology.* The encouragement of individuals to label themselves as Bayesian or non-Bayesian seems to me most unfortunate, suggesting that the ideas Dennis Lindley is advocating have to be accepted as universally applicable or totally rejected. Perhaps the term *exclusive Bayesian* should be used for those who wish to attack *all* formal statistical problems via personal probability; others may be more selective in their use of these ideas.

2. *Comparisons.* It is a pity that the comparisons in the paper are largely between the Bayesian approach and the Wald decision theoretic formulation. Other approaches, rather more in the Fisherian tradition, seem more relevant for the careful interpretation of scientific and technological data than the Wald formulation. Of course, such other approaches have their own difficulties and often involve what are sometimes called adhoceries; one may only hope that, as so often, today's (good) adhocery is the basis for tomorrow's general theory.

3. *Direct Use in Applications.* There have surely been in recent years a good many fruitful applications of formally Bayesian arguments in various areas of study, but, so far as I can see, rather few of them have depended strongly on the elicitation of specific prior beliefs, but rather have been fairly close to Jeffreys' line of argument involving flat priors, which, if used with caution, produce, often very elegantly, answers close to those from sampling theory. Lindley writes as though the main obstacle to implementation of specific priors is the difficulty of eliciting them, but there is the more basic issue as to the desirability, in certain cases, of keeping very separate, as far as is feasible, (a) what is regarded tentatively as given for the discussion in question, (b) what is provisional personal judgment and (c) what is provided by the data, under certain assumptions. It is not at all a question of eliminating personal judgment, but rather of isolating its role and, often, of leaving that role as a qualitative one. This seems especially desirable at the frontiers of areas of science and technology where prior

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