Comment

Michel Mouchart

It is a pleasure to have the opportunity to comment on Professor Lindley's paper on the foundations of inference. Professor Lindley should be warmly congratulated and thanked for giving such a rich and thought-provoking paper, the reading of which is extremely rewarding.

A basic concern of this paper is to remind us how rich the Bayesian approach is and how poor is its appreciation by the statistical community: apart from selected members of an exclusive club, the gastronomes would seem to ignore where the good eating places are! Let me suggest that the logical bases of Bayesian statistics, how appealing they are, do nevertheless raise some difficulties, the unawareness of which does not help for a larger spread of the Bayesian approach; and that sound statistical practice should maybe not be exclusively addicted to the Bayesian procedures.

In short, I tend to interpret Savage's Foundations as follows: if a decision-maker is able to define the primitive concepts of the holy trilogy "Act-Event-Consequence" in such a way that the proposed set of axioms is acceptable, then a "coherent" behaviour of decision-making is representable through maximization of expected utility. Formally, this theorem of expected utility concerns the bilinear representation of complete pre-orders. At that formal level, I know of at least two types of arguments suggesting that the range of application of that theorem might be less wide than apparently claimed by Professor Lindley.

One argument turns around the difficulty of defining the primitive concepts in a way that should be operational and should make the axioms defining rationality acceptable. A recent survey by Drèze (1987) shows, in particular, that problems known under the heading of "moral hazard" and of "state-dependent preferences" are indeed substantial and may involve, in particular, serious difficulties in identifying separately utility and probability. Even if I feel more sympathy for those advances which, like Drèze's, develop Savage's approach rather than for those which dismiss it, I nevertheless believe that, in some instances, the real situation cannot be accommodated by just "maximizing expected utility."

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A second argument is related to Savage's discussion of the "small" world and of the "large" world. I am in total sympathy with Professor Lindley when he insists, in several places, on the importance of that distinction, but I believe that its implications should be pushed further. Indeed, it should be recognized that the theorem of expected utility makes no allowance for the cost of elaboration and of operation of the model. If, for example, I needed, every morning, two hours of computer time before deciding to bring or not to bring an umbrella in my briefcase, who would consider this attitude as "rational"? In statistics, we are not willing to describe an all-encompassing model before we start analysing data; we rather start with a "simple" model which represents our "small" world and implicitly preserve the possibility of "enlarging" our small world if need be.

This difficulty is logically related to a remark made by Pratt, Raiffa and Schlaifer (1964) when they noticed that the extensive form of analysis requires one axiom more than the normal form of analysis, namely an equivalence between present preferences among conditional lotteries and conditional preferences among present lotteries. This supplementary axiom, the fifth one in Pratt, Raiffa and Schlaifer (1964), may be interpreted in the framework of temporal stability of preferences, an issue made more difficult in case of state-dependent preferences, or in the framework of the question whether any received information has actually been modeled before it has been received. Thus the question of which information should the supplementary axiom embody is bound to the question of the border between the small world and a larger one. Unfortunately I know of no theory to draw such a border. I nevertheless think that a crucial role of statistical methodology is precisely to give guidelines in modelling rather than limiting itself to dictate rules of inference for given models.

Florens and Mouchart (1988) have suggested the design of a kind of Bayesian hypothesis testing as a device for a Bayesian statistician to handle his own doubt about his model and the basic argument for not measuring in terms of probability (some aspect of) his uncertainty is precisely the cost of developing a more complex model. Thus in Drèze and Mouchart (1989) we argue that hypothesis testing should be welcome in the tool kit of the Bayesian statistician, in forms varying from a pure sampling theory procedure to a pure Bayesian two-decision problem, admitting intermediary procedures as in Florens and Mouchart (1988) 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

DRÈZE, J. (1987). *Essays on Economic Decisions under Uncertainty.* Cambridge Univ. Press, New York.

DRÈZE, J. and MOUCHART, M. (1989). Tales of testing Bayesians. CORE Discussion Paper No. 8912, Université Catholique de Louvain, Louvain-la-Neuve, Belgique.

FLORENS, J. P. and MOUCHART, M. (1988). Bayesian specification tests. CORE Discussion Paper No. 8831, Université Catholique de Louvain, Louvain-la-Neuve, Belgique. To appear in *Contributions to Operations Research and Economics: The Twentieth Anniversary of CORE* (B. Cornet and H. Tulkens, eds.). MIT Press, Cambridge, Mass.

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