

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.

ADDITIONAL REFERENCES

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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 ap-

proach 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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opinions fluctuate dramatically both between individuals and in time. The prescription "just give the likelihood" is unhelpful in problems of realistic complexity. I certainly agree that where, as in work on expert systems, it is desired to enter quantitatively such opinions, it is reasonable to exhaust the possibilities of probability before going to more elaborate notions. In particular one must try and persuade advocates of fuzzy logic to give an operational justification for their rules of manipulation.

4. *Role of Axioms.* While there is a clear role for axiomatization in pure mathematics, even there I thought the spirit of Bourbaki to be on the wane. In substantive areas the role of axiomatic approaches is much less clear. What have attempts to axiomatize the foundations of quantum theory contributed, for example (not a rhetorical question)? Do axiomatizations of personal probability do more than show the consequences of the question-begging assumption that all probabilities are for all purposes comparable?

5. *Formulation of Personal Probability.* Now consider the second key question. Suppose that we want a theory of personal probability. Is the one outlined here satisfactory? Here I am unclear whether the criticisms that follow are basic or concern essentially issues of presentation. The primary emphasis is on self-consistency, coherence. But in forming my own judgments and assessing other people's I am not so concerned with being coherent as with being, in some sense, near to the truth. In the use of "experts" judgment, I want, at least in principle, to know why they hold the views they do. Presumably they are experts because they have seen more relevant information than the rest of us. At least in principle, what was that information and why have they interpreted it as they have? Now Lindley may well reply that of course their probability must be coherent with that information and the more such good information the better. In that sense the point at issue is one of presentation, but I would prefer to put the information first, with instructions on how to use it to derive a probability, leading to a position closer to Reichenbach than to Ramsey and de Finetti. This is all the more relevant when, as would often be the case, the experts do not agree with one another.

6. *Temporal Coherency.* I hope that in his reply Lindley will say something about what seems to me a particularly major limitation of his treatment. Coherence, and let's accept that as the basic requirement, refers to one individual at one time. Prior probability refers to assessment, perhaps about some hypothesis, in the absence of the data subsequently to be collected and analyzed. Now the following is a common experience. Before the data are obtained, one considers the possible broad outcomes and what their interpretation might be. The data are obtained and show some fea-

ture totally at variance with what was expected. On reflection, it is seen that in fact the new feature ought to have been anticipated on general grounds, perhaps indeed that an initial theoretical analysis of what was to be expected had been done wrongly with a numerical or conceptual error. In my own experience of laboratory work, this is a relatively common occurrence. That is, in some cases the prior probability must depend sometimes on the data and even more often on experience gained while collecting the data. There is nothing incoherent in this, in that coherency refers to one time. Yet some of the radical conclusions of coherency depend on temporal coherency. In particular, any notion that the Bayesian treatment evades the difficulties of hypotheses formulated in the light of the data is, I believe, illusory. I am not suggesting that prior probabilities should be changed frivolously as data become available only that sensible behavior may demand change. To be specific, suppose that Y is normal with mean θ and unit variance and careful consideration suggests that either $\theta = 10$ or $\theta = -20$, say with equal prior probabilities. One observes $y = 0$. The data may be biased and the prior arguments sound. But suppose that reconsideration of the prior arguments shows that there is a third possibility namely that $\theta = 0.1$ which is entirely reasonable. This is not I believe at all adequately covered by holding back ε of prior probability, partly because the rational assessment of possibilities based on evidence other than the data may suggest that 0.1 ought to have high probability and partly because one has no guarantee that had the observation been -5 some ingenious person might not have concocted a theory that explained that convincingly. The ε would have to cover a host of possibilities some never explicitly formulated. I believe that these considerations, which are of course not new, represent in idealized form genuine dilemmas. While the arguments do not justify the paraphernalia of multiple comparisons, they do suggest that there is a genuine need for confirmatory studies of explanations constructed after seeing the data and that the simple Bayesian analysis of them is inadequate.

7. *Combination of Information.* Merging of prior and likelihood is an instance of the combination of information from several sources. The analysis of series of experiments bearing on the same issue has a long history and there seem from a common-sense point of view two questions to be faced:

- (a) Are the various sources of information mutually consistent? If not, can discrepancies be explained, or can they reasonably be represented as random, etc.?
- (b) Given consistency, how is merging to be carried out?

The Bayesian formalism deals elegantly with (b) but ignores (a). I am, of course, not suggesting that individual workers using Bayesian methods would necessarily ignore the issue, but the point is surely enough to refute any claim that the Bayesian formalism as normally presented is a comprehensive one. This point is related to 6.

In summary, I think it important to keep these arguments in perspective. The key issue in many applied statistical problems is not the framework for formal inference but rather the judicious choice of questions asked and their formalization, the choice of model in particular.

Comment

Simon French

As usual Professor Lindley has provided us with much to ponder. In this article he has presented the Bayesian position with great clarity; and, as a Bayesian, I can find little quarrel with most of his words. Nonetheless, as a discussant I have a duty, and there are one or two comments that I might make

There are times when someone says something with which you disagree, but you are unsure of precisely why. One such time happened to me in a *viva voce* examination, and Professor Lindley was my examiner. He has been pressing me on my understanding of the Bayesian position, and—I think—I had been reciting my catechism well. Then we came to the subject of utility. I searched for words to capture my understanding, but those that came did not find favor until I mumbled something about probability and gambles between the best and worst consequences. Professor Lindley leant back in his chair. Utility was probability, he explained, in the sense that he now uses in Sections 4.1 and 4.2. This explanation discomfited me, but I was unsure why. In any case, it certainly did not seem the time to press the matter further. Perhaps now is.

Professor Lindley contrasts the Bayesian paradigm with the “Berkeley” largely on their differing uses of probability. To me the difference seems more fundamental. In all analyses we build models. Some models are purely descriptive: they describe the world outside the person who “commissions” the analysis—call him or her the scientist. Other models are normative: included in them are a representation of the scientist’s judgments. Moreover, in normative modeling the intention in representing the scientist’s judgments is not to describe them as they are but to represent them in an idealized way (French, 1986; Bell, Raiffa and Tversky, 1988; and Phillips, 1984). For instance, in

reality few people’s beliefs are fully coherent, but we represent them in a Bayesian analysis by idealized, coherent probabilities. The purpose of doing so is to provide the scientist with guidance on how his or her judgments *should* evolve. If you like, the idealized behavior of the scientist built into the analysis is offered as a “role-model,” suggesting how the scientist’s views should change in the light of his or her data.

For me the difference between the Bayesian and Berkeley schools of inference lies in the unwillingness of the latter to use normative modeling. Berkeley statistics strives to be objective by leaving the judgments of the scientist out of the model. That Berkeley statistics ultimately fails in its attempt to be objective is a point on which I know Professor Lindley and I are in total agreement, and also one that I shall not pursue here. Rather let me return to the question of utility and its standing *vis à vis* probability.

Scientists are not the only people to have need of normative modeling. Decision makers, as Professor Lindley notes, also can benefit from its support. Normative modeling techniques can be used to guide and advise anyone who needs to express and act upon judgment in a formalized manner. Focusing on decision makers, the great strength of the Bayesian approach to decision analysis is that it separates the modeling of belief and uncertainty from the modeling of preference and value judgments, only drawing these two strands together in a coherent fashion at the end of the analysis. Now preference can exist independently of beliefs and uncertainty: I do not need to consider gambles to know that I prefer bananas to oranges. Since preference has an independent existence from beliefs and uncertainty, its modeling need not lead to a derived measurement system founded upon probability theory. There are ways of modeling preference in the complete absence of uncertainty: ordinal value functions, multi-attribute value functions, value difference functions, etc. None of these

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