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Comment

José M. Bernardo

I was delighted to be asked to contribute to the discussion of this article by the man whom I have always proudly considered my *maestro*. I will limit my comments to a couple of issues.

1. Professor Lindley has long been arguing for the indirect assessment of probabilities, suggesting that one should always try to "extend the conversation" to include other related events, and then combine the results by simple use of probability theory. It is hard to overestimate the importance of such advice, and the work he reports on conditions under which improvement is guaranteed is especially welcome.

I would like to illustrate this procedure with a suggestive example drawn from my recent work in election forecasting. Trying to predict the outcome in Valencia of the recent European Parliamentary elections, I designed a survey where 1000 people over 18 randomly chosen from the census were asked to state not only the party they intended to vote for, but *also* the party they voted for in the last election. By only using the numbers $\{n_i,\ i = 1, \cdots, 6\}$, of the people expressing their intention to vote for party i, I got the estimates of the percentages of the vote to be obtained by each party which are reproduced in the first row of Table 1.

Alternatively, using the numbers $\{n_{ij},\ i = 1, \cdots, 6,\ j = 1, \cdots, 6\}$, of the people expressing their intention to vote for party i given that they voted j last time, and then using the probability equation

$$p(i \mid \text{data}) = \sum_{j=1}^{6} p(i \mid j, \text{data})\, p(j),$$

I obtained the estimates reproduced in the second row. Note that the $p(j)$'s, the proportion of people who voted for party j last time, *are known*, for those are the results from the past elections.

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In both cases I used a hierarchical Multinomial-Dirichlet model, with a reference prior for the Dirichlet (hyper)parameters, and allocated nonresponse by means of a probabilistic classification procedure (Bernardo, 1988) based on the social profiles (age, sex, level of education) of the nonrespondents, which are known from the census.

Comparison of these estimates with the final results, reproduced in the third row of Table 1, is striking. The direct estimates are rather poor, probably due to the bias induced by people's propensity to relatively overstate their alignment with the party in power (the socialists in Spain). The indirect estimates, however, are surprisingly good, with an average absolute error of about 0.4%, to be compared with the standard deviations of about 1.5% which would correspond to the naïve analysis of the sample of size 1000. It is important to note that I had no need to invent some form of "bias correction"; probability theory did it all "automatically."

2. Any review is invariably biased by his author's preferences, and Lindley's account is no exception. I would like to draw attention to one of my own biases, the role and use of reference "noninformative" priors, which he has chosen not to mention.

In Section 5.1, Lindley recognizes the need for robust procedures with respect to the choice of the prior $\pi(\theta)$, to the point of considering this necessary for the change of paradigm to take place; surprisingly however, he blames Berkeley for not taking on the job. But, if Berkeley has not, Bayes has made some progress. Indeed, reference priors (Bernardo, 1979; Berger and Bernardo, 1989) are best seen as robust

TABLE 1
European parliamentary elections. Percentage of valid votes in the province of Valencia

	Socialist	Conservative	Nationalist	Communist	Liberal	Other
Direct	53.9	15.7	7.2	8.0	5.5	9.8
Indirect	41.1	20.0	10.4	7.3	6.4	14.8
Final	41.0	20.7	11.0	6.5	6.3	14.5

procedures which provide sensible answers when the prior specification is in doubt, very much as nonparametric 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 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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