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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

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