

unkindest remark (which is not so unkind): "It is difficult to see how these questions can even be posed within the frequentist framework." This seems wrong. There is no difficulty in posing the questions, in either the frequentist or Bayesian framework; Hodges just did it. The problem is finding answers.

Now there comes a shade of difference between us. He is a little more optimistic than I am about the potential usefulness of Bayesian techniques for properly integrating judgments about uncertainty. For example, he discusses predictive distributions starting from (i) a prior on models and their parameters and (ii) a likelihood function for the data given the model and parameters.

This is quite sensible, provided there is a sound basis for choosing the prior and the likelihood. Unfortunately, Bayesian policy analysts can be just as slaphappy in such matters as us frequentists. For discussion of this issue, see Freedman and Navidi (1986) or Hill (1985).

Good statistical analysis can be done in either the frequentist or the Bayesian framework. However, for either approach to succeed, the analyst has to get the model right, or close enough. That idea may seem ridiculously old fashioned. As policy analysts can be heard to sputter, "Models be right? How can they be right? They're all approximations. Even Newton was

wrong. And a mystic besides." Because nothing is perfect, anything goes.

Hodges wants "to bring de Finetti to . . . practitioners." As I understand him, for de Finetti a prior represents a major intellectual commitment to be adopted only after serious investigation of the subject at issue. If policy analysts followed that percept, we would all be better off. The real issues here are of science, not statistical technique.

#### ADDITIONAL REFERENCES

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## Comment

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Now comes James Hodges to inform us on some of the larger issues of statistics. And what are these issues? They are the ones that statisticians have dealt with—lo these many years—uncertainties from various sources. And there are other issues besides—is it an observational study? a controlled experiment? a retrospective investigation? a haphazard collection of items? Is what is measured or observed actually what one defines as measured? Are there flawed observations? Was the experiment or trial carried out according to the protocols? Is there a temporal imperative with regard to an action or a decision? There is, to say the least, limited interest (other than procedural

validation perhaps) in the prediction of events that already have occurred and been observed.

What is the point then? The point is that we have here a lucid and trenchant exposition vividly reminding us of three of the principal sources of uncertainty or variation. What is more novel than most previous explications is that the sources are related to predictivism, which is stressed as the penultimate aim when taking an action is the ultimate goal. Hence, from my point of view, there is really nothing to quarrel with. But it is the job of a discussant if not to be quarrelsome to be at least quibblesome—to coin a neologism.

Hodges intimates that for proper application of statistical methods, the implementation of de Finetti's approach is required. He also states that the approach "lacks a crucial connection to real problems." I would like to quibble with both these points. In regard to the latter point, we have only to realize that de Finetti was involved in applications especially in finance,

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