principal method for checking that kind of failure, at least in the United States, is the malpractice suit, and such suits might even provide some empirical evidence about human frailty of technical professionals, although not necessarily transferable to the cases Hodges has in mind!

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Comment

Joseph B. Kadane

I agree with Jim Hodges' approach to problems of robustness and uncertainty, and congratulate him for his clear exposition of it. I would, however, add a few remarks and references.

Although his paper cites de Finetti as coming "closest to the goal of a complete context for statistical activity," Hodges does not in this paper bring his analysis very close to de Finetti's ideas. For example, he does not mention the extreme subjectivity of de Finetti (probabilities represent a person's opinions; different people may have different opinions). Whose opinions do or should a Rand logistics study represent? Are different experts consulted on different aspects of the problem? If so, by what principles should such opinions be brought together?

A second important aspect of de Finetti's work is his emphasis on prevision (see Goldstein, 1986). There are important questions about elicitation using de Finetti's methods when ethical neutrality fails, as it will for most experts most of the time (Kadane and Winkler, 1987a, 1987b).

A third important aspect of de Finetti's work is his insistence on finite additivity of probabilities. de Finetti believed that while your probabilities might be countably additive in a given situation, there is no axiom that they must be. Mere finite additivity changes the nature of probability theory, particularly in the failure of conglomerability (Schervish, Seidenfeld and Kadane, 1984). This has a variety of consequences for statistics (Kadane, Schervish and Seidenfeld, 1986; Hill, 1980a). It would be interesting if Hodges would remark on how these aspects of de Finetti's work may have influenced his work and that of his Rand colleagues, or how they might.

With respect to Bayesian ideas of robustness, there are several important approaches left unmentioned. First, there is the classic paper of Edwards, Lindman and Savage (1963), which introduced the idea of stable estimation. There is a series of papers (Kadane and Chuang, 1978; Chuang, 1984) concerning what happens if the prior, likelihood or utility as assessed is slightly off from "true." These two papers study conditions under certain topologies in which the achieved expected utility is continuous. There is also important work of Novick and Ramsey (1980) and of Hill (1980b).

Hodges mentions puzzlement that so few applications use predictive distributions. In the area of parametric elicitation, these have been used for some time. Predictive distribution in this context have the advantage of being able to present questions to an expert on variables that are familiar, instead of about parameters of an unfamiliar distribution. For papers along these lines, see Kadane, Dickey, Winkler, Smith and Peters (1980), Kadane (1980) and Winkler (1980). The former gives a concrete application in the Appendix. A second use of those programs in a medical context is described brifely in Kadane (1986).

Finally, Hodges might be interested to learn of an explicitly Bayesian effort on the spare parts problems for Naval aircraft almost 20 years ago (Brown and Rogers, 1973). There the problem was that the airplane in question had not yet flown, so priors based on spare part usage of other airplanes were used, together with a judgment about how similar the mechanics (and hence, perhaps, spare parts usage) would be. An additional problem was that spare parts built while the airplanes were being built were much less expensive than spare parts built later, and that spare parts could be partially built, and then completed,

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if needed. The uncertainties were so great that the Navy's initial requirement for realiability would have been extremely costly.

Hodges' paper is a very welcome addition to the literature.

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Comment

Albert Madansky

The best way to referee a mathematics paper is to read only the statement of the theorem and then proceed along the lines of the flow chart given in Figure 1. The process of reading the entire paper through and checking its work in detail is clearly a second-best approach to refereeing. In the same vein, the best way to read a paper whose title is "Uncertainty, Policy Analysis, and Statistics" is to stop at the title and try to construct the list of questions that a paper with such a title should answer. And this I did.

My first question was: "Why is the area of policy analysis different from all other areas in which statistics is applied?"

I next speculated on how the word "statistics" in the title is to be used—to denote "things statisticians know" (i.e., the corpus of knowledge classified by *Mathematical Reviews* into category 62) or "things statisticians do?" And if the latter, when is it that the statistician crosses the invisible line between "doing statistics" and "doing something else?" Indeed, how is that invisible line defined? Finally, who today is classifiable as a statistician, now that our profession and the computer revolution have jointly made our wares as available as over the counter nonprescription drugs?

As to that word "uncertainty," I mused about whether Hodges is referring to the Knightian use of the term, as contrasted with "risk," to distinguish between subjective and objective probability? Or does he have a different use for that well-worn term?

Consistent with my paradigm for mathematical refereeing, I did not pass the title page until I had constructed answers to these questions, after which I dived into the paper. To my surprise I found none of my questions answered. Instead I found yet another list of the steps in the process that a statistician goes through when dealing with an applied problem, along

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