studies should be criticized "on the grounds that they take insufficient account for structural uncertainty." But does that mean we should drop our tools, as the quote from Freedman suggests we do, and pass the buck completely? Doesn't a statistician have something to contribute even to the Freedman-recommended "ad hoc analysis by experts?" I am sorry if my desired for clever turn of phrase, wherein I refer to Freedman as the Neturei Karta of statistics, conveyed to Hodges the caricature characterization of Freedman's position as merely that of a defender of our discipline's virtue. I hoped to engage the reader to think about the more pressing issue, whether or not a statistician qua statistician has a role in (if you will) policy analysis when "the basic theory is incomplete or the data sparse." And I was hard put to pin down Hodges' position on this issue in this paper.

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Comment

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On the one hand, this paper claims that, both in theory and in practice, statisticians currently fail to acknowledge and incorporate important aspects of uncertainty in their modeling and analysis methodology, thus potentially distorting the inference and decision making processes in many areas of application. On the other hand, it claims that the subjectivist approach of de Finetti provides the most promising general framework for developing a language and methodology that might overcome the defects of current approaches. I am entirely in agreement with these views and therefore naturally welcome Hodges' paper, both in its own right and as a focus for a general discussion of the issues raised.

However, the structuring of the paper left me a little unclear as to what particular emphasis was intended in various of its sections. Sometimes, the emphasis seemed to be on drawing a pragmatic boundary between those problems and activities that can and cannot be approached by using some kind of more-or-less formal statistical modeling and analysis. At other times, the emphasis seemed to be on drawing attention to the unique merits of the Bayesian approach in providing a natural and unified framework for the development of precisely those tools that Hodges seems to consider so desirable, including predictive forms of uncertainty statements and between- as well as within-model uncertainty evaluations, both as outputs in themselves and as the basis for sensitivity analysis. Policy analysis applications seemed to fall somewhat between these two tools. Were we supposed to see policy analysis as an archetypal area where the boundary problem is particularly acute? Or as an archetypal area where Bayesian methods particularly come into their own? I fully realize that Hodges is attempting a grand overview of a large number of conceptual and practical problems that are all too rarely discussed together, but I would welcome some clarification from him of the main messages he was hoping we would extract from all this.

What I certainly do recognize from Hodges' running example and his general discussion is the total inadequacy of any view of modeling and analysis that does not appreciate the sociologic and institutional dimensions of dealing with large, messy systems in large, messy organizations. In an unpublished joint study undertaken for a major government agency in the United Kingdom, Dr. Ray Paul, of the London School of Economics, and I considered similar broad issues of model building and validation in representing and summarizing uncertainties in the context of very large scale problems. I shall briefly describe some of our general perceptions and conclusions and would very much welcome Hodges' views as to whether and to what extent we are thinking along the same lines. A

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closely related view is set out in Landry, Malouin and Oral (1983).

Model development and use typically involves a progression through five broad stages: perceived problem situation, conceptual model, formal model, technical solution and summary output of some kind. In this process, the problem situation is defined and elaborated by the perceptions and behavior of a number of participants, who may include subject matter experts and their administrative or managerial overseers, as well as statisticians and other quantitative analysts. The conceptual model that emerges is a broad-brush mental image of the problem situation, corresponding to the participants' identification of the features to be included, their interrelationships, the level of detail of required description and the form of inference or decision required. The formal model then consists of the translation of this conceptual model into a formal language, typically mathematics, usually converted into some form of computer code. The technical solution adopted will depend on the model and the form of output required, but will typically be a combination of algebraic manipulations, analytic approximations and numerical algorithms. The output, which may include both numerical and graphic summaries, might be directed toward an immediate decision problem or might provide a variety of conditional uncertainty statements directed more toward diagnostics and sensitivity analysis and a further iteration of the modeling and analysis process. In addition, it should be recognized that this entire process is predicated on three types of data base, mental, written and numerical, and that in major studies, which span long time periods, there might well be changes in the participants, their perceptions and the relevance of some or all of the data bases.

Taking the above as a schematic for the various processes involved in dealing with large, messy problems, there are a number of points at which steps could and should be taken to increase participants' confidence in the modeling, analysis and reporting by systematically heightening awareness about the uncertainties implicit in these processes. In particular, such awareness is needed in respect of the uncertainties involved in arriving at a conceptual model of the problem, the potential distortions involved in passing from the conceptual to a formal model, the static and dynamic relevance, integrity and coherence of the data bases, the appropriateness and adequacy of the technical machinery applied in order to obtain answers and, finally, the selection of a form of output, which is both practically useful and intellectually honest in its incorporation of the necessary conditional caveats and sensitivity studies.

It seems to me that this kind of analysis corresponds closely to Hodges' plea for a less narrowly based approach to uncertainty on the part of statisticians. However, Paul and I drew one strong conclusion from all this, which Hodges does not seem to emphasize to the same extent, namely, that the quantitative analyst cannot and should not be acting in splendid technical isolation in such studies, but instead should form part of a team, whose members represent a range of substantive and statistical expertise and liase closely and regularly on the basis of informed mutual respect. It is here that sociologic, institutional and, of course, budgetary, constraints inevitably enter the picture.

So far as Hodges' advocacy of the multimodel Bayesian approach is concerned, this seems to me an excellent way of dealing with the intra- and interpersonal hesitations and disagreements, which will doubtless then result in a team context from the systematic heightening of awareness discussed above. In addition to the references cited, he will find further support for this general approach in Dickey (1973) and Smith (1978, 1984, 1986).

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