

ACKNOWLEDGMENT

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ADDITIONAL REFERENCE

HAMMOND, K. R., ANDERSON, B. F., SUTHERLAND, J. and MARVIN, B. (1984). Improving scientists' judgments of risk. *Risk Anal.* 4 69-78.

Rejoinder

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We are grateful to the editor, Professor Morris H. DeGroot, for taking an active interest in our manuscript and for organizing the discussion. We are sure that workers in this area will be equally grateful to all the discussants for sharing their thoughts and shedding some additional light on the murky, multifaceted problem of aggregating expert judgments. Their comments add welcome dimensions to our survey and demonstrate that there is yet no consensus about how to reach a consensus.

The most serious difference of opinion occurs between Professor Shafer, who argues against the Bayesian paradigm for groups of all sizes including $n = 1$, and the remaining discussants who support and focus on the Bayesian approach with qualification. Among the Bayesians, Drs. Winkler, Morris, and Hogarth regard an elicited subjective probability as a measurement or as information which can be aggregated through a suitable supra Bayesian approach. In fact, all three appear to favor this approach even in situations where no natural choice exists for the supra Bayesian. Professor French, on the other hand, sides with de Finetti's completely personalistic view of probability. The latter suggests that interpersonal comparability of probabilities may not be possible, in which case it is not clear whether the supra Bayesian aggregation of elicited opinions would ever be meaningful to those who support this viewpoint.

In our opinion, Bayesian methods provide the sole normatively acceptable answer to the aggregation problem when the group reports to a third party. The Bayesian solution may not be so useful, however, in situations where the group as a whole is seeking a consensus or wishes to summarize its opinions for the benefit of others "at the end of the day." We regard this problem as one of fundamental importance in this area. Solving it would bring us one step closer to finding a middle ground better suited to modern science, between the classical notion of objectivity through replicability and the frequency theory of statistics on the one hand, and the entirely subjective theory of the Bayesians on the other. This issue is

recognized by French, but he fails to see the relevance of axiomatic approaches to its resolution. Interestingly, the search for consensus, in its epistemological sense of unanimous agreement, has aroused a great deal of interest in philosophy, but it has been largely ignored by statisticians, who tend to take an "operations research" view of the whole subject matter. In the discussion, only Winkler is willing to admit the need to tentatively consider axiom-based formulas, and he does so only because "the modeling approach may be difficult to apply in actual situations."

The challenge of the theory as it stands is that it is not always clear when a given situation calls for compromise, summarization, or consensualization. In this regard, Hogarth's recommendation that combination of opinion could be guided by the decision context would seem to be a useful observation. Confusion in the objectives of the theory derives in part from the context-dependent meaning of such words as "consensus" or "opinion pool." We are only beginning to recognize that there is more than one consensus problem. It is not surprising, therefore, that there should be "no single combining procedure for all seasons," as Winkler put it. What is surprising, however, is to see French and Morris seize on the supra Bayesian paradigm as a way of specifying the objectives of any problem. Morris goes even further in suggesting that we adopt this point of view as a way of evaluating the relative merits of prospective pooling methods or formulas. Although it would be legitimate for an individual to evaluate a group procedure on these grounds, this consideration would be irrelevant to the value of the procedure for the group as a whole.

Let us now turn to some of the more technical issues which were raised in the discussion. We begin with Professor Shafer's criticism, which focuses on the deficiencies of the Bayesian (and hence the supra Bayesian) approach. In our paper, we acknowledge that the supra Bayesian approach will inherit all the criticisms of the Bayesian philosophy. The value of Bayesian versus nonBayesian statistics has been and is still the object of a vigorous debate in the statistical