

Does it Make Sense to be an “Objective Bayesian”? (Comment on Articles by Berger and by Goldstein)

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Abstract. The subjective-objective dialogue between Goldstein (2006) and Berger (2006) lays out strong cases for what seem to be two schools of Bayesian thought. But a closer look suggests to me that while both authors address the pragmatics of their approaches, only one qualifies as a school of thought. In these comments I address briefly seven dimensions: the history of Bayesian thought, the different roles for a Bayesian approach, the subjectivity of scientists and the illusion of objectivity, the subjectivity of the likelihood function, the difficulty in separating likelihood from prior, pragmatism, and the fruitless search for the objective prior.

Keywords: Dutch book, Holy Grail, Normative theory, Subjective likelihood.

It is a distinct pleasure to comment on two thought provoking papers, by Goldstein (2006) and Berger (2006), which in one form or another ask fundamental questions about the Bayesian enterprise, not just in theory but in practice. I offer some thoughts on aspects of their arguments, rooted in part in the historical development of Bayesian inference—e.g., see Fienberg (2006).

History of Bayesian Thought. I need to begin by amending Berger’s history account somewhat. It is true that one can trace the roots of objective Bayesian ideas to Bayes and Laplace. And while both gave arguments for prior distributions that carried limited information, they did so from different perspectives and neither pushed for their choice of priors to meet the demands of objective science. Moreover, Laplace subscribed to a subjective definition of probability, as the degree of belief in a proposition. Perhaps we should not glory in their “objective” priors but take note of the fact that it took others many years later to enrich the inverse probability approach, such as Edgeworth and Karl Pearson, who argued for priors based on experience. Objective probability ideas only emerged in mid-nineteenth century but did not take hold in statistics until the creation of statistical tools to rival inverse probability in the twentieth century.

The Bayesian Approach. What do we mean by the Bayesian approach? Both Goldstein and Berger talk about Bayes’ Theorem for combining priors and likelihoods, but they tend to move back and forth among three perspectives: normative, descriptive, and prescriptive. I leave the pragmatic question about statistical practice to the end.

The *normative* perspective is about the theory, and we all need to acknowledge that

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there is a coherent foundation for subjective probability that integrates it with utility. One of the crowning achievements of twentieth century statistics is the axiomatic foundation of subjective probability and utility, born of the efforts of Ramsay, de Finetti, and Savage. There is no comparable foundation for objective Bayes; there is no acceptable normative theory. The *descriptive* is about how people actually reason; after the work of Kahneman and Tversky we know that they are neither objective or subjective Bayesians. Finally, the *prescriptive* approach is what we try to tell others to do in practice; its adoption is the goal of the advocates of both camps.

A principal problem with the normative theory when it comes to prescription is that the ultimate aspects of likelihood (model) and prior revision require us to step outside the normative framework, and thus are not coherent. Mixtures and model search within the standard Bayesian paradigm are rarely sufficient, c.f. Lindley (2005).

Subjectivity of Scientists. Berger invokes the myth of the objective scientist. He and we know better. For many illustrations of counter-examples among the great scientists in history, see Press and Tanur (2001).

Subjectivity of the Likelihood. There are two principal parts to Bayesian formalism using Bayes' Theorem: the prior and the likelihood. Much of the dialogue focuses on the prior, but I want to argue that both are important and that both are subjective—not just the subjective likelihood of Lavine et al. (2006), where a descriptive subjective version of the posterior is used to induce a likelihood function. The focus on the robustness of prior specification, linked to objective Bayesian school of thought, is often misplaced. Statisticians need to focus more on likelihood and features and implications of the hierarchical structures they employ. There is a considerable degree of arbitrariness of what is likelihood and what is prior in many problems—e.g., see Bayarri et al. (1988) and Fienberg (1990).

Pragmatism. Both Berger and Goldstein argue that their approach works when it comes to practical implementation. I agree that pragmatics are important, and here the normative subjective theory is important but not sufficient. The question for both the objective and the subjective approach is “How well do they work in practice?” To my thinking, this depends on the quality of the data being analyzed (the statistician can help in the design here) and the care taken with the modeling of both the likelihood and the prior. In some settings, such as the NBC Election Night model of the 1960s and the 1970s (which used a fully Bayesian hierarchical model), considering alternative priors is important. For the NBC Election Night implementation, this meant multiple priors based on past elections to choose from in real time, and the choice was often crucial in close elections. But for many other situations, such as in the Mosteller and Wallace (1964) analysis of the Federalist papers, what matters is the likelihood. For the Federalist analyses, it was important to learn how the posterior odds for several papers shifted when Mosteller and Wallace used a negative binomial model instead of a Poisson one for word counts.

Some argue for frequentist measures like calibration, but as [DeGroot and Fienberg \(1983\)](#) make clear, calibration alone is not sufficient and not even always desirable. Combining calibration with refinement through probability scoring can produce far better probability assessments and better science and policy, without worrying about the long run, which never quite happens.

Objective Bayes is Like the Search for the Holy Grail. [Good \(1972\)](#) once wrote that there are “46,656 Varieties of Bayesians,” a number he admitted exceeded the number of professional statisticians at the time. Today there seem to be at least this many varieties of objective Bayesians, with each seeking out his or her own method for arriving at the perfect objective prior and then allowing for other idiosyncrasies. Each method fails in some important way precisely because of the lack of true normative foundations. For example, [Eaton and Freedman \(2004\)](#) give a Dutch book argument against the use of Jeffreys’ prior for the normal covariance matrix. I have criticized intrinsic priors for contingency tables both because of their dependence on the likelihood function and because they appear to have bizarre properties when extended to deal with large sparse tables. To me, the search for the objective prior is like the search for the Holy Grail in Christian myth. The goal is elusive, the exercise is fruitless, and the enterprise is ultimately diverting our energies from *doing* quality statistics, c.f. [Chapman et al. \(1975\)](#).

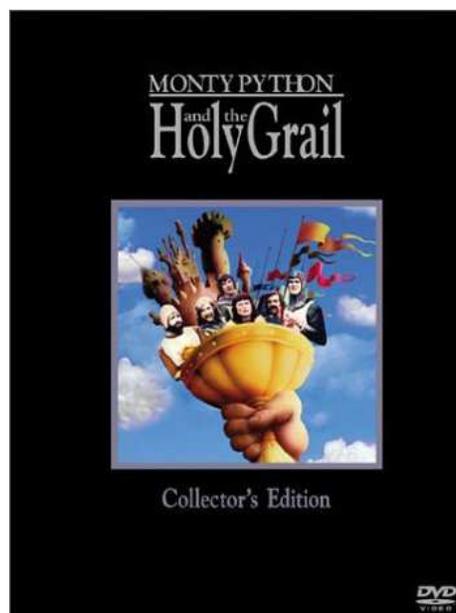


Figure 1: In Search of the Holy Grail. Source: [Chapman et al. \(1975\)](#)

Conclusion. The bottom line for me involves drawing upon a mix of pragmatism and principle. While I'm sure that, in the hands of some Bayesians like Berger, objective Bayes is a sensible approach, I'm equally convinced that a full subjective approach can win out most of the time. This belief, coupled with the fact that subjective Bayes wins hands down on normative grounds, helps *me* answer the question in the title of this note: "No, it does not make sense for *me* to be an 'Objective Bayesian'!" You will need to answer the question for yourself. But if Berger's argument is really that being an objective Bayesian is superior to being a frequentist, I agree fully on pragmatic grounds.

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