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Rejoinder

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The three discussants have offered three complementary perspectives on the material in my paper and in different ways help to sharpen the focus on the appropriateness and utility of the Bayesian perspective in government and policy settings. I am indebted to them for their comments and critiques, which by and large remain couched in compliments, for which I also thank them!

I did consider responding using a variation on Alan Zaslavsky's clever culinary metaphor. But it would be difficult to match him tit for tat as he was even able to adapt Jimmie Savage's (1961) oft-repeated remark that the Fisherian fiducial school's approach was "a bold attempt to make the Bayesian omelet without breaking the Bayesian eggs," to apply to some modern frequentists who borrow from Bayesian ideas. In the end, I decided to simply offer a few observations of why I think so much has changed over the past 50 years, with the hope that these might explain why I differ with a number of the comments from the discussants.

My education as a statistician goes back to the early 1960s when the number of people expressing strong Bayesian perspectives could fit in a small seminar room at a university, and we often did so as part of the Seminar in Bayesian Econometrics that the late Arnold Zellner convened twice a year. Applications in those days typically meant small-scale numerical illustrations using conjugate priors for analytical convenience, and Bayesian approaches were rarely taught in statistical courses except for at a handful of places, and then only to graduate students. The towering achievement of Mosteller and Wallace (1964) in bringing a systematic Bayesian approach to the analysis of the Federalist Papers thus served as an eye-opener to the statistical community and showed that Bayesians could do serious substantive applications that harnessed the power of the largest computers of the time. For some insights into their effort I recommend Chapter 4 of Mosteller's 2010 posthumously-published autobiography on this work.

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For most of today's readers of *Statistical Science*, it may be hard to imagine the almost complete dominance of the frequentist perspective in our journals and in application fifty years ago. It was in part for this reason that I began my examples with some details on the NBC Election Night Forecasting team from the 1960s because it too was an anomaly. On the other hand, something that was true in the 1960s, as it is today, was that most statistical education and research was built around statistical models and inference from them. The principal departure from this model-based perspective came in the area of sample surveys, where essentially the only source of random variation considered by authors and practitioners was that associated with the random selection of the sample and this then provided the basis for inference about population quantities—what we now describe as design-based inference. This perspective was so deeply embedded in the operations of national statistical agencies that it still remains through to today. I remember making a presentation in the late 1970s at a sample survey symposium on why one should view surveys on crime victimization in the context of longitudinal models for individual respondents and households, in which I criticized the narrow cross-sectional perspective adopted by the U.S. Census Bureau in its work on the National Crime Survey (which was in fact a longitudinal survey but not analyzed as such). My remarks were barely completed when Morris Hansen, who was seated in the front row, stood and took me to task because I did not understand the limitation of my perspective and the fact that government agencies understood the limitations of the data they collected and why models had no place in their analysis.

Even in the 1950s and 1960s, frequentists were being influenced by Bayesian ideas, and Charles Stein's results on shrinkage estimation, which were later adapted in the form of empirical Bayesian estimation by Efron and Morris (1973), drew heavily on the form of Bayesian weighting of sample quantities with prior ones, albeit with a frequentist outcome in mind. Several of us taught this Bayesian motivation to students at the University of Chicago, where I was a faculty member from 1968 to 1972, and I suspect this may have indirectly influenced Bob Fay, who was my undergraduate advisee and who later co-authored with Roger Herriot their landmark paper on small area estimation (Fay and Herriot, 1979).

The foregoing is a somewhat longwinded way of explaining why, to paraphrase the Virginia Slims commercial from the 1960s, "we've come a long way, baby," Graham Kalton's protestations to the contrary. When I first visited the Census Bureau, shortly after my exchange with Morris Hansen, few of the statisticians could even understand my ideas on log-linear models and their relevance to census activities. This has changed quite markedly, although many in the agencies have strong training in statistical theory and methodology, the remains resistance to explaining the model-based and often Bayesian motivation of approaches being advocated, even though there is internal recognition of this strong influence. This is especially true in the context of post-enumeration surveys for assessing censal accuracy. Where Kalton and I disagree strongly is on the use of models to analyze and interpret the results of large-scale government social surveys. For example, most of the interesting analyses of the Current Population Survey (CPS), used in the U.S. to produce the monthly unemployment rate, are based on statistical models and on the implicit longitudinal structure of the survey. This is certainly the perspective of most policy analysts outside the government who use the CPS in their work.

Zaslavsky notes that one of the aspects of the objective Bayesian school is its use of Bayes as a device to generate calibrated (frequentist) probability statements. That is clearly a substantial part of the modern literature, but it should play little role in many applications, I believe. Consider disclosure limitation to protect confidentiality in statistical databases. We are surely interested less in protecting an infinite sequence of hypothetic databases generated using the same probabilistic mechanism than we are in protecting the database at hand, once we have collected it. Thus conditioning on the data we have rather than the data we might have had makes eminently more sense to me. If an objective prior at the top of a hierarchical model can succeed in doing this, I certainly have no objections.

Zaslavsky also refers to the practice of Bayesian model averaging. Again this is a place where we do not fully agree. I see Bayesian model averaging as fitting well within the subjective Bayesian paradigm, but primarily for prediction-like problems where different models could conceivably have quite different and possibly non-overlapping specifications. When model averaging is used for inference about parameters in models, however, the results are often nonsensical, because the "same parameter" in different models often has a totally different meaning depending on the rest of the model specification. Regression analysis offers a good example of this phenomenon.

David Hand notes that all models are approximations at best, and both he and Graham Kalton refer to George Box's famous *dictum* that "all models are wrong, but some are useful." I agree and I also agree with Hand that any approach to the analysis of data in practice requires much more than invoking the Bayesian mantra. Statisticians really need to know what they are doing, both substantively and statistically. A Bayesian algorithm does not necessarily make for a good Bayesian analysis and proper inferences. Hand's example of borrowing strength in large sparse tables of adverse reactions in the post-marketing surveillance of drugs harks back to many of the earlier examples of Bayesian ideas and methods I refer to in the paper. I thank him for this and the other examples.

All three discussants caution that Bayesian methods are not the answer to all policy problems. I agree, and I often adopt likelihood-based methods in my own work when a *de novo* Bayesian approach seems forbidding. I remain a subjective Bayesian, however, and no longer see the "threat" of subjective priors as a major obstacle to the adoption of Bayesian methods and analyses.

Does one size fit all? Of course not. But Bayesians come in all stripes and varieties today and they work on diverse applications. I believe we can look forward to the increasing use of Bayesian methods in many domains, including those described in my paper.

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