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Rejoinder

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In writing my essay I presumed I was voicing, with a few novel nuances, a nearly universal attitude among contemporary statistical practitioners—at least among those who had wrestled with the incompatibility of Bayesian and frequentist logic. Then David Madigan collected commentaries from several thoughtful and accomplished statisticians. Not only do I know Andrew Gelman, Steve Goodman, Hal Stern and Rob McCulloch, and respect them deeply, but I would have been inclined to imagine I had been speaking for them successfully. Their remarks shook me from my complacency. While they generally agreed with much of what I had to say, there were several points that would clearly benefit from additional clarification and discussion, including the role of subjectivity in Bayesian inference, the approximate alignment of our theoretical and real worlds, and the utility of p-values. Here I will ignore these specific disagreements and comment further only on the highest-level issues.

We care about our philosophy of statistics, first and foremost, because statistical inference sheds light on an important part of human existence, inductive reasoning, and we want to understand it. Philosophical perspectives are also supposed to guide behavior, in research and in teaching. My polemics focused on teaching, highlighting my discomfort with the use of Figure 3 as the "big picture" of statistical inference. My sense had been that as a principal description of statistical thinking, Figure 3 was widely considered bothersome, but no one had complained publicly. McCulloch agreed zealously. Gelman and Stern, however, dissented; both find much continuing use for the notion that statistics is largely about reasoning from samples to populations. As a matter of classroom effectiveness, I am sure that many instructors can do a great job of conveying essential ideas of statistics using Figure 3. My main point, though, was that introductory courses benefit from emphasizing the abstraction of statistical models—their hypothetical, contingent nature—along with the great utility of this kind of abstraction. As we remarked in Brown and Kass (2009), when Box (1979) said, "All models are wrong, but some are useful," he was expressing a quintessentially statistical attitude. Figure 1 seeks to make Box's sentiment central to statistical pedagogy, and I tried to indicate the way the main idea may be illustrated repeatedly throughout an elementary course.

Recognizing Box's apparent influence here, Goodman then asked whether I was simply restating Box's philosophy, and he further prodded me to show how my own statement of statistical pragmatism could be consequential.

In his 1976 Fisher Lecture, cited by Goodman, Box railed against what he called "mathematicity," meaning theory developed in isolation from practice, and he stressed the iterative nature of model building. The fundamental role of model criticism based on Fisherian logic was emphasized not only by Box but also, in several roughly contemporaneous discussions, by Dempster and by Rubin, and these presumably influenced Gelman and Stern, who, together with their colleague Xiao-Li Meng, developed and studied Bayesian model checking procedures. Importantly, model criticism plays a prominent role in Gelman et al. (2004). The aim of my discussion, however, was somewhat different than what I take Box to have intended. I understand Box to have said that estimation should be Bayesian but criticism frequentist, or inspired by frequentist logic. Statistical pragmatism asserts, more simply and more generally, that both forms of logic have merit, and either can be used for any aspect of scientific inference. In addition, I suggested the commonality of subjunctive statements to help us acknowledge that the big issues, in practice, are not Bayes versus frequentist but rather the effects of various modeling assumptions, and the likely behavior of procedures.

Stern noted that the pragmatism I described "seems to be a fairly evolved state for a statistician; it seems to require a clear understanding of the various competing foundational arguments that have preceded it historically." I agree. Along with Goodman, Stern wondered whether such an eclectic philosophy could influence statistical behavior, especially when tackling unsolved problems. I would claim that it does. I admit, however,

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