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Objective Bayesian Statistics ... Do you buy it? Should we sell it? (Comment on Articles by Berger and by Goldstein)

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The modern conception of scientific method as an objective and value-free learning procedure is the source of the distrust and disrespect shown to science by many people today. In defiance of this conception, I believe the statistical community should openly participate in the development of a revitalised image, accentuating the crucial role of beliefs and values in the conduct of scientific activity. The conception of science as a belief-centered and value-oriented process is supported technically by the operational subjective theory of probability, developed most notably through the stimulus of Bruno de Finetti with subscribers throughout the world. There are undoubtedly difficulties in its application, but I do not think they are insurmountable.

It is to Jim Berger's great credit (Berger, 2006) that he has frankly laid bare the most convincing argument possible in favour of the marketing of "Objective Bayesian Statistics":
1. It is impossible to specify what the promised "objectivity" means (if that even matters).
2. In full voice, the scope of the alluring objectivity must be limited to the narrowest of realms.
3. Even in this realm there is nothing particularly objective about the advertised procedures, which rely on other claims for their touted fame.
4. Yet objectivity is an enchanting image that is required by the majority of proponents of science who do not want to accept personal responsibility for their "scientific inferences."
5. Thus, Bayesian statisticians would do well to standardise our product and market our wares as "objective Bayesian procedures" before someone else expropriates the name ahead of us!

The marketing department has taken over from the production department. The goal is neither product quality nor service, but sales.

I do not believe that the statistics community should provide false pretences of having technical solutions that can satisfy the pitiful whimpers we are enticed to resolve: "I do not want to do a subjective analysis, and hence I will not use Bayesian methodology." Rather, I think the scientific community needs to stand up and say "Pity you! Welcome to the human race."

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From my perspective, the valuable achievements and promise of science are today in grave danger of being ignored, dismissed and unsupported in democratic societies, precisely because of the false image scientists portray about themselves. The more science tries to aggrandise itself as a specialist society of privileged individuals who have access to procedures of knowing that are outside the domain of risky human judgement, the more it will be disdainfully rejected and ridiculed by the common person, who is too well aware that no such magical procedure exists.

The understanding of science and of Nature that the would-be objective Bayesians want us to buy into is a hackneyed mechanistic view of two centuries ago, embellished with probabilistic epicycles that have been required to prop it up. Rather than being generated by mechanistic functions that can be discovered by impersonal procedures of the scientific method, it is proposed that Nature is derived from the machinations of stochastic generating systems. Objective Bayesian methods are touted as improved procedures for estimating the generating model parameters.

In buying into this world view, the proponents of objective Bayesian methods accept far too much that is self-serving for scientists and their unchallengeable entourage of methodologians. We should be honest and progressive in constructing a new image of what science and scientific inference is about, much more in keeping with the really exciting outlook made possible by the scientific insights of the post-mechanistic era.

I would like to make three brief comments on technical matters raised in Berger's promotion of the objective Bayesian method.

The first concerns a misleading promise that the method can give sensible answers to imaginary problems that are needlessly posed in terms of unobserveable probabilities. These supposedly require estimation via data observations generated independently. I refer to Berger's first example (with Mossman) of *Medical diagnosis*. The simple fact of the matter is that medical investigation yields a sequence of pairs ($\mathbf{D}_n$, $\mathbf{H}_n$) $\equiv \{(D_i, H_i)\}_{i=1}^n$, and the evidence they provide is meant to inform us for an assessment of predictive probabilities such as $P(D_{n+1}|H_{n+1},\mathbf{D}_n, \mathbf{H}_n)$, as opposed to $P(D_{n+1}|H_{n+1})$. The well-studied judgement of partial exchangeability yields a complete resolution of this problem in terms of a four-parameter mixture distribution, with four sufficient statistics. See for example, Lad (1996, pp. 229-235), which merely elucidates the analysis presented publicly by de Finetti in 1939. Comparison of an assessed probability conditioned on the data with an assessed probability not conditioned on the data provides a measure of the information contained in the data. This measure is relative to the judgement of partial exchangeability via exchangeablity within groups. There is no statistical measure of information in data out of context of subjective judgements regarding how to assess it. Nor does there exist any "true probability" denoted by $\theta = P(D|+)$ that requires estimation.

The construction of appropriate mixing functions (so-called prior distributions) that represent details of uncertain viewpoints among agreeing or disputing medical parties may well be difficult, but it is surely not impossible, especially with the powerful array of computational procedures we have developed. A mere approximation is better than nothing, and no restriction on functional form need be imposed. Such constructions are necessary if the power of available information is to be assessed. I do not think that a proclamation that inferences based on an arbitrary mixing function are somehow "objective" helps promote scientific endeavour at all. Berger's willingness to relegate the important judgement to regard the data $\{(D_i, H_i)\}_{i=1}^{n}$ partially exchangeably along with (D_{n+1}, H_{n+1}) as a separate matter is most unfortunate. "The relevance of the data" is surely a crucial matter of subjective judgement. When exchangeability is challenged, the balancing of the data information with other considerations for making predictive inference is vitally subjective. Such adjustments should not be suppressed as "unscientific" via specious claims of objectivity.

Finally with respect to this example, I hope only a mention is required to remind us all that the desireable property of the predictive probability $P(D_{n+1}|H_{n+1},\mathbf{D}_n,\mathbf{H}_n)$ is its relevance to *this observation* of D_{n+1} and H_{n+1}, rather than to "coverage properties" of a standardised procedure we might follow in supposed repetitions of the procedure in myriad applications. The substantive problems of confidence intervals remain, no matter how you dress the intervals up in the emperor's objective Bayesian clothing.

My second concern is with the proclaimed reason that Bayesian analysis should just be treated conventionally using certain default procedures: "for instance, when considering tests or models that have differing dimensions, it seems impossible to rigorously define a *best* method of communication, so that we will likely have to settle for conventional methods." In fact, there is a very substantial foundation for statistical analysis of competing scientific claims based on sequential scoring of probability forecasts using proper scoring rules. See for example, Lad (1996, Chs. 6, 8.5). This does not rely in any way on comparability of dimensions of model parameters. The partial exchangeabilities that the modelling portrays require integration over model parameters to yield the desired forecasting distributions for observable quantities. Whether one prefers the richer foundation provided in the work of de Finetti, or the milder form in expositors such as Seymour Geisser, the details are eminently computable; so-called "statistical models" are not real entities that merit being estimated. To the extent that models mean anything, they are models of someone's (some group's) considered uncertain opinion about observable quantities. It is far preferable to focus on our real experiences of recorded history and to assess the validity of competing scientific understandings through their performances in forecasting, than it is to provide conventional procedures for estimating objective Bayesian distributions for unobservable probabilities or non-existing model parameters.

My third comment concerns the fact that so much clarity about uncertainty, scientific observation and scientific inference is given away in succumbing to the demands for objectivity required by the pseudo-scientific practitioners of "data analysis." The concept of exchangeable judgements, for example, provides such a refreshing escape from the impossible conundrums provided by out-of-date and misleading concepts such as independent random variables and causal analysis that only a wholehearted embrace of subjectivist concepts will free us from them.

Particularly galling to me is the notion that "teaching objective Bayesian analysis is also considerably easier than teaching subjective analysis, in that one does not need to teach the difficult subject of elicitation, which requires significant understanding of probability." From my viewpoint, *the most important thing we should be teaching to introductory students is not how to use routine statistical procedures by pushing buttons on a computer, but how to make risky assessments and judgements.*

Some 15 billion years ago, as best we can tell, we were a dense mass of exploding gasses. Today we are as we find ourselves, evolved into our present state of partially-informed being and knowing, but uncertain precisely about all we have been, and uncertain about what we will become. One thing we can be sure of is that there has been no repeatability of stable conditions that provides a real base for "the primitive notion of probability" as something outside of ourselves. Probability is nothing other than the logic of our uncertain judgements about the world. For the statistical community to pretend that as a primitive notion it provides a method for making objective inferences about the world would be both a lie and a shame.

Objective Bayesian Methods? Let the buyer and seller both beware!

References and two suggestions for reading

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