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Comment on Article by Gelman

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We should all be grateful to Andrew Gelman (and to *Bayesian Analysis* editor-in-chief Brad Carlin) for providing us with a new opportunity to discuss polemical issues which affect all of us.

I would like to begin with an important point of agreement with what is stated in the paper: the discussion of computational (mainly Monte Carlo) issues should not be allowed to obscure the need for further analysis of inferential questions. Indeed, we still do not have an overall agreement on the best Bayesian answers to fundamental problems in inference, including apparently basic issues such as point or region estimation or hypothesis testing, for which the demand should be obvious.

That said, I would like to state some selected answers, with some of which the author (who chose not to state his own views) might actually agree. These are all written from an 'objective' Bayesian viewpoint.

1 Multiple methods

I do not think that different methodologies should be used for different problems, but the best available one, very much as one should not use different theories of mechanics to analyze different aspects of the world we live in, but whatever theory seems better suited today. Of course, the details will differ (different modelization techniques, different priors, different loss functions) but one should always present a posterior distribution and, if a decision problem is present, one should minimize some expected loss.

2 Objective priors

In scientific reporting and in public decision making one should indeed avoid purely subjective priors, but this is precisely what 'objective' Bayesian inference (and particularly reference priors) is all about. Here I mean 'objective' in precisely the same way that frequentist methods claim to be objective, in that the final result only depends on the model assumed and the data obtained. It is well known that (typically analytical) reference posterior distributions are available not only for those relatively simple problems for which a formal frequentist solution exists, but also for many others (like hierarchical models) where frequentist methods cannot be applied. Moreover, reference posteriors may be numerically computed when analytical answers cannot be obtained. For a recent appraisal of reference analysis see Berger, Bernardo and Sun (1).

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3 Vague priors, why bother?

Unfortunately, the Ehrenberg quote is an often-stated fallacy. A weak prior (which may be very different from a uniform prior) is the mathematical expression of minimal prior knowledge on the quantity of interest, relative to the information which the data could possibly supply. This prior is required to obtain a posterior, and this is precisely what enables the scientist to make useful inferential statements. I believe it is rather evident that what a scientist wants to claim is that the true value of his quantity of interest lies in a given region with a certain probability, given the particular data he or she has obtained, rather than the frequency with which the procedure, if repeated millions of times, will cover the true value. That said, the coverage properties of objective Bayes credible regions prove that they have the (sometimes exact, always approximate) frequentist coverage that one would expect, thus providing some form of external calibration.

4 Hierarchical modeling

Statistics only really works with somewhat similar elements, and exchangeability of the actual measurements is often only the first step. Indeed, the 50 states are different, like separate hospitals are surely different, but it would be foolish to ignore that they do have some similarities and proceed to an independent analysis of each state or hospital, thereby losing important, relevant information. But of course, this joint analysis (cf. meta-analysis) is well beyond the reach of conventional statistics.

5 Empirical Bayes

Empirical Bayes methods are not Bayesian. Under some conditions, their results may be seen as a possible approximation (as, for instance, when, given a large data set, an unknown nuisance parameter is substituted by an estimate rather than integrating it out). This procedure requires however some stringent conditions to be valid and, even then, one would only obtain an approximation to the appropriate results.

6 Minimal assumptions

Conventional statistics often claim to make minimal assumptions when strong conditions are actually implicitly assumed. For instance, analysis made using only the first two moments of the data implicitly assume multinormality, for otherwise important relevant information would be lost and, yet, those analysis often claim to be general 'model free' techniques.

7 Statistical biases, p-values et al.

Our anti-Bayesian fictional persona should be subject to some re-education using well-known, standard counter-examples. To insist on unbiased techniques may lead to negative (but unbiased) estimates of a variance; the use of p-values in multiple tests may lead to blatant contradictions; conventional 0.95-confidence regions may actually consist of the whole real line. No wonder that mathematicians find it often difficult to believe that conventional statistical methods are a branch of mathematics.

References

[1] Berger, J. O., Bernardo, J. M. and Sun, D. (2008). The formal definition of reference priors. *Annals of Statistics* 36 (to appear) 451