Bayesian Analysis (2014) **9**, Number 1, pp. 23–24

Comment on Article by Rubio and Steel

José M. Bernardo *

An Interesting Problem with Limited Solutions

The authors analyze a very useful model - a three parameters location-scale model with induced skewness - from an "objective" Bayesian viewpoint. As they point out, the model has found many interesting applications in various different applied contexts, and it would be very useful to have a Bayesian objective solution which could be used as a reference, or benchmark.

For some reason, the authors choose to limit their analysis to the use of different variations of Jeffreys priors. I have a number of queries with respect to this particular choice.

Asymptotics. All justifications of Jeffreys priors finally depend on the asymptotic normality of the joint posterior distribution of the parameters of the assumed model, but this asymptotic behaviour is not justified in the paper. Without such a proof, the meaning of the Fisher information matrix is unclear, and the reasons to use some form of Jeffreys prior are less than compelling.

Multivariate Jeffreys prior. Jeffreys multivariate rule, the square root of the determinant of the Fisher information matrix, has never been a good general choice. Jeffreys himself did not really defend its use. Blatantly, in the simplest location-scale problem, the normal $N(x|\mu,\sigma)$ model with both parameters unknown, he suggested instead the use of the right Haar measure $\pi(\mu,\sigma) = \sigma^{-1}$, which is also the reference prior when either μ or σ are the quantities of interest. Even in the case of a resulting proper prior, as in the multinomial model case, Jeffreys multivariate rule is known to lead to very poor inferences. As a matter of fact, I am not aware of a single example where Jeffreys multivariate rule is the more appropriate choice for a joint objective prior.

Product of independent Jeffreys priors. The use of the product of independent Jeffreys priors is an ad hoc alternative to the typically bad behavior of the multivariate Jeffreys rule. This, however, may only be justified with joint asymptotic normality and orthogonal parameterizations, and this is not the case in the model analyzed here. Beyond mathematical simplicity, I cannot see any reason to use them here.

Posterior propriety is not enough. Obviously, an improper prior which may lead to an improper posterior given a minimum size sample cannot ever be accepted. However, propriety of the posterior is not a sufficient condition for the objective posterior to be acceptable. Many other considerations – most importantly the coverage properties of the resulting credible intervals – have to be taken into account before a particular

*Facultad de Matemáticas, Universidad de Valencia, 46100-Burjassot, Valencia, Spain. jose.m.bernardo@uv.es

 DOI:10.1214/14-BA861

objective prior may be recommended for general use. The recommended priors in the paper are not justified from this point of view.

Reference priors. As their own publications prove, the authors are well aware of the existence and attractive properties of reference priors (see e.g., Berger and Bernardo (1992) and references therein for details). I am surprised to see that there is no attempt to derive those, and no justification for this rather surprising omission. I conjecture that the corresponding reference priors, or an overall approximation to them in the sense of Berger et al. (2013), would provide a demonstrably better set of solutions to objective inferences within this model than those offered in this paper.

References

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