

Discussion of “Objective Priors: An Introduction for Frequentists” by M. Ghosh

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The paper by Ghosh provides a useful introduction to the main ideas underlying objective priors and how these ideas might profitably be used by frequentist statisticians, both at a theoretical and practical level. The aspects likely to be of most interest to this group of statisticians are those concerning probability matching, allowing valid frequentist procedures to be derived via a formal Bayesian analysis. But they should also be interested in priors that arise from decision-theoretic considerations, not least since the consideration of risk criteria, such as mean squared error for estimation or operating characteristic function for testing, is ubiquitous in the frequentist approach. As pointed out by the author, at a theoretical level the shrinkage argument, which I have also used extensively in the past, provides a neat way of deriving frequentist asymptotic results.

My discussion will focus on an examination of the main criteria that have been used to obtain objective priors and, partly related to this, the extent to which the theory and practical application can be extended to more complex scenarios. Before launching into this I would just like to comment on the commonly used term “objective” in the present context. As soon becomes apparent in this field, there is an array of possible criteria available for the development of objective priors, some of which depend on a specific choice of parameterization, and there may be no unique solution even for a given criterion. Thus the choice quickly ceases to be purely objective. My own preference is to use the term “nonsub-

jective,” which indicates that the prior is detached from subjective beliefs about parameters but which does not impart such a strong sense of broad agreement as to what the prior should be in any particular case.

1. COMPARISON OF CRITERIA

First, a general point about alternative criteria for the development of objective priors. I have a strong preference for criteria that would lead to the use of properly calibrated subjective priors whenever they are available, so that the consideration of objective priors in some sense generalizes a property of a fully subjective Bayesian approach. In a sense this is true of probability matching since this leads to (approximately) correct coverage of posterior regions in hypothetical repeated sampling. This in turn implies that these regions will also be calibrated over repeated use, as would automatically be the case if a properly elicited subjective prior were to be used. The same cannot be said for moment matching in the sense described in Section 5.2; there seems nothing in this criterion that would lead one to use a subjective prior when available.

Similarly, consideration of a proper scoring rule in a decision-theoretic approach would indicate the use of an elicited subjective prior whenever one is available. As a consequence, I would be uneasy using a decision-theoretic criterion that was not based on a proper scoring rule. For example, it does seem surprising that, even in the scalar parameter case, Jeffreys’ prior turns out not to be optimal under the distance measure (3.13) with $\beta = -1$. The problem is that, unlike the Bernardo criterion that arises when $\beta = 0$ (see later), none of these distance measures corresponds to an average regret based on some primitive loss function that produces a (negative) score when data x are observed and a prior predictive distribution $\pi(x)$ is adopted. So there seems to be no obvious sense in which we would recover a subjective prior distribution whenever one is available.

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Although there is some reference to predictive probability matching in Sections 5 and 6, the paper is largely a review of objective priors obtained via parametric criteria, which usually require a focus on one or more specified parameters of interest. This has certainly been the most popular area of study and, as a technical device for obtaining frequentist procedures, it performs a useful function. However, the focus on parameters is a cause for concern for many Bayesian statisticians. Such approaches normally require a specific choice of parameters of interest, such as in quantile probability matching or the construction of group reference priors. The idea that an analysis should be redone when the spotlight turns to alternative sets of parameters is disturbing. In particular, in complex real-world applications there will potentially be many parametric functions of interest. An alternative to quantile matching is higher-order matching for highest posterior density or other regions, which may not require a specific choice of interest parameters. However, there is an infinite variety of ways in which a region can be chosen. Indeed, in the scalar parameter case, given *any* prior it is possible to choose the region in such a way that higher-order matching is achieved (Severini, 1993; Sweeting, 1999).

An alternative approach is to study the behavior of predictive distributions. This is appealing as the parameterization then becomes irrelevant. Just as in the parametric case one can consider predictive probability matching (Datta, Ghosh and Mukerjee, 2000; Severini, Mukerjee and Ghosh, 2002) and predictive risk (Komaki, 1996; Sweeting, Datta and Ghosh, 2006), and Ghosh has contributed to both of these areas. In the former case the criterion (4.23) is replaced by the following. Let Y be a future observation from the model and let $y(\pi, \alpha)$ denote the $(1 - \alpha)$ -quantile of the predictive distribution of Y based on the prior π . If it is also the case that

$$\text{pr}\{Y > y(\pi, \alpha) | \theta\} = \alpha + O(n^{-r}),$$

then we have predictive probability matching; typically r will be 2 here. In the latter case we can consider the regret when the prior π is adopted and θ is the true parameter value. Adopting the logarithmic scoring rule $-\log \pi(y|x)$, which is the unique local proper scoring rule, this has the general form

$$(1) \quad d_{Y|X}(\theta, \pi) = \text{E}^\theta \left[\log \left\{ \frac{f(Y|X, \theta)}{\pi(Y|X)} \right\} \right].$$

Priors that attempt to control this risk might be considered to be more ‘general purpose’ than priors that require the specification of certain parametric functions.

Having used a sensible broad criterion to obtain a prior, one could then go on to investigate its parametric properties. For example, there may be more than one prior that produces the same (low) predictive risk and the choice between these priors might be made on the basis of a particular interest parameterization. In Examples 1 and 2 of the paper the right Haar prior $\pi(\mu, \sigma) \propto \sigma^{-1}$ is exactly predictive probability matching and also arises as a minimax prior under (1) (Liang and Barron, 2004). We can then see that, for example, it is exactly probability matching when the interest parameter is μ or σ and second-order probability matching when $\theta = \mu/\sigma$ is the interest parameter, as shown in Example 2 (continued).

It is instructive to compare the above predictive risk criterion with the basic reference prior approach of Bernardo (1979, 2005). The reference prior criterion in Section 3.1 is maximization of the Kullback–Leibler divergence between the prior and posterior distributions. As shown by Clarke and Barron (1994), this is equivalent to finding the minimax solution under the regret

$$(2) \quad d_X(\theta, \pi) = \text{E}^\theta \left[\log \left\{ \frac{f(X|\theta)}{\pi(X)} \right\} \right].$$

Note that (2) is based on the proper scoring rule $-\log \pi(x)$. This may be contrasted with (1), which is based on the proper scoring rule $-\log \pi(y|x)$, as suggested by Geisser in his discussion of Bernardo (1979). The former is based on scoring the prior predictive distribution, which is arguably less relevant than the posterior predictive distribution on which the latter is based. We are not so much interested in predicting the data already observed as new data yet to be observed. This distinction is reminiscent of model fitting, where it is the fit to as yet unobserved data that is more relevant than the fit to observed data. Note also that working in terms of the posterior predictive distribution avoids problems of impropriety of the prior, requiring only that $\pi(x) < \infty$. Thus, to continue the discussion of Example 1 in the paper, in contrast to the predictive criterion (1), Jeffreys’ prior emerges as the minimax solution under (2), whereas it is inadmissible under (1).

In more complex examples (1) involves a complicated function that includes components of skewness

and curvature of the model. However, it is argued in Sweeting, Datta and Ghosh (2006) that it is more appropriate to consider the regret

$$(3) \quad d_{Y|X}(\tau, \pi) = \mathbb{E} \left[\log \left\{ \frac{\tau(Y|X)}{\pi(Y|X)} \right\} \right],$$

where the expectation is taken over the joint distribution of X and Y under the prior τ . This is because we are not so much interested in comparing the performance of π with that in a lower-dimensional submodel at a fixed parameter value as comparing its performance with that of other nondegenerate prior distributions for the current model. Moreover, when an elicited prior τ is available criterion (3) will lead us to use this prior. An asymptotic analysis of (3) and the adoption of a minimax criterion, for example, produces sensible priors in specific examples. Another appealing aspect is that the asymptotic predictive criterion does not depend on the amount of prediction.

2. MORE COMPLEX MODELS

Some of the most important and challenging applications of the day, such as environmental science, biomedicine, neuroscience and genomics, demand large, sophisticated and often high-dimensional models. The results in Section 4 of the paper on first- and second-order matching priors are mathematically attractive, but there is clearly a need to explore the extent to which these results can be profitably used in more complex models. As the author points out in Section 6, objective priors have been successfully developed for a number of more complex problems. However, there remains a need for semi-automated procedures so that suitable “safe” default priors can be developed rapidly for arbitrary model structures. Major difficulties include the difficulty or impossibility of obtaining a closed form expression for Fisher’s information and, even if this is possible, of solving the required partial differential equations. Levine and Casella (2003) proposed an algorithm for the implementation of probability matching priors for a single interest parameter in the presence of a single nuisance parameter. However, the implementation requires a substantial amount of computing time. An alternative approach is outlined in Sweeting (2005), where it is shown that suitable data-dependent priors can be developed in some cases. Staicu and Reid (2008) proposed an elegant analytic solution based on higher-order approximation of the marginal posterior distribution. It seems

to me, however, that some form of data-driven approach will be the only viable way to extend probability matching ideas to general frameworks.

Apart from computational difficulties, the major theoretical difficulty of all the approaches to objective prior construction that rely on sample size asymptotics is the potential breakdown of the theory in high-dimensional parameter spaces. In some cases it may be possible to identify directions in the parameter space about which the data are relatively uninformative. This can be conveniently explored, for example, via an eigenanalysis of the observed information matrix. Although the model is high-dimensional, most of the variation of the likelihood may take place on a lower-dimensional manifold of the parameter space. This means, of course, that the model is close to being non-identifiable, which causes difficulties if the parameters themselves are of direct interest. However, this may be amenable to analysis using a predictive approach. If a parameter only enters weakly in the model, then the predictive distribution should not depend critically on the prior chosen for that parameter and asymptotic theory should apply in such cases.

Although versions of probability matching priors and reference priors in nonregular cases have been investigated by Ghosal (1997, 1999) and Berger, Bernardo and Sun (2009), it will be a major challenge to develop multidimensional priors in an automatic way when some aspects of the model are regular and others nonregular.

I suspect that the application of objective priors for high-dimensional problems will be of greater interest to Bayesian than to frequentist statisticians. Given the difficulties of deriving such priors in these cases, the frequentist may well abandon this route and explore alternative simulation-based approaches. On the other hand, a suitable high-dimensional prior is essential for the Bayesian statistician to operate at all. Yet the greater the dimension of the model the less likely it is that reliable prior information will be available on all the parameters, let alone on their mutual dependencies. Furthermore, as noted earlier, it is less likely that there will be just one or two parameters of interest, so I believe that the quest will focus more on the identification of safe, general purpose priors that allow the inclusion of subjective information when available, rather than on priors tailored to specific parameters. If this ambition is realized, then the resulting priors should be thought of as no more than “reference” priors, in

the broad sense of the word, and should not replace the need for sensitivity analysis.

3. SOME OTHER DIFFICULTIES

Many Bayesian statisticians remain sceptical about the need for objective priors to represent ignorance and a common practice is to utilize proper but diffuse priors instead. However, care has to be taken that the tail behavior of such priors is not too thin, otherwise the prior may have the unexpected effect of dominating the likelihood. Consider a random sample from $N(\mu, \sigma^2)$. Suppose that μ and σ^2 are taken to be a priori independent with normal and inverse Gamma distributions, respectively. How diffuse should these distributions be and how sensitive are the results to these choices? Specifically, suppose that $X_i \sim N(\mu, \phi^{-1})$, where ϕ is the precision parameter, and μ, ϕ are a priori independent with $\mu \sim N(0, c^{-1}), \phi \sim \text{Gamma}(a, b)$. Suppose we observe data 529.0, 530.0, 532.0, 533.1, 533.4, 533.6, 533.7, 534.1, 534.8, 535.3. Take $a = b = c = \varepsilon$. What is the effect of the choice of ε ? The value $c = 0.001$ is not small enough: the “noninformative prior” dominates the likelihood and the mean of the marginal posterior of μ is close to zero. Effectively, this happens because the normal tail of the prior for μ is thinner than the Student t -tail of the integrated likelihood of μ . The value $c = 0.0002$ is also not sufficiently small, although if a Gibbs sampler starting near the sample values is run, then it will not detect the problem at all until after a large number of iterations and it will appear from trace plots as if the sampler has converged. A value of c less than 0.0001 is needed for the likelihood to dominate the prior. If we run into such problems in simple models like this, then there has to be a great deal of concern for higher-dimensional models. So objective priors do matter; it is virtually impossible to reliably elicit a high-dimensional prior distribution and there are pitfalls associated with using vague but proper priors.

Yet another difficulty arises when the likelihood does not tend to zero at the boundary of the parameter space. In that case an improper prior may lead to an improper posterior, forcing the use of a proper prior. The objective selection of such a prior is likely to be problematic. An example is the dispersion parameter in a Dirichlet process mixture model. Some authors simply set the hyperparameters in a Gamma prior to be very small, but clearly this requires great care as we know that in the limit we will obtain an improper posterior.

4. CONCLUDING REMARKS

I do think that frequentist interest in Bayesian statistics should be rather more than simply its potential use as a device to obtain valid frequentist procedures. When there is some concern about the priors adopted, Bayesians will often “look over their shoulder” at frequentist properties, if only to check that the prior is not producing some anomalous behavior (cf. Example 3 in the paper). Likewise, frequentist statisticians should find it useful to do the same, possibly to provide an indication that they are not falling seriously foul of the conditionality principle, or possibly to see to what extent their confidence statements have direct probability interpretations. Finally, I would like to thank the author for his interesting review of this area and for stimulating me to think a little more about the basis for the construction of objective priors and the challenges that confront this field of research.

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