Comment on “Constraining climate forecasts: The role of prior assumptions”

J. D. Annan and J. C. Hargreaves

FRCGC/JAMSTEC, Yokohama, Japan

1. Introduction

In an interesting and useful paper, Frame *et al.* [2005] (hereinafter F05) call attention to the way in which alternative formulations of a supposedly uninformative prior can significantly affect the results of Bayesian estimation when observational constraints are weak. In particular, using a uniform prior in climate sensitivity S gives a substantially different result to that obtained using a uniform prior in feedback $\lambda \propto 1/S$. The mathematical aspects of their presentation are clear and convincing. However, we show here that their proposed solution to this problem is inconsistent and inadequate, and it seems implausible that any such simplistic prescription can be considered appropriate for dealing with what is in fact a rather fundamental issue in Bayesian estimation. Moreover, their exploration of the problem perpetuates and highlights a widespread and fundamental con-

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J. D. Annan and J. C. Hargreaves, FRCGC/JAMSTEC, Yokohama, Japan (jdannan@jamstec.go.jp)

fusion which has existed in the literature to date over the presentation and interpretation of observationally-constrained estimates, especially of climate sensitivity.

2. Incoherence

When using Bayes' Theorem, we update a prior pdf $P(X)$ with data X_o using a likelihood function $P(X_o|X)$ to form the posterior $P(X|X_o)$:

$$P(X|X_o) = P(X_o|X)P(X)/P(X_o)$$

where $P(X_o)$ is a normalising constant. In order to do this, a prior distribution $P(X)$ is required. F05 give a simple demonstration of how important the choice of prior may be, by comparing results which are obtained when the prior is chosen to be uniform in sensitivity versus uniform in feedback. As Kriegler [2005] notes, a similar paradox can be found in numerous guises in the climate sciences, such as when considering the atmospheric CO_2 level in 2100 versus the resulting radiative forcing, or ocean diffusion coefficient versus heat uptake.

F05 recommend that "the solution" to this problem is to choose the prior to be uniform in the variable which is being estimated, and they describe the results so generated as "objectively determined". One immediate problem with their proposed approach is that it generates inconsistent results. Stripping away the terminology of climate science may make the point more clearly. Consider a single observation X_o of an unknown but positive definite variable X, which takes the value $X_o = 2$ with an observational uncertainty of 0.5 (assumed to be the standard deviation of a Gaussian deviate). If we wish to estimate X and $Z = X^4$, then F05's proposal is that we should perform these estimations using a uniform prior on each variable in turn, which would generate the results $P(X > 3) = 2.3\%$ and $P(Z > 3^4) = P(X^4 > 3^4) = 7.8\%$.

Clearly, these contradictory results for logically equivalent events cannot possibly both be simultaneously credible. F05's estimates based on equilibrium and transient warming are incompatible in a similar way. It is therefore hard to see how these results can be presented as probabilistic estimates at all, if we interpret a Bayesian probability in the usual way as the degree of belief in a proposition. Their method and results are vulnerable to a Dutch book argument, and thus cannot possibly form a basis for rational decision making.

3. Objectivity and the 'ignorant' prior

F05 claim that the uniform prior U[0,20] for S represents "no knowledge" about S. However, this is easily seen to be false. Their prior actually implies a set of very specific beliefs including for example $P(S > 6) = 70\%$ ("S is likely greater than 6C"), and $P(S > 10) = 10 \times P(2.5 < S < 3.5)$, results which depend strongly on the particular limits that they chose for the bounds of their prior. Not only are these beliefs far from "ignorant", we consider them to be extraordinarily alarmist when compared to the opinions that climate scientists actually have expressed, even when we examine the literature prior to detailed quantitative analysis of recent and historical climate change [eg NAS, 1979]. Of course, their highly alarming prior directly feeds through into the posterior pdf via Bayes' Theorem.

The difficulty of defining suitably uninformative priors for Bayesian estimation has been widely considered [eg Bernardo & Smith, 1994, Chapter 5.4 and 5.6]. Various "reference priors" have been proposed, but although these may have some attractive properties for specific classes of problems, they can hardly be considered as generating objective probabilities for practical applications. Indeed, we must not lose sight of the fact these probabilities are fundamentally subjective in that they do not relate directly to the climate system itself, but rather to our beliefs about it. Moreover, there is no prior that actually does represent "no knowledge". Rather than claiming their particular choice to be the correct approach, it would in our opinion be more reasonable to accept that different scientists may reasonably make different decisions. In our view, Rougier [2006] provides a more firmly-grounded basis for probabilistic inference in climate science.

4. Intractability

A further more technical difficulty with F05's proposal is that it does not provide a solution for any multivariate problem where more than one source of prior uncertainty is present. Indeed in their example, their specific choice of prior for ocean heat capacity is not discussed or justified, merely presented as a (subjective) *fait accompli*. When the parameter(s) of interest are not direct inputs to the model, but instead some nonlinear function of the inputs, then the inversion required to form a uniform prior may well be ill-posed or intractable. It is not at all clear if and how these problems can be addressed, although we note the authors' promise to do so in the future.

5. Interpretation of the results

F05 assert that a user will generally be expecting an answer to the question "what does this study tell me about X, given no knowledge of X before the study was performed?" In the current context, this could equivalently be written as the hypothetical "what would our estimate of climate sensitivity be, if we had no data and knowledge other than that considered by this study?" Even ignoring the previous issue of whether it can be meaningful to talk of "no knowledge", it is important to realise that most readers will actually be rather more interested in an answer to the direct question "what is climate sensitivity?", and we suggest that this is a more useful question to address. In describing their results as "an objectively determined range of uncertainty" F05 actually appear to present their results as an answer to the latter question. However, the answers to the latter two questions could only be equivalent if the study in question used all the information which is relevant to the determination of climate sensitivity. This could be performed via the use of an expert prior, as was done by Forest *et al.* [2002], although care must be taken to avoid the accusation that the prior amounts to the double-counting of data.

This error is illustrated by the manner in which F05 handle the short-term response to the eruption of Mt Pinatubo. Although this may not by itself provide a very tight constraint on climate sensitivity, it certainly provides some evidence in favour of conventional values [Wigley *et al.*, 2005; Yokohata *et al.*, 2005], and this evidence is largely (although perhaps not fully) independent of that already provided by the multidecadal heat balance. Moreover, there are several other eruptions for which we also have observational records of short-term post-eruption cooling. These together strengthen still further the evidence for moderate climate sensitivity, since it is highly unlikely that natural variability would have opposed the forced cooling on every occasion (as high sensitivity would require). If F05 had *combined* this evidence with their earlier estimate based on the recent multidecadal warming, rather than merely considering it as an *alternative* constraint, they would surely have tightened the bounds on their main result and thereby come closer to generating a credible estimate.

For a trivial demonstration of this issue, consider an example in which we have (for simplicity) an agreed uniform prior in X, and an observation, $X_1 = 4 \pm 1$ (again with a Gaussian uncertainty quoted at one standard deviation). We could reasonably conclude from this that $P(X > 6) = 2.3\%$. If a second independent observation, $X_2 = 3 \pm 1.6$ becomes available, then the correct approach is not to consider it as a separate constraint in combination with the uniform prior (in which case we might well conclude that "X_2 provides no useful upper bound on X") but instead to update the previous posterior, in which case we find $P(X > 6) = 0.4\%$.

We note that F05 are not alone in this aspect of their approach. Nevertheless, it seems worthwhile to point out here that any purported pdf which ignores much of the available evidence can hardly represent the belief of an informed scientist, since additional evidence will inevitably reduce the uncertainty. Annan & Hargreaves [2006] and Hegerl *et al.* [2006] have recently presented calculations which demonstrate the practical importance of this issue. As an important corollary, the influence of the prior becomes markedly less important as more sources of evidence are combined and the posterior pdf becomes more concentrated around the maximum likelihood value determined by the data.

6. Conclusion

F05 have made a useful contribution in highlighting the influence of prior assumptions on estimates where data are limited. However, there are substantial problems with their proposed solution. Not only does it generate inconsistent answers which therefore cannot reasonably be interpreted as probabilities in the standard Bayesian context, but it is also incomplete for any multivariate problem, and it promises a false objectivity where none is possible. Furthermore, their work perpetuates the widespread confusion between forming an estimate of climate sensitivity, and merely assessing the value of a particular subset of data in helping to form such an overall estimate.

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