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

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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 (in Section 2) 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

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confusion which has existed in the literature to date over the presentation and interpretation of observationally-constrained estimates, especially of climate sensitivity. We expand on this further in Section 3.

2. The choice of the prior

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, and if we are (or wish to pretend we are) ignorant about X, then some sort of diffuse prior is generally used — for example, a prior which is uniform over a wide support. F05 give an elegant 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". Perhaps the greatest 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 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, $Y = X^2$ and $Z = X^4$, then F05's proposal is that we should perform these estimations using a uniform prior in each variable in turn, which would generate the following results: $P(X > 3) = 2.3\%$, $P(Y > 3^2) = P(X^2 > 3^2) = 3.6\%$, $P(Z > 3^4) = P(X^4 > 3^4) = 7.8\%$.

Clearly, these contradictory results cannot possibly all be valid, let alone the correct "objectively determined" solution. In fact, F05's estimates of climate sensitivity and transient warming must be 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. A further difficulty with their 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) choice they have made.

The difficulty of defining a suitably uninformative prior for Bayesian estimation has been widely discussed and has motivated the development of the field of imprecise probability [eg Walley, 1991]. Various approaches to this problem have been presented but no consensus solution exists. While we cannot survey this subject in any depth here, it does not seem credible for F05 to claim to have provided an objective solution to this general problem. The basic problem is that a uniform prior in climate sensitivity S does not actually represent a state of complete ignorance as to S, but rather represents a very specific belief that for example S is ten times more likely to lie in the range [10C,20C]

than in [2.5C,3.5C]. This also implies a strongly skewed prior belief in $\lambda = 1/S$, for which there does not appear to be any clear basis.

One possibility as to how to sidestep this issue would be to present the results as a likelihood function, without attempting to convert this into a pdf. For example, in the case of the above example, we would find that the likelihood ratio of $P(X_o|X = 3)/P(X_o|X = 2)$ is 13.5%. The obvious benefit of this approach is that this result does not change when presented in terms of $P(X_o|X^2 = 3^2)/P(X_o|X^2 = 2^2)$. Specification of the prior is now clearly the responsibility of the user, and whatever choice he makes, including the new observational evidence will result in some concentration of the posterior around the observed value $X_o = 2$. Such an approach may be useful for other scientists, but it is probably not suitable for more naive end-users who may wish to learn about X but have even less basis than a scientist for selecting a suitable prior. However, this highlights another problematic aspect of F05, which we discuss in the following section.

3. 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?" However, most users will actually be interested in an answer to the direct question "what is climate sensitivity?" and F05 certainly present and discuss their results as if they address this question. The answers to the latter two questions would 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 in Forest *et al.* [2002], although a prior that is traced more directly to other observational evidence might be preferable. Although the answer to the hypothetical conditional version of the question may be of some academic interest, it is unlikely to be of much use outside the immediate field, and surely is of little or no value to policy-makers. We note that equating the answers to both types of question has been widespread in the field [eg Andronova & Schlesinger, 2001]. Although such a simplification may have been defensible when the first few observationally-constrained estimates were being formed based on recent observed warming, this is certainly no longer the case now that numerous diverse lines of evidence have been investigated. Annan & Hargreaves [2006] have recently presented a somewhat simplified calculation which demonstrates the practical importance of this issue.

F05 directly commit this error in their treatment of 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 have observational records of post-eruption cooling, which 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). Adding this evidence to their earlier estimate based on the recent multidecadal warming, rather than

merely considering it as an alternative, would surely tighten the bounds on their main result.

For a trivial demonstration of this issue, consider an example in which we have a known uniform prior in X, and two independent observations, $X_1 = 4 \pm 1$ and $X_2 = 3 \pm 1.6$ (again with Gaussian uncertainties quoted at one standard deviation). If we were to undertake a study to investigate what X_1 tells us about the probability of X exceeding 6, and ignore other data, then we could conclude that "according to this study, $P(X > 6) = 2.3\%$". Considering the second observation on its own generates a broader distribution for X which has a slightly larger probability (3%) of exceeding 6, and so it would also be possible to say that X_2 on its own provides no useful constraint on X. However, it is wrong to conclude that X actually does have such a high probability of exceeding the threshold. In fact, when both observations are combined using Bayes' Theorem together with the uniform prior, we find $P(X > 6) = 0.4\%$. Similarly, claims that climate sensitivity has a substantial probability of exceeding some high threshold such as 6C, based on an analysis that combines a small subset of the observational evidence with an uninformative prior, cannot be considered valid at least in the absence of any clear argument that the all of the unused evidence is literally worthless when considered in addition to that already used. Such arguments have not been presented in the literature to date. 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.

4. Conclusion

F05 have made a useful contribution in highlighting the influence of prior assumptions on estimates where data are limited. However, their 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. As importantly, 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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