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Earth System Dynamics
Discussions

Interactive comment on "Climate sensitivity estimates – sensitivity to radiative forcing time series and observational data" *by* Ragnhild Bieltvedt Skeie et al.

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In my view the article, while in principle suitable for publication by ESD, would be much improved if the following issues were addressed:

1. The information provided on the results is too limited. The median is a more appropriate best estimate measure than the mean for skewed distributions such as that for ECS. That is why the IPCC AR5 report gave medians, but not means, for all the observationally-based ECS estimates that it showed (Figure 10.20b). The medians should be shown, at least for the ECS and TCR posteriors, either instead of or in addition to the means, and likewise given in the Abstract and the main text. It is also slightly

strange, for a Bayesian analysis, that the posterior PDF for ECS is only shown in panel j) of Figure S1.

2. The TCR estimate is of interest to readers as well as the ECS estimate, but it only seems to feature in Figure S2, with no values given. The Main analysis median TCR estimate and its 5-95% uncertainty range should be stated in the main text and, preferably, also in the Abstract.

3. The study uses a subjective Bayesian analysis. The priors used likely have a major influence on the results, but finding out what they are requires referring to both Skeie et al 2014 and Aldrin et al 2012. A wide uniform prior seems to be used for ECS. It is well known that doing so biases ECS estimation upwards and greatly fattens the upper tail of the posterior. (Annan and Hargreaves 2011: "We show that the popular choice of a uniform prior has unacceptable properties and cannot be reasonably considered to generate meaningful and usable results."; Lewis 2014). Using a noninformative joint prior would produce estimates that were at least approximately unbiased, but calculating one could be difficult. Providing results based purely on the joint likelihood function, using a frequentist profile likelihood method, would be a reasonable alternative. If, as seems likely to be the case, the profile likelihood peaks at approximately the same point as the marginal likelihood for ECS (being the mode of the posterior, as a uniform prior for ECS is used) then the maximum likelihood estimate for ECS would be $\sim$1.75 K.

Also, showing what the characteristics of the ECS posterior are when a prior for ECS that is uniform in 1/ECS (and therefore is proportional to 1/ECSˆ2) is used would be helpful. That prior will be close to noninformative. [Given that fractional uncertainty in forcing (RF) is approximately symmetrical (Fig. 3(b)) and dominates that in GMST (and in ocean heat uptake), a uniform prior will be approximately noninformative for 1/ECS, and on a change of variable to ECS a uniform prior becomes 1/ECSˆ2.]

4. The stepwise update results are interesting, but difficult to interpret in the absence

of adequate quantitative information as to the changes in data values and uncertainty ranges involved.

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References

J. D. Annan and J. C. Hargreaves (2011) On the generation and interpretation of probabilistic estimates of climate sensitivity Climatic Change 104, 423–436

N Lewis (2014) Objective Inference for Climate Parameters: Bayesian, Transformation-of-Variables, and Profile Likelihood Approaches. J Climate 27, 7270-7284

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