

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

### **Anonymous Referee #2**

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This paper is an updated analysis of Skeie et al., 2014, using the same simple model and analysis method, but updated radiative forcing (following IPCC AR5), deep ocean data, and updated data to 2014. The paper is very well written and carefully executed. Its a very valuable addition to the literature, and sheds light to the effect of various analysis choices and data uncertainties on the overall ECS result. I also like the discussion of the Johannsen result in comparison to the paper here, which does favour substantially higher ECS values. it is very informative to see this discussion and I find this aspect of the paper particularly valuable.

There are some aspects of the paper that I think could still be improved: a) I find the

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treatment of gaps in observations opaque. I THINK what happens is that the observations are treated as an estimate of global mean temperature i.e. the fully covered simple model is compared to the observations which do contain missing data. This needs to be clarified. It also affects the results of Richardson et al which are as strong as cited only for the case of comparing a fully covered model with gappy data. If accounting for the actual observational coverage by comparing like with like (model limited to same datapoints), the result gets less sensitive which is reasonable. Its not necessary to change the method, but it is necessary to be clear please. If the full model field is compared to the data, it might also be useful to be more explicit about the lack of coverage in rapidly warming high latitudes. For the deep ocean, it seems that the upper 700m are compared in model and data for the upper ocean case only - this could be again said more explicitly

b) prior: while I am not as convinced that the nonanonymous reviewer about the value of an objective prior, I did like that earlier papers of this group showed results using different priors. I think it would be nice to do this here as well in order to illustrate to what extent the prior still matters.

c) the plotted updated Skeie 2014 analysis is different from the published version - I would find it cleaner to also add the originally published range in figure 1.

d) it would be nice to see a bit more about the TCR results here as well.

e) The sensitivity tests are interesting and it would be useful to discuss some of them in the body of the text, particularly the case of efficacy - maybe even give some further ranges in abstract. There is clearly some sensitivity here that isnt accounted for in the main result, so this should be made more clear.

f) it is not quite clear to me how internal climate variability is treated. I assume it is done as in the main Skeie et al., 2014 result, i.e. assuming that internal variability is reflected in the residual and no additional estimate is given. It would be informative to hear how this estimate compares for example to model estimates. It is interesting to

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hear that the updated RF series yields smaller residuals.

g) should we treat the main analysis B as the more reliable result or the total OHC using 4 series case C? its is quite remarkable how much the pdf shifts in the latter case. Is the model good enough to reliably separate upper and lower ocean? there is some good discussion in the paper but I am left undecided about this. A bit more clarity would be helpful.

Small comments: p 3 model section: should this refer here already to using the Andronova and Schlesinger model? also, what are the 7 parameters? this would be very useful to hear what they parameterize and which parameter uncertainties are systematically investigated and which are not. this is probably retrievable from earlier papers but worth reiterating here.

Figure 5 discussion and caption: It would be helpful for a reader to understand what case B,C,D are from the caption - I got it on second but not first reading. (eg averaging rather than separate treatment etc).

In figure 4, I find it slightly confusing that one of the OHC series is systematically outside the estimated 90% range. can this be explained in the text?

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