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Earth System Dynamics Discussions

Interactive comment on "A lower and more constrained estimate of climate sensitivity using updated observations and detailed radiative forcing time series" *by* R. B. Skeie et al.

Anonymous Referee #2

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THis ms estimates the equilibrium climate sensitivity, using a method similar to one published in a precursor paper, Aldrin et al., but applying updated estimates of radiative forcing, and an improved estimate of internal climate variability as well as a combination of data for ocean heat uptake. The ms finds a quite narrow estimate of ECS and TCR, and explores the origin of this tighter constraint. The work is interesting and important, although it does have some weaknesses, some of these need to be at least discussed more clearly in the ms: 1) the statistical method description is very terse - for readers familiar with Aldrin et al it is clear, but it would be better to explain a bit more clearly how the pdfs are derived. Also, can you remind a reader: is the Aldrin method tested using GCM historical runs with known sensitivity? that would be a powerful evaluation

of the method.

2) my major worry is the use of 3 ocean heat uptake datasets within the observational vector. I am not quite sure what this means statistically (doesnt this weight the ocean data more?) and also, I worry that this is interpreted as the three datasets providing three independent samples of the true values - in reality, the three datasets share similar data, and sample the uncertainty in processing only - does the result really reflect this? However, the difference of the 3 OHC data approach to the 1 OHC data approach seems reasonably small, but a clearer explanation as to why the combination of OHC data would increase confidence should be given. The problem is illustrated some by figure 3, where the posterior interval does not include the larger trends in one of the datasets (unless I am missing something) - this strong conclusion, which rejects one of the datasets as far outside the posterior uncertainty, is hard to justify just based on statistics only - or am I missing something?

3) the data model comparison figures in the paper suggest that the model with best fit underestimates the recent warming (in other words, attributes some of it to internal variability), for all temperature datasets for the recent time, and for the change in OHC over time in at least one dataset (figure 3 shows undersimulations). this should be clarified and the uncertainty this highlights should be discussed. Other models, eg CMIP5 models on average reproduce the warming better over much of the period, although less recently - it is intuitively not clear why this paper suggests a better fit with small sensitivity even if this doesnt fully resolve some of the observed trends. I wonder if this finding may be sensitive to model error, such as in the ratio of SH to NH aerosol response, or response time or shape of forcing. To my mind, this suggests a further role of structural model uncertainty in the result - if you agree, it would be good to mention this further uncertainty which would probably widen your pdf some more.

5. Lastly, now that the AR5 is out, it would be good to crosslink to it rather than AR4.

Minor comments: 1. it took me a while to understand that the last sentence in the abstract refers to the main findings range, not a widening of it - maybe clarify.

l 14, p 787: the references to LGM estimates are a bit aged, maybe refer to some newer ones (Schmittner, Hargreaves).

the introduction is well written.

p. 794, l 3: but the temperature response and interhemispheric difference to the same aerosol forcing in models can be quite different, as seen in some of the detection attribution work - this also relates to the hemispheric gradient - so isnt there a source of uncertainty that is not accounted for in this one simple model? if the authors agree, this should be mentioned - model uncertainty might widen the estimated narrow pdf

p 795, l 19-21 has some typoes

p 796 top: doesnt the ipcc report conclude that the recent forcing is smaller (at least over the 15 yrs) than the long term trend because of negative natural forcing? I find these lines hard to reconcile with that can you explain please? Similar, p 798 top paragraph.

p. 799 last paragraph: the carbon feedback is not really included in ECS, as ECS describes the temperature response to CO2 concentrations - please revise.

p. 804 discussion of AMO: other authors (e.g. Booth et al.,. although it is not uncontroversial) discuss that some of the AMO response may be forced. Can you mention this too?

Interactive comment on Earth Syst. Dynam. Discuss., 4, 785, 2013.