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Ocean Science Discussions

Interactive comment on "The transient sensitivity of sea level rise" *by* Aslak Grinsted and Jens Hesselbjerg Christensen

Anonymous Referee #2

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Summary: The manuscripts defined a transient sea-level rise sensitivity as the linear dependency of the rate of sea-level with centennially averaged global mean temperature (surface?) temperature. The authors estimate this sensitivity from observations and from future climate simulations from the CMIP5 model ensemble. They conclude that the model-derived values are smaller than those derived from 'observations' and thus the future sea-level rise may become larger than those projected by climate models.

Recommendation: This is a surprisingly short manuscript, which in my view leaves many technical detailed unclear. It does not have a result section, and so it was for me difficult to interpret what the sole figure 1 and the sole table 1 is actually representing. The very concept of transient sea-level sensitivity requires a much deeper physical

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discussion, My impression is, therefore not positive,. The manuscript seems in many respects to be incomplete.

1) The definition of sea-level climate sensitivity, although used in some previous studies, is at least rather questionable, and it was clearly questioned also in the AR5 report itself. This manuscript should at the very least justify in the first place why this concept is meaningful. For instance global mean sea-level rise is brought about by two very different mechanisms: expansion of the water column and melting of land ice. A back-of-the-envelope calculation yields that the global sea-level rise caused by the capture of an energy flux of 1 w/m2 by the liquid ocean, and its subsequent expansion, is about 1.9 mm. This is very different from the sea-level rise caused by the capture of of 1 w/m2 by land-ice and subsequent melting, assuming the ice is already at 0C, (94mm). Of course, this also depends on where the heat flux is captured and many regional details, but the difference between 1.9mm and 94mm is in principle enormous. Therefore, the very concept of a linear relationship between energy flux imbalance and the rate of global sea-level rise is physically questionable, at least it requires a plausible justification, as the 'sensitivity' depends on the relative contribution of thermal expansion and melting. This contribution is rather uncertain for the future, but it seems to me clear that in the near future melting will play a much bigger role through glacier melting, then perhaps a smaller role as glaciers are completely melted and then again a bigger role when melting in Greenland and Antarctica sets in. So it is really difficult for me to envisage a simple linear relationship to describe this dependency. It may be that in practice it works, but this needs to be justified. Unfortunately, I do not see which data could be used to justify this assumption. The centennial smoothing assumed in this study would require several millennia of data for a robust justification.

2) Related to point 1, the CMIP5 global climate models do not include land ice melting. This is the reason why the IPCC AR5 included a contribution to estimated sea-level rise by expert knowledge. But I wonder how the comparison between AR5 models and observations can be meaningful, when one of the key components is missing in

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the models. Therefore, it is not really surprising that the sensitivity estimated from models is smaller than that estimated from observations. This is again the reason why the IPCC augmented the estimated sea-level rise by 2100 with an approximate contribution from land-ice melting.

3) The approach in this manuscript seems rather similar to the approach by Rahmstorf (2007). The reader would like to know in what aspects both approaches differ, and how this difference my affect the results.

4) I struggle to understand what Figure 1 and Table 1 are exactly showing ? Certainly the caption or the main text should include a much lengthier description. Points that remain unclear to me are: what is the averaging window (100 years as suggested in the main text?) If yes, the global mean temperature observations would be just 1 point ?), What does the point labeled as Sat9 represents ? Probably it represents the data in the satellite era, but there is no mention of this in the main text, only one paper listed in the reference list. The same can be said about TG7. To be honest, at this point I wonder whether the authors have carefully checked the manuscript before submitting.

In the case of observations, if my interpretation is correct, the linear fit is constructed using two points, both with different characteristics(one represents centennial means, the other satellite-era means). Is linear fit with just two points enough to be extrapolated ? The extrapolation would be even more questionable when considering that the physical processes would change over time, as explained in my point 1. How were the uncertainties calculated considering that the errors in each of these data points are different ?

Further points

5) The main text mentions reconstructions of sea-level in the preindustrial period, but were have they been used ? There is no mention of temperature reconstructions that could be used for the estimation of sea-level sensitivity

6) The caption of the table mentions a level of significance in the difference of the sea-level sensitivity. How has it been calculated ?

7) The temperature anomaly are referred to the base line 1986-2005. What is the reason for this short base line, when the link between T and sea-level rate is assumed to be at centennial scales ? It does not seem consistent. I guess there is an explanation for it, but the manuscript is so short and concise that the reader is left wondering

The latter are just examples of open technical questions that should be clear in a properly formatted manuscript, with proper length

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