

## Review Perrette et al. ESD.

This paper treats projections of regional sea level changes. A topic, which deserves further attention, as there are barely papers trying to do so. Nevertheless there are many points, which need further attention as will become clear from this review. I therefore suggest major revisions.

### Major points

This paper treats all different ice melt scenarios as equally likely. This makes a wrong impression on the adaptation and impact community. The current idea is that an ice dynamical contribution of more than 1 m as suggested in figure 2 is fairly possible as there is no evidence for this. I just quote Pfaffer et al. 2008 "Although no physical proof is offered that the velocities given in Table 2 cannot be reached or maintained over century time scales, such behavior lies far beyond the range of observations and at the least should not be adopted as a central working hypothesis"

The way the thermal expansion is calculated is weak, whereas on page 375 it is stated that this is the major improvement of this paper. First of all thermal expansion can be derived directly from the GCM output without the need of the parameterization used here and secondly the authors use the CMIP3 data whereas CMIP5 is already available. So they suggest they use RCP scenarios but as far as I see it they use in fact SRES scenarios. If they would use CMIP5 then there does not remain the need to use MAGICC6, which seems an advantage as there is a strange blend of different models now. As such the paper reads a bit outdated. This also holds for the ice dynamical contribution. There is more around than just the PF08 estimate.

It is unclear to me why the authors use so extensively the possibility to put crucial information in the appendix. This interrupts the flow of the paper. There is no need to reduce the length of the paper in ESD. So I strongly suggest to put more of the vital information in the main text, and reduce the appendix or preferably remove it.

### Minor points

Line 5 the way it is formulated suggests that ocean dynamics are fully accounted for in GCMs. This is not true, as the freshwater feedback from the ice is usually not accounted for. Please rephrase.

Line 19. The question is whether the lack of closure is still manifest given the recently published higher estimates for the contribution of small glacier to the 20<sup>th</sup> century sea level budget. I would suggest not using this as justification of the approach adopted. It makes the paper vulnerable and it is not necessary.

It is also not clear what is meant with "as well as" it suggest that our current understanding of SLR is incomplete because of the large uncertainty in projections. How can the understanding depend on the uncertainty in projections? Please be more accurate!

If you refer to Meehl et al in line 24 it seems fair to be more complete. They did not neglect fast ice dynamics, there are even numbers for it to be worth mentioning, but

they decided not to add them to the other assessed values for good reasons. The paper is also a little outdated in the sense that there are a few papers out which attempt to quantify the ice dynamical contribution.

Page 359, line 8 at many places throughout the manuscript there is reference to Bamber and Riva as if this is the first paper suggesting a gravitational effect from ice mass changes. This is obviously not the case. It is always questionable which is the best paper to refer to, but I suggest using Farrell and Clark 1976, wherever the statement is general. This paper is not even mentioned...

Page 359, line 24. Please provide arguments why your approach is better rather than a loose statement that it is better than previous attempts.

Page 360. It is bluntly stated that GCM are not sufficiently accurate to estimate thermal expansion. This is not my current understanding of the literature. I thought that GCMs are in reasonable agreement with observations on this point. I might be wrong, but in that case it would be beneficial if the authors come up with a couple of good references providing evidence that all GCMs are doing a poor job with respect to the steric component. A little discussion would be needed then to justify that the Ocean Heat Uptake is then still a reliable quantity.

Page 361 You use a scaling coefficient from cumulative ocean heat uptake in combination with MAGICC6. Is there a justification that the scaling coefficient is constant in time and space? This seems not straightforward to me, what about the time scale if tuning period and projection period are not equally long or not as far from equilibrium. The coefficient is derived from thermal expansion; why not use directly thermal expansion from the GCM??

Page 362, line 12. Please provide reference for the value of 1.646.

Page 362 line 17 not very elegant to refer to definition of Tzero two pages ahead.

Page 362 line 18 I would think that the value of 0.43 mm/yr should be different for every model member, but from the text I have the impression that this is not the case. Can you please comment or clarify.

Page 363 line 6, a reference after line 6 would be appropriate.

Page 363 line 10, I think the common practice is that a fingerprint pattern refers to gravitational, rotational and earth dynamics processes. Here the fingerprint is a distribution of Radic and Hock that is something else, please define this differently.

Page 363 line 15-20 Unclear which regions are used shows this somehow in the paper.

Page 363 line 22 which another fingerprint do you have in mind? Show the difference or quantify, now the statement is too vague. I have a feeling what you mean but that is not enough.

Page 363 It is unclear to me why not every GCM ensemble member should have its own distribution for the small glacier contribution. The temperature and precipitation changes are different for every model? How is this accounted for?

Page 364 does show refer to RH11?

Page 364 line 12. Given the reference Rahmstorf 2010 and Rignot 2011 you suggest that the former is the model and the latter the observations. Given that interpretation you argue that there is a discrepancy between process based projections and observations. You can say a lot about the Rahmstorf 2010 estimate, but not that it is a process-based estimate of the ice sheet contribution. So I have no clue what you mean to say here.

Page 364 line 14 I think you mean to say warming of the surrounding ocean, please phrase more carefully.

Page 364 line 22. You have to specify which historical steric and MGIC contributions you use for the subtraction. This might matter a lot given the more recently higher estimates for the MGIC contribution.

Page 365 equation 2. I believe this equation has been criticised in the literature where it has been disputed whether  $a$  is a constant. I do find it not very elegant that there is no attention for the caveats of the use of this equation.

Page 366 I miss the intuition to understand why you define the AR4 estimate as bottom-up approach, maybe you should think about a more intuitive definition of the different scenarios.

Page 366 line 16. Is there evidence for extrapolation of the Rignot et al values for the entire century? Maybe you should reread the formulation as given by Rignot et al. themselves.

Page 366 line 19 What about the Greenland lower bound?

Page 367 line 4. If you use Rignot et al, you have to be consequent and accept that the ice loss accelerates rather than a linear increase. This matters for your 2025 estimates.

Page 367 line 5 and 6. Can be omitted I don't see the relevance.

Page 367 line 12. Rignot et al explicitly state that their results should not be interpreted as such. So it is a bit cherry picking to still use these high estimates. You don't need it to make the story that regional patterns are important.

Page 367 line 18 example of inadequate referencing to BR10, this is general gravitational theory.

Page 368. I doubt whether Pfeffer would agree with the way you use his results. Maybe you should take the chance to check this out.

Page 368 line 13. The distinct path is related to the unrealistic linear trend in time.

Page 369 line 19 You introduce crustal uplift but you only explain this in detail much later.

Page 370 line 5-10. Strange formulation I suppose the crustal uplift in the Netherlands is well known and not uncertain, that is not what the text reflects.

Page 372 line 15 remove reference BR10

Page 373 line 4 CMIP5 runs are available! Outdated information

Page 373 line 12 strange reasoning as GIA has a spatial pattern.

Page 375 novel dynamic fingerprint. This is really the weak part of the story, from a GCM one can directly get expansion to my knowledge.

Page 375 line 13. How do you know it is robust or what do you mean with that phrase?

I left out comments on the supp. Info as I think a reorganization is necessary with more info in the main text.

Figures 1a layout has some problems. Is 1a ocean dynamics or thermal expansion or steric? Quantity "fingerprint" for panel d is odd. (b-c) should be (b-d). panel e-h is confusing one has to read the caption very carefully to understand. Maybe a separate figure would be better.

Figure 2. Explain shading in caption. Not realistic values for upper estimates see major issues and if used than it should be as an acceleration as PF08 clearly state.

Figure 3 hard to see what is in the figure, seems too much info.

Figure 6. I don't understand why the b-panel has barely any differences.