

## Response to Anonymous Referee #2

Reviewer #2 made a number of valid points which we addressed in the revised manuscript, but it also appears that a fundamental aspect of our work was not understood: our projections are designed to be flexible and applied for scenarios which have not yet been run with GCMs, hence the use of the various scaling relationships in that paper. We have proceeded to the following changes to our manuscript:

- Put more focus on the reasons to use MAGICC and scaling relationships instead of direct GCM outputs, in particular by changing the title to: “A scaling approach to project future regional sea-level rise and its uncertainties” (see discussion below)
- Additional analysis with a sample of CMIP5 models (in supplementary material).
- Focus on two scenarios: on a “high” (semi-empirical top-down approach) and “low” (process-based, or component-wise) ice sheet scenario. Do not use the Pfeffer et al. case in the projections.
- Put more material in the main text instead of in the appendix, in particular what is related to the scaling relationships.

Detailed answers below (reviewer’s comments are indicated in *black*, *italic*, our responses in blue).

*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.*

The median sea-level rise projection for the highest emission scenario (RCP 8.5), with the top-down semi-empirical approach yields 106 cm by 2090-2099 (Figure 2, Table 2), from which about half is due to ice-sheets (54 cm) (Table 2). Ice dynamic contribution remains therefore well below 1m even in this extreme scenario.

*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 proposed “upper bound” from Pfeffer is not used as a central working hypothesis, but as an upper bound in our paper, as is clear from the manuscript:

“This scenario is more speculative, especially for AIS, whose ice streams, contrary to GIS’s outlet glaciers, are not constrained by bedrock topography (Pfeffer et al., 2008). It is presented here to cover the full range of anticipated 21st century ice-sheet contribution found in the literature to date.”

To cite Pfeffer:

“More plausible but still accelerated conditions lead to total sea-level rise by 2100 of about 0.8 meter. These roughly constrained scenarios provide a “most likely” starting point for refinements in sea-level forecasts that include ice flow dynamics.”

Median semi-empirical projections are between 75cm and 106cm across the scenarios, which are not in contradiction with the most likely starting value of 0.8 meters and the proposed upper bound of 2m from Pfeffer. Latest developments in physical modeling indicate possible contributions of 15-17 cm from the Greenland ice sheet under idealized forcing (Graversen et al., 2010; Seddik et al., 2012), which is not far from the median range of 22-27cm in our top-down case.

However, considering the latest observations (Moon et al., 2012), which do not support continuously

accelerating outlet glaciers over Greenland, we are willing to concentrate on a “high” (semi-empirical top-down approach) and “low” (process-based or component-wise) ice sheet scenario, and abandon the Pfeffer et al case from our projections.

*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 than 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.*

The comment of the reviewer indicates a fundamental misunderstanding of our approach. Moreover, he confuses emission scenarios and GCMs used to simulate them. We have designed our method to be flexible with respect to the emission scenarios, and to propagate uncertainties from emissions to regional sea-level projections. For that purpose, we use GCMs to derive scaling relationships between climate responses of interest and global mean quantities that can be simulated by MAGICC6 (between ocean dynamics and global mean temperature, and between cumulative ocean heat uptake and global mean thermal expansion, among others). Then, we use global mean projections from MAGICC6 (a fast, simple climate model) in combination with the scaling relationships to project climate responses for scenarios which have not necessarily been simulated by GCMs.

Specifically, simply using the GCM output directly would a) make it impossible to include, e.g., carbon-cycle uncertainties (which is done in MAGICC), their effect on temperatures and hence SLR into our consideration of end-to-end uncertainties and b) investigate additional scenarios which have not been simulated by GCMs. This methodological flexibility is a fundamental point of our paper and this misunderstanding explains many other critical comments from the reviewer. We made that point clearer in the updated version of the manuscript, in particular by changing the title to “A scaling approach to project future regional sea-level rise and its uncertainties”.

More precisely, our scaling with ocean heat uptake to project global mean thermal expansion has a clear physical interpretation (thermal expansion is related to ocean warming), and was shown to be robust with CMIP3 (new figure 2) and CMIP5 (new figure S2) data, and has a clear physical interpretation (thermal expansion is related to ocean warming). Concerning ocean dynamics, the scaling with temperature is less straightforward – but justifiable in the framework of the linear response theory. To address the concerns of the reviewers, and despite the early stage of the CMIP5 archive (there are still data gaps and remaining errors), we performed additional analysis with a sample of CMIP5 models to further investigate the robustness of the scaling relationships.

*This also holds for the ice dynamical contribution. There is more around than just the PF08 estimate.*

This point from the reviewer is valid. We briefly discussed additional literature (Graversen et al., 2010; Hellmer et al., 2012; Price et al., 2011; Pritchard et al., 2012; Seddik et al., 2012). However, a review of the likelihood of various ice sheet scenarios is not the focus of our manuscript and we keep the discussion to a minimum, with appropriate references.

*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.*

The same considerations of fluidity pushed us to leave material in the appendix, but we agree that more material could be put in the main text, in particular concerning the scaling relationships. We modified the manuscript accordingly.

**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.*

**We do not mention GCMs at this place of the manuscript. However, we dedicate an entire section to this point in section 5.3 of the manuscript.**

*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 20th 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.*

**We agree with the reviewer and modified the introduction.**

*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!*

**Agreed. Has been rephrased.**

*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.*

**We modified the introduction and do not refer to Meehl in that context anymore.**

*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...*

**This comment is not entirely fair: we also mention Clark and Lingle, 1977 on line 9. We replaced it with Farrell and Clark 1976, and added that reference at other relevant places throughout the manuscript.**

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

**The statement referred to by the reviewer is:**

*“By propagating uncertainties from future greenhouse-gas emissions to global and regional SLR, our approach provides an integrated uncertainty analysis – going beyond previous analyses of model ensembles for the SRES scenarios (Slanger et al., 2011).”*

*which could indeed be made more explicit. Our scaling approach allows for more flexibility in investigating the response to a particular emission scenario (See previous responses in the “major points”). We changed that formulation in the updated manuscript.*

*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.*

**This is a misunderstanding caused by the statement:**

*“The global mean ocean heat uptake is well simulated with the MAGICC6 model (Meinshausen et al., 2011, 2009), however at present the emulation of thermal expansion estimated from GCMs is not sufficiently accurate for the purposes of this study”*

We meant that MAGICC was not doing a good enough job at emulating GCM's thermal expansion output. GCMs, on the other hand, are performing fairly well (e.g. Domingues et al., 2008), that is why we use GCMs to calibrate the cumulative ocean heat uptake to global mean thermal expansion relationship. Obviously, that was not sufficiently clear. The ambiguity was removed in the updated manuscript.

*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??*

It seems that our methodology was not understood. What the reviewer refers to as "thermal expansion" (which is apparently meant for steric expansion, in other words thermal + haline (steric) expansion), is simulated in two steps in our approach:

- 1) The global mean thermal expansion is used by scaling MAGICC6's cumulative ocean heat uptake (see method part and old figure S1/new figures 1,2,S1,S2)
- 2) Ocean dynamics (which can indeed mostly be approximated by the local steric expansion minus the global mean thermal expansion (salinity has a negligible global effect)) is obtained by deriving patterns from GCM, with local scaling coefficients in unit of meter per degree C. These patterns (maps of scaling coefficients) are derived from linear regression of the GCM local sea level outputs against global mean temperature, at each grid cell.

This was indicated in our manuscript in section 2.1.2:

"For each GCM, we derive a pattern of SLR with units of meters SLR per degree of global mean surface warming. These patterns are then randomly combined with MAGICC6 probabilistic global mean temperature (GMT) projections, and added on top of the global mean thermal expansion."

We made our methodology clearer, in particular by summarizing our general approach in introduction to section 2.1. (Steric Sea Level).

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

The reference (Wigley and Raper, 2005) is cited line 8. Has been repeated line 12 for clarity.

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

Tzero was actually defined line 6 of the same page.

*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.*

The value is indeed slightly different for every ensemble member, due to slightly differing MAGICC6's historical temperature simulations (constrained by uncertainty in temperature data) and uncertainty in MGIC model parameters (see eq. 1 and Table 1). The quoted value of 0.43 mm/yr indicates the mean, and the spread (omitted by the reviewer in his comment) is indicated by the uncertainty, which we fully wrote as  $0.43 \pm 0.12$  mm yr<sup>-1</sup> in the text. As indicated in the manuscript, "parameter values and their uncertainty ranges are indicated in Table 1. These are systematically sampled as part of our Monte Carlo approach".

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

Agreed.

*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.*

We have the same definition of a fingerprint as the reviewer. The MGIC fingerprint has been computed for the present study, based on the projected regional MGIC losses from Radic and Hock, and with the same

model as Bamber and Riva. The new version of the manuscript reads as follow:

“[...] We circumvent this limitation by assuming a fixed spatial distribution of the melt, solving the sea-level equation for on a recent regionally-differentiated 21st~century model projection (Radic and Hock, 2011). We therefore created a MGIC gravitational “fingerprint”, which describes the regional sea-level deviations in percent from the global mean MGIC contribution.”

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

We used all regions from Radic and Hock which contribute more than 1 mm SLR by 2100, as stated in our paper (and shown in Figure S6, except for Greenland and Antarctica which are modeled separately):  
“Note that we do not model the 7 regions of RH11 that are projected to individually cause less than 1 mm SLR by 2100, together accounting for about 1 % of the total MGIC contribution” (P. 363, L.19)  
We amended the manuscript with the statement: “An overview of the MGIC regions retained to derive the fingerprint can be found Figure S11 in the Supplement”.

*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.*

We mean the fingerprint in old Figure S5a. Has now been indicated more precisely.

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?

Our approach only uses a single pattern based on the mean from RH11. We have only global MGIC projections, which are then used to scale our fingerprint. A sensitivity analysis w.r.t. to various MGIC distributions is shown in the supplementary (old Figure S5a).

Page 364 does show refer to RH11?

Indeed, will be rephrased.

*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.*

Rahmstorf 2010 was cited here as it discusses that discrepancy. We agree that citing both Rahmstorf (2010) and Rignot et al (2011) is confusing, and could suggest an interpretation like the one the reviewer mentioned. This was not our intention. In the updated version of the manuscript, we changed the introduction of that section and solved this ambiguity.

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

Has been rephrased.

*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.*

Our model is calibrated with global mean sea-level as presented in (Rahmstorf et al., 2011). We subtract projected steric and MGIC projections.

*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.*

We avoided an in-depth discussion of the caveats considering that it was discussed elsewhere, and to go to the essential of our regional projections. We simply mentioned what we considered to be the major caveat of that model:

“One caveat in their application is that the semi-empirical relationships between temperature and sea-level variations is calibrated over a relatively narrow range of global mean temperature variation compared to the projected warming by 2100”

We referred to Rahmstorf et al (2011) where the reader can find more in-depth discussion about the robustness of the model. In particular, statistical aspects and sensitivity of model parameters to various assumptions has been addressed there.

*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.*

The AR4 case is a bottom-up approach in the sense that it aggregates the main contributions to SLR individually, rather than the “top-down” approach in the semi-empirical setting, where the total SLR is estimated first (and in our case disaggregated by subtracting several “known” components afterwards). With the rearrangement of our manuscript around two ice-sheet scenarios (IPCCAR4+/semi-empirical), we believe it has become clearer to the reader.

*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.*

We agree with the reviewer that such extrapolations are more a thought experiment than a projection. They were indicated here only to help quantify what the current trends are, if continued on a century time scale. In the updated version, we do not refer to the extrapolated trends.

*Page 366 line 19 What about the Greenland lower bound?*

Price et al (2011) proposed a minimum contribution from Greenland’s ice dynamics of 6 mm by 2100, only from dynamic adjustment to present-day losses. It is so small that we did not include it in our projections.

*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.*

*We simply dropped the Pfeffer et al. (2008) estimate from our figures and mention it only in the discussion.*

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

The reviewer is contradictory here: we have mentioned this to stress the shortcomings of the Pfeffer approach. However, since we do not use Pfeffer et al scenarios anymore, this statement is not present anymore in the updated manuscript.

*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.*

*Same as above: we do not cite the extrapolated trends anymore.*

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

We meant specifically Fig. 3 of BR10. We added the reference to Fig. 3. We nevertheless complemented the reference with Farrell and Clark (1976).

*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.*

The reviewer does not give specific arguments and just has doubts -- that cannot be rebutted. As explained above, we eventually dropped the Pfeffer et al. (2008) case from our projections.

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

Not relevant anymore, since the Pfeffer case was removed.

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

We implicitly mentioned crustal uplift p. 363, at lines 6 (“elastic deformation of the solid Earth”) and 28 (“elastic uplift of the solid Earth”). We now mention it more explicitly: “elastic crustal uplift” in the same section.

*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.*

We maintain that the GIA contribution for the Netherlands is not well known. The current crustal motion is indeed measured, but it is largely influenced by anthropogenic effects.

*Page 372 line 15 remove reference BR10*

We refer very precisely to Figure 3 of BR10.

*Page 373 line 4 CMIP5 runs are available! Outdated information*

We updated the statement to reflect the current status of the CMIP5 archive and our additional analysis.

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

Our reasoning has not to do with GIA’s spatial pattern. We do not include GIA because the current GIA rate will be the future GIA rate for all practical purposes.

*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.*

See above discussion. We stand to our scaling approach.

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

‘robust’ in the sense of ‘scenario-independent’. This was investigated in the supplementary (Old Fig. S3 and S4, Table S1).

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

We agree to move more information in the main text.

*Figures 1a layout has some problems. Is 1a ocean dynamics or thermal expansion or steric?*

Fig. 1a is ocean dynamics. Fig. 1e is global mean thermal expansion, which is almost the same as global mean steric expansion. We removed the panels e-h in the updated manuscript.

*Quantity “fingerprint” for panel d is odd. (b-c) should be (b-d).*

Indeed, thanks.

*panel e-h is confusing one has to read the caption very carefully to understand. Maybe a separate figure would be better.*

We suppressed subfigures e-h and rearranged the other panels in separate figures.

*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.*

Explanations for shading were omitted: this has been fixed. PF08 has been removed from the figure, as explained above.

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

Has been simplified with a focus on the RCP 4.5 scenario and 2 ice-sheet cases.

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

This is due to the fact the uncertainty in global mean thermal expansion dominates the steric uncertainty, apart in the north Atlantic subpolar gyre, ACC and other regions indicated in the text. A finer color scale would indicate more contrasts.

## References

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