The Cryosphere Discussions

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Comment on tc-2021-348

Anonymous Referee #1

Referee comment on "Antarctic contribution to future sea level from ice shelf basal melt as constrained by ice discharge observations" by Eveline C. van der Linden et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-348-RC1, 2021

In the manuscript, van den Linden et al. use linear response functions from Levermann et al. (2020) which were derived from ice sheet model responses in sea-level contribution to perturbations by uniform sub-shelf melt rate increases. Sea-level projections are updated by using CMIP6 instead of CMIP5 models and by recalibrating the sensitivity of melt rates to ocean temperature changes using observed mass changes. The authors conclude that with the new calibration, sea-level projections are lowered in comparison to LARMIP2 (Levermann et al., 2020) and ISMIP6 (Seroussi et al. 2020; Edwards et al. 2021).

First of all, I want to thank the authors for this well written manuscript which is easy to understand and follow. Unfortunately, I think that the approach presented in the manuscript cannot be applied this way. However, the approach could be interesting and informing further studies, so I suggest two possible modifications that would make it applicable in a methodologically correct way.

Major comment:
The central issue is that the calibration factor gamma, which relates ocean temperature changes at depth to sub-shelf melt rates changes, is fitted over the historic period in the Weddell Sea, Ross Sea and in East Antarctica, all three regions where very little mass gains or losses have been observed (see Rignot et al. 2019; Figure A1 in the manuscript), in particular in comparison to the overall volume of the regions (if the methodology can be applied to the Antarctic Peninsula should also be checked). This makes a sound calibration with changes in ocean temperatures from CMIP6 models over the historic period basically impossible, due to a number of issues: (1) the changes in mass in the respective region might not even be causally linked to ocean forcing but explainable through, e.g., surface mass balance changes; (2) changes in ocean temperatures in CMIP6 models show a wide spread and how close they are to real changes and if they can actually capture subtleties in the historic record that can be linked to the small changes in ice discharge, is questionable.

This means that the calibration factor is fitted between two numbers that are zero or quite small, but have large uncertainties, so that in the end the calibration factor is not really constrained. And this is physically correct, because if there is no enhanced ice discharge due to changes in ocean forcing in the historic record, as for example in the Weddell Sea, the melt sensitivity to ocean forcing cannot be deduced from observations. This problem shows for example in the result that most calibration parameters are zero in many ice-sheet-ocean-model combinations (section 3.1). And that, even if the parameters are fitted to represent past discharge, they largely underestimate the observed mass loss (Fig. 6).

Two suggestions to avoid this problem are:
1) focus the study on the Amundsen Sea (potentially also the AP), where actually enhanced ice discharge has been documented extensively and linked to enahnced ocean-driven melting. This would allow you to derive a sound fit for that region. Then you could compare the projections for the Amundsen Sea with ISMIP6 and LARMIP2 for that region. Alternatively,
2) if you want to include the whole of Antarctica, you could use your proposed method to fit gamma in the Amundsen Sea (and AP), where substantial discharge occurred, and assume an uncertainty distribution of gamma (based on LARMIP, your fit, ISMIP6 calibration,..) for the other regions.

Since these would mean major changes to the manuscript and potentially the central findings, in the following I mostly omitted comments on the methodology and results that will be affected by the major comment above:

Minor comments:

- the study uses "discharge" in some places, but what it actually means is "changes in discharge", as the latter should in principle be associated to changes in sub-shelf melt. Please check and correct.
- page 1, line 24: specify "long-term"
- page 2, line 53-55: ISMIP6 used more than the quadratic calibration, and those were two options for calibration (not done at the same time)
- page 3, line 69, line 85: this is discussed as if the scaling factor in LARMIP only has disadvantages, but it actually has the advantage that also uncertainties in the global mean temperature changes (and not only the CMIP trajectories) were included in the uncertainty estimate
- page 5, line 100; Table 2: the sub-surface temperatures used as ocean forcing look too shallow for me. The relevant water masses are those at depth of the continental shelf that drive the melting close to the grounding lines. I would expect these more around 800 to 1000m depth.
- page 6, line 118-120: why do you not correct for individual model biases but by the ensemble mean?
- Fig. 3 caption: change to "Annual… time series of the CMIP6 multi-model mean (green) , model drift and bias-adjusted, and the GREP ensemble mean (orange). Both are smoothed by a five year running average filter."
- page 7, line 121: this sounds weird, the water is not warmed in the cavities, but it's the change in the pressure that lowers the freezing point and increases the thermal driving
- page 6, line 137: that T_f is a constant for each region is a very coarse assumption, could be discussed in the discussion.

- page 9, line 171-174: why this condition for the “unbounded”? Would you get a better fit for the Amundsen Sea if you would remove this?
- section 3.2: LARMIP2 did not calibrate the melt-factor with observed changes in ice discharge, but it did compare the obtained mass losses over the historic period, which actually look like a much better fit than your results (Fig 6, Levermann et al. 2020).
- section 3.1: do you have an estimate on the uncertainty of your calibrated melt factors for each combination?
- figure 5: instead show the sensitivity in m/a/K, your legend is hard to see, maybe increase the intensity of the colors?
- page 13, line 239: but they should, by construction. This indicated the underlying problem with the methodology.
- discussion: you could add Payne et al. 2021 for a comparison between CMIP5 and CMIP6 effects on AIS sea-level projections; you should discuss the errors that arise through not including surface mass balance changes in your fitting procedure.