

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-348

Anonymous Referee #2

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-RC2>, 2021

Review of van der Linden et al. "Calibration of basal melt on past ice discharge lowers projections of Antarctica's sea level contribution".

General Comments:

In this paper, the authors use linear response functions of ice sheet models from LARMIP-2 to estimate contributions to sea level in three future emissions scenarios. Forcing is derived from ocean conditions in CMIP6 models and is further bias-adjusted and corrected for model drift. Their methodology resembles that of LARMIP-2, however the key difference is that instead of calibrating basal melt rates on observed melt rates (as in ISMIP6), basal melt rates were calibrated on observation-based estimates of ice discharge (Rignot et al., 2019). The authors use two different forms of melt rate parameterization (linear and quadratic) and then use their derived values for gamma to update sea level rise projections under three SSP projections out to 2100. They conclude that their method results in lower projections of Antarctic sea level contribution than the other main methods used in IPCC AR6 (ISMIP6 and LARMIP-2).

I want to thank the authors for this work, and their new approach in calibrating to a different observational dataset than has been typically used in the community so far. They have recognized and tried to address a need to better-constrain gamma values. That said, I have a few broad-brush concerns about the paper.

First, the calibration of basal melt rate is based on past ice discharge in regions (e.g. Ross, Weddell) where very little ice loss was taking place. As a result, gamma values are often equal to zero, and so future contributions to sea level from these regions is inevitably muted despite the potential for actual increases in thermal forcing. My concern here is that it is hard to derive a meaningful value for gamma in regions where there is minimal mass loss, and therefore hard to deduce the actual sensitivity of the region to changes in ocean temperature. The uncertainty around CMIP6 ocean warming coupled with the lack of historical mass loss in some of these key regions makes finding a valid range of gamma very difficult. This problem is manifested when the authors find that ESM-RF pairs underestimate the magnitude of observed Antarctic sea level response despite being tuned to match it. Furthermore, after picking the top 10% of ESM-RF pairs, some regional hindcasts do not capture the observed sea level response (Figs A1 & A2). As a result, I'm concerned that the conclusions drawn about the future contributions to sea level, and their uncertainty analysis, are rendered less credible.

Second (and this is a correctable issue) I felt that the rationale/motivation for doing the calibration on ice discharge rather than basal melt rate was not well-articulated. I think the authors could add stronger language for why this method is worthwhile. This should also be put in the context of ISMIP6 and LARMIP-2 methodologies, and what potential issues calibrating to basal melt rate could create.

Specific Comments:

L41: UK-ESM has included an evolving ice sheet in a GCM framework. And there are other efforts currently in the works in other groups to include an evolving Antarctic ice sheet in a ESM. So not sure what is meant by 'short-term' here I guess.

L69: Please specify why GSAT is a less desirable metric than subsurface temp. Or alternatively, why is it a more useful metric? Easier to derive?

L72: How is future loss consistent with past loss? Please elaborate on pros/cons of ISMIP6 calibration methodology and your methodology here. What makes your useful? Motivation needed.

Table 2: Is this the mean grounding line depth? I believe so, but if it is, shouldn't Amundsen GL depth be closer to ~500m?

L108 – L114: Could rearrange this to start with the motivation first (ocean T bias will affect magnitude of basal melt rate in quadratic estimate, therefore these are the steps we take to deal with it). Currently this feels like you don't know why you're dealing with bias correction until after it is explained.

L118: Why remove the mean bias, which may even have the opposite sign to the model-specific bias? Why not just remove the bias for each model?

L143: Please note why/how this assumption is flawed.

L148-149: Is there wide variability in salinity between far-field and sub-shelf? I.e. Please comment on whether using far-field salinity climatology is a fairly broad assumption, or not? Please discuss. The same goes for T_f too.

L166: consider rephrasing this since but here you say sea level contributions are calibrated on ice discharge, but the rest of the paper states that basal melt rate is calibrated on ice discharge. I understand what you mean here, but this may be confusing for readers.

Section 2.3 in general requires more work to increase clarity for the reader. Please describe the iterative process, how it works. And please expand on the rationale for why to do unbounded vs. bounded methods, as well as the discussion on how the 'unbounded' calibration range is determined. The final paragraph of this section also needs some more clarification.

L138: Please define T_0 . It is not defined.

L227: What calibrated gamma values? All of them? The full range? The median? Mean? This is unclear to me.

L239-242: What is "reasonable extent"? Also, this seems fairly problematic to me. If the ESM-RF pairs cannot capture the magnitude of the observed Antarctic sea level response, even though they are calibrated to do so, what does this say about the methodology? This is assuming that the past conditions and sensitivities hold into the future as well, which may or may not be true, particularly when feedbacks become triggered (e.g. MICI). This is illustrated by the observation that pre-2010 discharge is overestimated, and post-2010 discharge is underestimated.

Figure 6: What gamma is used? Median? Mean? Full range? Is the shaded area intermodal spread, are you using a spread in gamma too or just a singular gamma value?

L407: Please spell out the argument that applies to ISMIP6 gamma-values.

In general I enjoyed the discussion, and thought that plenty of this type of language could have been used as motivation/context in the introduction. Just something to consider.

Minor Comments:

L 7-8: Explicitly state all three SSP scenarios used.

L23-24: include references for model (ice sheet models, I assume?) and geological data being referred to.

L25: Moreover, melt of *Antarctic* land ice...

L32: Using similar methodologies to what? To each other, I assume...?

L32-35: Can you mention why uncertainties are increasing?

L36: Future projections are *always* based on modeling (not often).

L38 & L42: What do you mean "balanced by data from ESMs"? Please clarify.

L39: Is there a citation for claiming ice sheet-ESMs show undesirable/unrealistic trends ?

L46: Used as *one* basis for projections...

L56: In that study, the temperature...

L75-76: What does it mean to arrive at an estimate of future mass loss that is consistent with observed mass loss over the past four decades?

L86: Reference Lambert et al (2021) here would be appropriate.

L91: Please state explicitly what Levermann et al calibrates to.

L97: What does "delayed ice sheet response" mean here?

Table 1: What level does "subsurface ocean temperature" refer to here? I assume it is the mean depth of the grounding line (table 2), but please be clear.

L103: The ocean temperature time series...?

L107: Remove text: "For the quadratic melt parameterization".

L123 & 142: Do *any* CMIP6 models represent cavities?

L126: Table 3 is referenced, but perhaps prematurely. I suggest T3 and T4 are swapped in order.

L138: "See Table 4..." already noted earlier.

Figure 5: Increase font size here please.

L227: "as specified on top" à "as specified in the titles"

L281-285: This text seems to be repeated again later on from L290-295. Please give this another proofread to make sure there is no longer repetition and the flow works well.

L326: "The differences with ISMIP6 and LARMIP-2..." I think you mean difference between this study's results and these two studies, but this phrasing leaves this ambiguous.

Fig11&12: Label the x-axis please. And possibly increase fontsize.

L345: "is computed *using forcing from state-of-the-art...*"

L346: "CMIP6 *applied to* linear response functions..."

L364: Is there an appropriate reference for the comments made about ESM performance?

L373: By climate-state dependent do you mean time-evolving?

Table 7: Please state what the percentages refer to. Also add units.

L376: Please elaborate on the processes you refer to here.

L384: The better prediction of future mass loss you do achieve is for less physically defensible reasons, no?

L386: Elaborate on the advantages and disadvantages.

L395: Therefore, it is interesting...

L434-435: Please state what you mean by 'highest' and 'lowest' methods.

FigA1 & A2: Font sizes of tick labels and axis labels could be increased for better readability.

In general, section headings could be more clear. For example, "Magnitude and Rate" and "Best Estimate" could be more specific to help the reader understand what the section is about.

