We would like to thank the editor and the two anonymous reviewers for their effort to review our manuscript and greatly appreciate their helpful comments for improving our study. We have addressed the raised points and provide replies to all comments below, with our responses indicated in blue.

Editor comments

My minor comments:

- "Ice discharge" is ambiguous, as it may refer to ice discharge at the ice shelf front (calving) or ice discharge across the grounding line (as in Rignot et al., 2019). Please clarify this at the beginning of the manuscript, as well as the link between discharge and sea level.

We added an explanation in the introduction (L. 67-69 & and L. 82-83).

- L. 8-10: there is no verb in this sentence -> are improved ?

Added 'are' (L. 9)

- L. 53-56: it may be worth mentioning Payne et al. (GRL, 2021), which describes the CMIP6 part of ISMIP6.

We added a sentence mentioning Payne et al. (L 61-62).

- "S" is used for both salinity (equ. 4) and sea level (equ. 5, Table 7). Please use different letters or upper/lower case.

Good point. We changed salinity to lower case s.

- L. 165-168: is the uncertainty on Rignot (2019)'s discharge taken into account in the calibration?

It was not included in the calibration since the uncertainty was small compared to the intermodel spread (L. 219-221).

- Tuning the melt rates to get the observed ice discharge is a way to get correct historical sea level trends, which was clearly missing in ISMIP6. However, if the ice sheet models used to derive the response functions are all biased in the same direction, e.g. not sensitive enough to climate perturbations, tuning the melt rates will aim to high-biased melt rates to compensate the low-biased sensitivity. In this case, getting the correct historical ice discharge would not give so much confidence that the response to future warming is correct. I have the same concern with a potential selection of CMIP models based on the observed discharge (as suggested L. 446-448). Please consider discussing this point.
Good point. We added a paragraph on this issue in the discussion section (L. 613-617).

Anonymous referee #1

Review on “Antarctic dynamics contribution to future sea level constrained by ice discharge observations”

Main comment:
From the replies I understand now that you use the historic observations of ice discharge to constrain the melt parameter (e.g., p3 lines 80). There is however one discrepancy in units which you might want to consider - I think you should at least discuss it. In the LARMIP2 paper, the ice sheet modelers were asked to provide the results in units of sea-level equivalent ice loss, which is calculated based on changes in the volume above flotation of the ice sheet. This does not directly compare to changes in ice discharge. In the extreme case, when an ice stream is just about at the flotation limit and very slightly grounded, it could be that its discharge increases and the grounding line retreats, but the sea-level contribution of this is quasi negligible.

We did use the grounding line ice discharge from Rignot et al. (2019), which is defined as “ice discharge by glaciers across the grounding line (where ice becomes afloat in ocean waters and detaches from the bed)” which means that is should in principle be comparable to our LARMIP-2 estimates based on the linear response functions which obtain the sea-level equivalent ice loss from the changes in the volume above flotation of the ice sheet. We have added an explanation in the introduction (L. 67-69 & and L. 82-83) and methodology section (L. 82-83).

Minor comment:
- p 1, line 15-16, and p28, lines 587-589: I do not understand your argumentation so far that your results support this statement. If I understand it correctly, it is based on how your calibrated melt parameters compare to the ISMIP6 median parameter. However, the ISMIP6 experiments also included the PIGL calibrations which is much more sensitive, and this is not included in your argumentation, or? So maybe re-calibrating parameters would also reduce the upper range of the ISMIP6 projections based on this parameter calibration, or am I missing something? Please explain your reasoning for this statement better.

Our comparison is based on the ISMIP6 AntMean method, not on the PIGL method. We only used the PIGL method for comparison to our basal melt sensitivity parameters (Fig. 5). If I understand the ISMIP6 set-up correctly, the AntMean method is used for producing their main results as presented in IPCC AR6. We expanded our explanation in the abstract (L. 14-16), results (453-457) and conclusion (L. 703-712).

- p 2, line 35: that the range of uncertainties appears to be increasing is arguably not because the we know less as implied by this formulation, but because more models and processes are included, i.e., the uncertainties become “visible”

Agree. We added this explanation in the introduction (L. 39-40).

- p 3, line 62: I was a bit surprised by calling this a “melt parameterisation” since in my head this is usually a 2-dimensional field of melt rates, but I think this is fine, maybe add a short explanation to make this clear.

We added an explanation in the methodology section (L. 187-189).

- p 7, line 125: this sentence still sounds weird to me as it is not the water that is changing its temperatures in the cavity.

OK, we removed this sentence as it is not strictly necessary for our methodological explanation.

- equation (4) do you also use the ice shelf cavity mean depth when testing the deeper ocean layers?

We use the depth that we use for the thermal forcing: changed in the text (L. 182-183, 186).

- p 9, line 166: show also the equations over which you are optimizing, this would make it easier to understand what you are describing here

We added the RMSE equation (L. 217).

- p 10, 180-181: give the median values, so that they are somewhere in the manuscript.

We added the median basal melt sensitivity values for ISMIP6 AntMean and LARMIP-2 in the caption of Table 3, and L. 385 and L. 387.

- p 10, section 3.1, please show the discharge curves you use for calibration in the Amundsen Sea and for the whole Antarctic ice sheet

The discharge curves are included in Fig. 6 (‘ice discharge observations’).

- p 10, line 196: if correct, add “..to sea level while CMIP models indicate an increase in ocean forcing,…”

Over the historical period the median of the CMIP6 models does not indicate a clear increase in ocean forcing in all regions (Fig. 4).

- p 10, line 200: add citations that support this attribution

Added citation to Pritchard et al. (2012).

- Fig 5: add % to the numbers in the top of the panels

Done.

- p 13, line 244-246: not sure I understand this sentence, please clarify

We rephrased the text (L295-304).

- p 14, line 255-256: earlier you stated that you include the linear parameterisation for comparison with LARMIP2?

Yes, we make a comparison later with the median basal melt sensitivity from LARMIP2 and therefore use a linear parameterisation. The phrase 'unless specified differently' is a bit vague. We now made it more explicit when the quadratic and linear parameterisation are used (L.312-314).

- p 21, line 384-398: reformulate this argument, you base your reasoning here on a comparison between projections, but we do not know which projection is correct and hence a conclusion about which methodology is better cannot be drawn. Instead you could use papers that support the quadratic relationship (e.g., Holland et al., 2008).

Agree. We included Holland et al. (2008) and Jenkins (2018) to support our argumentation.

- p 24, line 466-467: Please explain more. Which numbers do you compare to conclude this?

We added the standard deviations between the SSP scenarios and basal melt computation methods in the text to quantify the spread and compare it to the spread between ESMs and RFs (L.551-555).

- p 26, line 598: this could also indicate an insensitivity of discharge to basal melt (in the case of no buttressing)

We decided to remove the physical explanation from this sentence as insensitivity is actually not a realistic feature. It simply means that, in some cases, the calibration method is invalid.

- p 26, line 508-514: I am not sure I understand what you mean. Are you basically saying that FRIS cannot be calibrated at the moment?

Yes indeed. We rephrased the sentence so that it becomes clearer (L. 621-622).

- p 26, line 514: it could be misread at the moment that you calibrate with basal melt (not discharge), maybe be clearer here

We rephrased the sentence (L. 628-630).

- p 26: one point that is missing in your discussion is that you consider a constant basal melt rate increase over the entire ice shelf, no spatial patterns and effects are taken into account

We added this point to the discussion (L. 617-619).

- p 26, line 532: “physically correct” – I do not think that you can derive this from your previous reasoning.

Agree. We added citations to support this argument (L. 685).

- p 27, line 559-560: LARMIP2 did not mainly focus on the future, it did compare to historic ice loss and found their projections to be consistent

Although in LARMIP2 a comparison was made with Antarctic mass loss (IMBIE), it was not used as a constraint on the projections. However, since basal melt observations were used as constraint I will remove the comparison with LARMIP2 and ISMIP6 from this sentence.

Anonymous referee #2

My main comment is that, as the title states, the paper aims to deliver Antarctic sea level estimates based on a new methodology. However, the way it reads now, the actual SLR values are not woven through the text (the results exist in figures and tables only). I recommend that the Abstract, Results, and Conclusion sections all explicitly state the actual resulting values. This will improve readability as well since, at the moment, it is easy to get lost in all the methods and which ones lead to higher or lower SLR estimates. An example of this might be in lines 365-368, where the authors discuss the contributions they arrive at as compared to other studies (LARMIP and ISMIP6).

We have added the median sea level contributions of our main projections in the abstract, results and conclusion section.

Another related issue is the Conclusion section. Plenty of this section is repeated information from the Discussion. I think it would be helpful to spend more time in the Conclusion putting the authors’ results in context of other SLR estimates, and again, explicitly stating their new SLR estimates.

We reduced the Conclusion section and removed repeated information. We also added a paragraph in which we compare our results to LARMIP2 and ISMIP6.

Please see below for some other comments that will improve clarity for the reader.

Comments:

Section 2 (Methodology). I think you could use a very brief explanation of the steps in Figure 1 to walk through the steps so the reader knows what to expect in terms of flow before you launch into the details of each step in each subsection that follows.

We added a brief explanation in the methodology section (L. 109-129).

L105: Why do you choose the depth level 800-1000m in addition to the ice shelf base depth? Is there some rationale to choosing that depth? I know you’re trying to see if the depth matters, but is there a good reason to choose this one? Later on, near line 402, you do

mention that this is because this is where relevant water masses that drive melting closer to the grounding line originate, but I'm not sure I exactly follow. At the least a reference is needed here.

We added an explanation. The deeper ocean layer is chosen as it approximately represents the deeper water masses on the continental shelf that have access to the cavities under the ice shelves (L. 144-145).

Section 2.3
It seems to me that the calibration methods should come before the Sea level contribution method. Because 2.2 ends with talking about calibration, then you start into a very short sea level equation, and the back to the calibration. Consider re-organizing some of this for better flow that matches your actual methodological order.

We first address sea level contribution, since it is used in the calibration. However, we have changed the text now so that 2.2 does not end with calibration and it is only mentioned in the section on calibration.

Minor Comments:
Title. Suggesting: "Antarctic contribution to future sea level as constrained by ice discharge observations"

We have removed 'dynamics' as you suggest, but since we do not focus on the total contribution we have adapted it a bit to: "Antarctic contribution to future sea level from ice shelf basal melt as constrained by ice discharge observations'.

L36-37: commas: To address this issue, our study aims to gain more insight in the Antarctic contribution to, and uncertainties in, future sea level …

Added commas.

L66: …over ocean temperature changes as a driver is that uncertainties in GSAT…

Added.

L69: …this step by using subsurface ocean temperature as the driver ….

Added.

L78-79: thereby constraining the basal melt even before the observational period. I think I know what you mean here, but it could be rephrased for clarity.

Rephrased.

L79: As a calibration target, ….

Added comma.

L80 & 82: State the year for the Rignot paper explicitly when citing in-line.

Added year.

L126: Again, I wouldn't say ESM's typically do not represent ice cavities, if none actually do.

Removed 'typically'.

L130 and throughout: italicize in situ

Done

L148: CMIP6 ESMs do not resolve cavities, as far as I know. Clarify that by removing 'typically'

Removed 'typically'.

L188: Do you mean to say the basal melt is computed from subsurface TF anomaly? Also, please state explicitly in this sentence that this is coming from CMIP models.

Added.

L208: For the linear parameterizations, we compared our calibrated basal melt sensitivities to the values used in LARMIP-2.

Added comma.

L212-217: Please explain more explicitly why the underestimation/overestimation occurs. I'm not sure the reason is immediately clear.

We rephrased this paragraph and explained the underestimation and overestimation more explicitly (L. 265-270)

L218-219: A similar comparison was made for the quadratic paramterization, with the basal melt sensitivities applied in ISMIP6 (Jourdain et al, 2020). Here, the median Antarctic-wide calibrated…

Done.

L230: Dataset is one word

Changed data set to dataset.

L240: "Furthermore, the spread in our calibrated melt sensitivities…"

Changed 'the' to 'our'.

L243-246: “Models with calibrated melt sensitivity values outside the observation-based ranges would either underestimate or overestimate the past ice discharge if observation-based sensitivities had been applied. As a result, the spread in simulated ice discharge over the historical period will be lower for calibrated basal melt sensitivities than for the observation-based basal melt sensitivities.” I’m not sure the second sentence obviously follows the second. I actually think these sentences are just generally hard to follow, and could use some re-writing to improve clarity.

We rephrased the sentences and made the explanation more explicit.

L280: … with respect to the total Antarctic contribution cannot be reproduced either (about 70%...)

Added either

L287: performs better in the other region -- which region??

The region that was not used for the calibration. Added in text.

L292: Nevertheless, the mean response….

Added comma.

L315: Perhaps indicate that these results are shown in Figure 8 somewhere here.

Added reference to figure in the text.

L322: There is a discussion of ratios that are higher and lower depending on the method, but please state what the values of these ratios are.

We have added the values of these ratios in the text.

L325: What do you mean by highest basal melt method? Please clarify.

We’ve clarified this in the text.

L331: Discussion of large spread in the projections, but please put a value to this.

We added values for the ratios in the text to quantify the spread.

Table 8: I like this table, and the comparison you draw with ISMIP6 and LARMIP2. I am wondering why you picked 17% and 83% instead of 5 and 95% (I assume these are percentiles, but you may want to make this explicit in the caption). I also wonder if there is a way of visualizing these results in a figure? At the moment there are plenty of figures showing comparisons of different methods, but it would be nice if there were a figure showing all the final SLR projections as compared to other leading estimates in the literature.

We have explained the column names in the captions. We already visualised the projections using the median MIP basal melt sensitivities in Figures 8 and 9. Adding the ISMIP6 and LARMIP-2 AR6 values would make these figures a bit full, therefore we have chosen to put them in the table.

L403: … grounding line originate from ….

Added 'originate'.

Figure 7: Could increase size of this figure.

Done.

Figure 8 & 9: Please make all 6 panels the same size, and place legend either below or to the right of these six panels. X-axis label on bottom row is missing. Please also indicate more clearly that the blue and orange indicate the main methods used and that pink and yellow indicate the additional test with the single basal melt sensitivity it applied. Also, I think at least in my version, the red line showing the median is difficult to see, particularly in the red and orange distributions. Consider a different color?

Done.

L479: …, related to ice sheet instabilities and ocean dynamics, are not considered…

Added commas.

L489: remove (fully)

Removed.

L502: A physical explanation for a mismatch…. A mismatch in what? Please be specific.

Between observed and simulated ocean warming. Added in the text.

L508: …during the calibration period is representative of the future.

Sentence rephrased.

L526: What do you mean it is dominated by ice dynamics? This seems vague.

Replace 'dynamics' with 'discharge due to basal ice shelf melting'.

L545: …uses global mean temperature as the driver…

Added 'the'.

Consider making Fig 12 & 13 two panels in the same figure since they share the same structure, design, and legend.

Done.

Make Figure A1 and A2 the same sizes and ratios. A2 looks more stretched than A1.

Done.

L574: remove ‘ dynamics’

Done.

Conclusions section: Much of this is a repeat from the Discussion. Please consider editing the conclusions to include just the biggest take home messages and spend more time putting that in the context of the bigger picture of sea level projections, modeling Antarctic mass loss, and potential avenues and recommendations for future work.

Done.