Reply to reviews for 'The stability of present-day Antarctic grounding lines — Part B: Onset of irreversible retreat of Amundsen Sea glaciers under current climate on centennial timescales cannot be excluded.'

Reese, R., Garbe, J., Hill, E. A., Urruty, B., Naughten, K., Gagliardini, O., Durand, G., Gillet-Chaulet, F., Chandler, D., Langebroek, P. M., Winkelmann, R.

December 22, 2022

Reply to editorial comment by Florence Colleoni

Dear authors,

Apologies for the delay in the editorial process.

Both reviewers acknowledge the interesting conclusions of your manuscript. I have carefully red the response that you provided in the interactive discussions to both reviewers. Thank you for the great job addressing their comments.

There is a couple of points that I would like you to consider:

About the sub-shelf melting, which is clearly central to this study. The impact of quadratic parametrisation is not only about spatial pattern, it is also about the timing of retreat. This is something that you do not really discuss in your paper and in your response to reviewer 1. I would like to see a comment about it in the final manusript.

Another point is about the long-term spin-up of 400,000 years. It is not clear form the method section which climate forcing is used to do it. Then, the authors mention at line 480 that the grounding line retreat committed until pre-industrial is not accounted for. But, this is not the only issue, the long-term spin-up has some effect on the modelled inland ice velocities too and thus on the sensitivity of the ice response to long-term MISI. I would like to see a more developed comment about this in the discussion of the paper, as well as some more clear statements about the climate forcing used in the method section. Long-term spin-up has always been a delicate aspect of ice sheet modeling (and also oceanic modeling). Since we are now projecting long-term ice sheets evolution, I guess this deserves some careful notes.

I consider that all the comments raised by both reviewers 1 and 2 are good points and should be addressed carefully in the revised manuscript.

Therefore I recommand moderate revisions to this manuscript.

Many thanks for handling the review process of our manuscript. We addressed all comments raised by the reviewers 1 and 2 as detailed below and added the required discussion on the quadratic parameterisation on page 19 in lines 397 and following. For the thermodynamic spin-up we used constant climate conditions, we added this in line 190ff, and a discussion of these assumptions in lines 425 and following. We attach a latex-diff version of the manuscript in the response.

Following our replies from the public discussion, we made these additional changes to improve the manuscript:

1. *New PICO temperature corrections. During the review process we found that the temperature corrections applied during the PICO paremeter selection were too weak, i.e., the present-day melt rates were underestimated. We redid the parameter selection. The parameters are only marginally affected, but the temperature corrections changed.*

2. *New PISM ensemble. Based on (1), we updated our ensemble. Doing so, we also changed the ensemble parameters and now focus on the most sensitive parameters from the previous ensemble (C_d and δ). This allows us to sample more values of the parameter δ and also include the 'min' and 'max' parameters for PICO. Doing so, we found 21 possible present-day configurations for the Antarctic Ice Sheet of which we selected the 15 best (called AIS1-AIS15). For all configurations, we redid the present-day continued and reversibility runs. We find that our main conclusions are not affected by this. The main difference we find is that our states are more sensitive now, which is consistent with higher present-day melt rates, and reflected in all ensemble members showing long-term irreversible retreat of Thwaites and some now also showing retreat of Pine Island.*

3. *Section on theoretical framing. We moved the previous Section 2 on the theoretical background to the discussion as this section is not directly relevant for understanding the experiments, but it provides the context for a discussion on tipping of the West Antarctic Ice Sheet.*

4. *Table 2: we consolidated all previous tables with results about the PISM simulations into one table and added further information as requested by the reviewers.*

5. *We moved discussion elements from the PISM results and other sections to the discussion to shorten the manuscript.*

6. *We added a section to discuss the shortterm evolution and reversibility of our simulations.*

Reply to Michele Petrini Referee 1, https://doi.org/10.5194/tc-2022-105-RC1

General comments:

In this paper, Reese et al. use the 'Parallel Ice Sheet Model' (PISM) and the sub-shelf melt module 'Potsdam Ice-shelf Cavity mOdel' (PICO) to analyse the multi-millennial evolution of the Antarctic grounding-lines under a constant, present-day climate forcing, and the reversibility of associated large-scale changes. The authors first calibrate the sub-shelf melt module PICO against observed (Dotson ice-shelf) and modelled (Filchner-Ronne ice-shelf) melt sensitivity to ocean temperature changes. Optimised PICO parameters are then used in an ensemble of continuous spin-up (pre-industrial forcing) - historical (1850-2015 forcing) PISM simulations, which are evaluated against present-day observations. Simulations showing best agreement are then extended for 10,000 years beyond the historical period under constant present-day climate forcing and bathymetry. The evolution of the Antarctic grounding-lines is then analysed, and reversibility is tested for simulations showing large-scale retreat by reverting climate forcing to pre-industrial conditions.
In my opinion, this is a great paper, addressing an extremely relevant scientific topic (future states and reversibility of Antarctic grounding-lines) with the use of advanced modelling tools (e.g., PICO instead of simpler sub-shelf melt parameterizations) and innovative techniques to calibrate numerical modelling results against observations (e.g., PICO parameters optimisation, PISM ensemble scoring methods). The study presents some limitations (e.g., no isostasy, no full equilibrium reached at the end of the simulations), but these are clearly discussed throughout the manuscript, and are in my opinion acceptable considering the technical challenges (and, likely, computational costs) associated with this type of study.
In view of this, I consider this work definitely worthy of publication, and I commend the authors for the great deal of technical work they have undertaken.
I have only two major comments, mainly related to the quality and number of figures included in the manuscript. In fact, I think some important figures are missing, and some of the included figures do not allow the reader to easily verify what is stated in the main text.

Many thanks for reviewing our manuscript and for your constructive comments on how to further improve our manuscript. We are happy about your evaluation of the work! We hope to have answered all questions and addressed all improvements appropriately.

1. As stated in the main text (L140–141), I fully agree that the PICO calibration proposed in this study approach has a great potential to be used in further Antarctic studies, including future sea-level projections. However, I was a bit disappointed not finding any 2D-map of sub-shelf melt rates, and I think some of these figures should definitely be included (either in the main text or as Supplementary material) to see the outcome of the calibration procedure, and also to get a sense of the magnitude and spatial variability of the sub-shelf melt forcing. For instance, 2D maps of sub-shelf melt rates could be included for ANT2/ANT2+0.1K/ANT2+0.3K simulations at pre-industrial and present-day snapshots. Moreover, since the calibration goal is to obtain correct melt rates and sensitivity (P.7, L179-180), I think it is necessary to include one additional figure showing the comparison between present-day PICO and observed sub-shelf melt rates, and briefly discuss this comparison in the text. Another interesting thing (but I leave this as a suggestion, rather than a request) would be to show, for one or two snapshots, a comparison of PICO sub-shelf melt rates and same melt rates calculated off-line using the simple two-equation quadratic parameterization. In fact, in this study PICO is calibrated to 'behave' like the two-equation

quadratic parameterization in terms of sensitivity to ocean warming, but it would be interesting to see the difference in terms of spatial variability within the ice-shelf cavities.

Thank you very much for the suggestions. We created new a supplementary figure, see Fig. S2, showing the melt rates for the 'historic' ocean conditions and 'present-day conditions' for the best scoring initial configuration for the 'mean' PICO parameters (AIS5), the 'min' PICO parameters (AIS12), and the 'max' PICO parameters (AIS1). Scores and results for the model simulations of the new ensemble are given in Fig. B1 and Tables 2 and S6. Since we include the range of PICO parameters now, we did not explicitly test for the sensitivity to ocean temperatures as this is indirectly included when testing the different sensitivities to ocean temperatures through the different PICO parameters. We compare the aggregated melt rates per ice shelf for all parameter combinations in supplementary Tables with the new versions in the Supplement (Tables S1-S5) and added a discussion in the text in lines 126, 139 and following.
We decided not to include a comparison with a quadratic parameterisation. One central difference between PICO and the quadratic parameterisations as implemented for example in Jourdain et al. (2020) is that PICO uses only one temperature input per ice shelf while the quadratic parameterisation requires a two dimensional field or at least a vertical profile of ocean input at the ice shelf base. In fact, spatial variability in the ice shelf for the quadratic parameterisation will depend on this temperature input, which makes it not straightforward to compare both parameterisations. Work in this direction has been done by Favier et al. (2019) and Burgard et al. (2022). The latter paper was already discussed, we will add also the first paper to the discussion, e.g. line 364 and following. A discussion on the quadratic melt parameterisation was added in line 397 and following.

2. I found it very difficult to track the evolution of the Antarctic integrated ice volume in the spin-up, historical, 10,000-extension and control simulations, since these are included in three different figures (Fig. 4, Fig. 5, Fig. B2) with different scales. I fully understand why the evolution under historical forcing is highlighted in Fig. 4, but then I would also like to have time series similar to Fig. B2 for the present-day forcing and recovery simulations (maybe in the Supplementary). I was very confused when looking at Fig. B2, as it seems that at the end of the control runs the ice-sheet is not in full equilibrium - which is also stated in the text (P16, L365-367). However, by looking at Fig. 4, it seems that before introducing the historical forcing there is full equilibrium - maybe I am missing something, but I think some work should be done to show these results more clearly. I also think that a panel with integrated sub-shelf melting (perhaps averaged over ice shelf area?) like in Fig. 4 should be included also in Fig. 5. More in general, I think that the quality of some figures should be improved:

   Thank you very much for pointing this out. Some of the confusion might have come from us sometimes showing results relative to the control simulations and sometimes the not-corrected variables. We made Figure 3 and Figure 4 (former Figures 4 and 5) consistent by showing all simulations relative to the drift in the control runs and discuss this as well as the drift in the control runs in the text (we explain this in more detail in lines 237-243).
   Furthermore, as requested, we added the evolution of the ice volume under constant

present-day climate and under the reversed forcing to Figure B2. We prefer to not include the BMB in Figure 4 as the figure already contains a lot of information and the ocean temperatures are constant over the simulations so that any changes arise from changes in the geometry. We added a new figure to the Supplement S6 showing the evolution of basal melt rates, surface mass balance, grounding line fluxes and fluxes at the calving front.
We hope these changes make it easier for the reader to follow.

3. a) Fig. 4: it would be nice to include the figure with the observed ice thickness change from Smith et al. 2020, so that the reader can directly compare modelled and observed pattern/magnitude. The ensemble-average and BedMachine grounding-lines are a bit difficult to distinguish, I suggest using different colours/line thickness. I also think that the time series should also include total ice volume, not only dV. Finally, I think that in the caption it should be specified whether BMB refers to sub-shelf melting + grounded ice melting at the base (frictional heating, GHF), or sub-shelf melting alone (I'd rather include sub-shelf melting alone, but I leave this choice to the authors);

   Many thanks for the suggestions. Note that due to restructuring our manuscript, this is now figure 3. We increased linewidth to make the grounding lines easier to distinguish and changed the BMB value to only include the melting below ice shelves. While it would be nice to have a direct comparison, we prefer to not include the figure of Smith et al. (2020) as this is no original work of ours. To make it easier for the reader to pull up both figures, we will add a direct reference to the Figure in that paper (see line 244). Since ice volume differences between ensemble members are larger than the changes over the historic period, we do not include the ice volume in Fig. 3 as only straight lines would be visible in such a panel. However, to have the option to compare these values, we show the total ice volume in the Fig. B2.

   b) Fig. 5: it would be nice to also have the time series for the evolution of sub-shelf melting and grounding-line flux;

   We think that adding them here would overload the figure, so we created a new supplementary figure S6.

   c) Fig. 6: I suggest either showing this figure for the whole domain, or including a pan-Antarctic map to show the area considered in the zoomed figures;

   We changed this figure, which is now Fig. 5, to separate the millennial timescale reversibility experiments (panel a) form new, centennial timescale reversibility experiments (panel b). We added a box to show the zoom region used in the panels in (b) to the full Antarctic map in the panel (a). Since more ensemble members show large-scale retreat now, we only show a selection of runs in the second panel of this figure. We added an additional figure with reversibility plots for all members in the SI as Figure S5.

   d) Fig. 7: this figure is a bit too small, and there is a dark blue line labelled 'revere' which I assume is a typo. It would also be very nice to have this type of figures also for the 10,000–extension runs + recovery runs.

Thanks for the recommendations. We updated this figure (now Figure 6) to show the ice volume evolution instead of the grounding line flux as this is easier to interpret. We show both curves, 10,000 years and 300 years.

Specific comments:

P1, L8: I would add to the abstract the fact that isostatic rebound is not accounted for in the simulations, e.g., "…under constant present-day climate forcing and bathymetry".

Done.

P2, L54-56: I suggest rephrasing, and including a citation for CMIP5.

We re-arranged this whole section to first discuss the section on PICO and then introduce the PISM experiments. We reformulated the sentence and added a citation for CMIP5, see lines 50 and following of the new manuscript.

P3, L62: '... let the ice sheet states evolve'.

We understand this comment as to stop the sentence here and not repeat the experimental design in too much detail. Based on this we will shortened the paragraph removing some more general information. See lines 53 to 59 of the new manuscript.

P3, L65: '... to occur eventually under…'.

This sentence has been updated in the new paragraph, lines 53 to 59.

P3, L65-67: I suggest rephrasing, e.g., 'To test … retreat, the simulations showing large-scale retreat are extended for 20,000 years under reverted pre-industrial forcing'.

Done.

P5, L125-126: I suggest rephrasing.

Done, see line 506 and following.

P6, L134: 'The present paper can be understood to investigates…'.

We reformulate this to "The present paper investigates..." to make the sentence less complicated, see line 514.

P6, L149: '...which differ in between across…'.

Text updated to clarify this, see lines 69 of the new manuscript.

P6, L.160-165: I found these sentences not very clear, I suggest rephrasing.

We simplified this and removed the thermal driving from this section. We suggest a new definition in the caption of Figure 1.

P7, L165: '...we hope *aim* to represent'.

We moved this to the discussion and reformulated. See lines 360 and 366.

P11, Section 4.1: I suggest including how the SMB is calculated in PISM, either in this section or in Section 4.2.

We directly use the data from RACMO. We will extend in this in the methods section in lines 179 and following to make this clearer.

P12, L291: I suggest specifying how many thousand years, rather than stating 'several'. Also, I'd use 'at 8 km horizontal resolution'.

Done, see line 192 and following.

P12, L301: I suggest including the notation Hmax as in Table 2.

Hmax is not varied in the new ensemble any more as this parameter had little influence.

P13, L315: I would remove 'the best run is used ... in Urruty et al.', as this is not relevant for this paper.

Done.

P15, L326: I would expand here on what quasi-equilibrium means, and what are the implications. I think it would be enough to move the text at P16, L365-369 at the beginning of the section.

Good idea, thanks. Done, see lines 234 and following.

P15, L335: I would add some text linking the discrepancy from observation to simulated and observed sub-shelf melt rates pattern (either here, or in the discussion section). Also, the same could be done with simulated and observed SMB.

We added this caveat in the discussion, see lines 395-400.

P15, L343-345: I would split this sentence in two.

Since this sentence is more discussion than results, we integrated this this sentence into the discussion, see lines 390 and following.

P15, L348-350: I suggest rephrasing.

Based on the new results, this has been reformulated, see new lines 214 and following.

P16, L374-376: I suggest rephrasing.

Done, see lines 282 and following of the new manuscript.

P21, L468: I would use 'Moreover' instead of 'in particular'.

Done, see line 409.

P21, L468-470: I would also add something like '...as well as glacial isostatic rebound, which can induce changes in the local bathymetry and ice shelf cavity geometry'. Also, I think the paper by Whitehouse et al. 2019, ('Solid Earth change and the evolution of the Antarctic Ice Sheet', Nature Comms.) should be cited here.

We reformulated this, see lines 409 and following, and added the paper.

P24, L541: Rather than '...change in sub-shelf melt rates is thought to be a major trigger. . . ' I would use something stronger, e.g., '... recent observations and modelling suggest that. . . '.

Done, see lines 561.

Reply to Anonymous Referee 2, https://doi.org/10.5194/tc-2022-105-RC2

Comments to Reese et al. (2022)
This study investigates the committed grounding line retreat due to MISI for the present-day climatic forcing. The MISI hypothesis states that the ice flux across the grounding line increases when the ice thickness increases and hence, when the grounding line retreats on a retrograde sloping bed, a positive feedback arises. The assessment of the grounding line retreat due to MISI is achieved by performing long-term runs (10,000 years) into the future. The reversibility is also tested by running the simulations for 20,000 years using a pre-industrial forcing.
The simulations use (optimized) melt rates from PICO and the historical climate forcing provided by ISMIP6 for the period 1850-2015 (the actual SMB forcing is only defined from 1950 onwards and is kept constant before). I believe it is an interesting study that looks at the slow equilibration time of the ice sheets and the long-term feedbacks involved with respect to the marine-based parts of the Antarctic ice sheet. Below you can find my suggestions to improve the manuscript.

We thank the reviewer for their support and reviewing our manuscript! We address all suggestions below.

Main comments:
The manuscript shows that the main regions where the model parameters give grounding line retreat are the Amundsen Sea Embayment, the Filchner-Ronne Ice Shelf and the Ross Ice Shelf (along Siple Coast). In contrast to the observations, thinning is also identified along the Ross Ice Shelf and the Filcher-Ronne Ice Shelf in the simulations for the reference state. How realistic is the committed grounding line retreat in these regions when there is already a bias for the present day?

We agree that committed retreat in regions in which our historic simulations deviate from observed signals, as in FRIS and Ross, is not reliable. We thought about modifying the historic forcing to avoid such signals, but decided to record it as it is, since this is the ocean input we get from the CMIP5 model in combination with PICO that we decided to use. We changed Figure 5 (now Figure 4 in the new manuscript) by adding a label for the hatches in that figure to indicate regions in which observed and modelled present-day signals deviate substantially to hopefully make this more clear. We extended on the discussion in the updated version of the manuscript (e.g., lines 380-389 in the discussion.)

The modelled thinning rates in the Amundsen Sea Sector are rather low for the present day. To test for biases in the ocean forcing, a constant temperature anomaly is added to all ice shelves around the Antarctic. The Filchner-Ronne Ice Shelf and the Ross Ice Shelf are somewhat more closed off from oceanic heat, while the Amundsen Sea region might experience higher oceanic warming to match the observed thinning rates. Could it be more appropriate to apply a spatially variable ocean temperature anomaly to better match the observed thinning rates?

We agree that it would be an improvement to better match observed thinning rates. For this study, we decided to use the ISMIP6 forcing as it is rather than modifying it to improve the comparison of our model representation in present-day (e.g., removing the historic trend in the Weddell and Ross seas and increasing the trend in the Amundsen Sea). It would be *interesting, as a next step, to think about why these issues arise. Note that with our new ensemble we did not apply these ad-hoc temperature sensitivity tests any more as we include different PICO parameters, which indirectly capture differences in melt rates between the historic and present-day states as the parameter sets have different sensitivities of sub-shelf melt rates to ocean temperature changes. Interestingly, also with the 'max' PICO parameters included in the ensemble which yield a higher increase in ocean-driven melt over the historic period as they start with lower melt rates in 1850, the modelled historic magnitude and pattern of mass loss was not substantially altered, see Fig. 4 and Table 2 summarising the historic simulations.*

Specific comments:
L29: It makes more sense to me to report the regional warming around/above the Antarctic continent than the global mean.

Done, see line 32 of the new manuscript.

L50: This is confusing, it sounds as if you use the present-day climate forcing to test for reversibility of the grounding line retreat. I guess not because on L66 you say that you use pre-industrial climate forcing for the reversibility simulations. Could you rephrase to make clear that the forward experiments include the present-day forcing?

We moved this sentence to the end of the paragraph that we reformulated, see lines 40 to 58. Hopefully this is clearer now.

L306: Could you report the RMSE for ice thickness, ice-stream velocities, deviations in grounding and floating area and the differences between the ensemble members?

We added this information in a new supplementary Table S6 and refer to it in line 211.

L317: What is the rationale to look 10,000 years into the future? And why do you double the simulation time for the reverse experiments? On L372 you report that the ice sheet states evolve to a new equilibrium, but GL's might not have fully converged to a steady-state after 10,000 years.

We selected 10,000 years as a optimal value between computational time and duration of the simulation. For the reverse simulations we doubled the time to make sure that the states are really close to equilibrium as we found that re-advance takes longer than retreat. With the 20,000 years we aim to exclude the possibility that the grounding line is reversible, but the model just did not run for long enough. We add this reasoning in the end of the section, lines 228-229. The full equilibrium state is usually not fully reached after 10,000 years, but since we are primarily interested in large-scale retreat, we think that this time period is sufficient to understand the large-scale patterns. To obtain proper steady states, we would probably need to run the model for 100,000 year time scales. In line 401, we add a discussion on this. And changed the text in 276 and following to make clearer that the ice sheet evolves towards *a steady state.*

L347: The sentence 'This as well as the choice of the sliding law, has been found also in previous studies' looks incomplete.

Thanks. We added the missing piece, see line 263 and following.

L367: You report the model drift during the historical simulations, but what is the model drift during the next 10,000 years?

We add the model drift over the next 10,000 years in a Table 2 of the new manuscript. Table 2 also contains the information from the previous Tables 2,3,4. It is lower than 26cm and thus substantially lower than the reported mass loss under present-day climate forcing.

L379: The ensemble members indicating substantial grounding line retreat occur for more slippery bed conditions or higher oceanic temperatures. Hence making the model more sensitive increases the chances that the tipping point is reached. Low values for the till effective overburden fraction strongly enhance the grounding line retreat. Could you add a word on the likelihood for the model parameter choices made?

There are three studies that allow us to put an estimate on δ. Engelhardt and Kamb (1997) and Smith et al. (2021) performed measurements in boreholes of Whillans and Rutford ice streams, respectively. They estimate values for the effective pressure in the drainage system ($N = P_0 - P_w$ with ice overburden pressure $P_0 = \rho_i g h$ and subglacial drainage system water pressure P_w.). Those values are found to be within $0.7 \pm 0.7\%$ of the ice overburden pressure. Although the pore water pressure in the till P_{till} is different from the subglacial drainage system water pressure P_w, since the systems are connected and the water pressure must be continuous at the interface between water and till, those values above yield an upper bound on δ (which is the fraction of the effective pressure in the till to the ice overburden pressure). A direct estimate of Blankenship et al. (1987) yields a value of $\delta \approx 0.006$. We added this to the manuscript in lines 219 and following where the initial configurations are presented.

Figure 6: Put a box around the figures to increase clarity, maybe add names for the ice shelves to make it more clear for the reader what we are looking at.

We updated this figure, now with two panels. We show the area over which grounding line retreat is irreversible over the long multi-millennial timescales in panel a and added also shorter irreversibility experiments to determine the onset of irreversible retreat in panel b. We show the shorter experiments for a selection of runs. All experiments are shown in Supplementary Figure S5.

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