Author's response: Review of the manuscript "The stability of present-day Antarctic grounding lines — Part A: No indication of marine ice sheet instability in the current geometry" by Urruty et al.

Dear Reviewer,
Thank you for reviewing our manuscript. Your comments are helpful and we are glad to respond to them. Please find our responses to your comments below and we will address all comments in a revised version of the manuscript. In order to facilitate the reading of this document, our responses are given in blue and italic compared to your comments which are given in black without italic font.

The manuscript describes results of a multi-model study aiming to assess sensitivity of the current Antarctic grounding line positions to short-term perturbations in the submarine melting. The authors demonstrate that simulated grounding line position reverts to its initial state after the perturbations are removed. The study represents a substantial group effort, and simulations produced many results that can be useful for other kinds of analyses. The current version of the manuscript, however, requires significant modifications, because it has several misconceptions and self-contradictions.

We are grateful for this feedback on our manuscript and we hope we will clarify the misconceptions and self-contradictions that you highlight.

General comments

The authors conclude "This suggests that present-day grounding-line retreat is driven by external climate forcing alone. Hence, if the currently observed mass imbalance were to be removed, the grounding-line retreat would likely stop." The first part of these conclusions – the "present-day grounding-line retreat is driven by external climate forcing"– is absolutely correct. However, this is an observational evidence, rather than conclusions of this study. As the authors indicate on line 60 "... it is not realistic to assume the Antarctic Ice Sheet is in a steady state today", it is equally unrealistic to assume the Antarctic Ice Sheet has ever been or will be in a steady state. A state of an ice sheet cannot be considered in isolation from its environmental conditions – atmospheric and oceanic, at least. An ice sheet can be in a steady state only with respect to a specific set of these conditions that are constant in time. If these conditions change, the ice-sheet configuration, and its grounding line position change as well. The Earth atmospheric and oceanic conditions always vary, and they do so on a wide range of temporal scales (e.g. Jouzel, et al. (2007); Thomas et al. (2013)). Consequently, it is the variability of these conditions (that are in their turn are affected by the ice-sheet conditions) that drive variability in the ice sheets.

We do not clearly understand the argument of the reviewer and presumably only partly agree. Owing to the long-term response of ice sheets (decadal to multi-millenia), it is obvious that they cannot be in a steady state in an always varying climate. We agree that, ultimately, any change in the ice sheet is triggered by variations in the external conditions. However, ice sheets can undergo hysteresis and thus respond irreversibly, driven by internal dynamics, to such changes. Observing a mass loss and a retreat of the grounding line alone cannot discriminate whether these changes are only a limited and reversible response to the forcing, or a self-sustained collapse (initially triggered by climate forcing). Modeling studies appear fundamental to make such a discrimination and our work is an attempt in that direction. We have made changes to the manuscript in the introduction to make it clear that the real ice sheet is not in steady state today, and further justification for requiring a steady-state configuration for our modeling analysis. With regards to the sentence in the abstract, we agree it could be confusing to the reader that the statement "present-day grounding line retreat is driven by external climate forcing" could be concluded from observations. Importantly, what we can conclude here from our model experiments is that the current retreat is only driven by external climate forcing, and not yet driven by MISI alongside this. To alleviate confusion, we have removed this sentence from the abstract.

The abstracts starts with the sentence "Theoretical and numerical work has firmly established that grounding lines of marine-type ice sheets can enter phases of irreversible advance and retreat driven by the marine ice sheet instability (MISI).". The marine ice-sheet instability hypothesis was proposed by Weertman (1974) who was interested in "the steady-state size of a two-dimensional [unconfined] ice sheet ... that rests on a flat bed (flat before the ice sheet was placed on it) situated below sea-level". No part of the Antarctic (or Greenland) Ice Sheet has this configuration. While the authors acknowledge that the presence of the lateral confinement complicates the original Weertman's (1974) and later Schoof's (2007, 2012) conclusions that the bed slopes determine stability of the grounding line (lines 30-35), they seem to continue to use the bed slope as an indicator of stability throughout the introduction section. In addition to the ice-shelf lateral confinement, non-negligible bed topography found, for instance, under Thwaites Glacier, and very weak beds, such as under Siple Coast ice streams complicate stability conditions (Sergienko and Wingham, 2019, 2022). In the presence of feedbacks between the ice sheet characteristics (e.g., the surface elevation) and environmental conditions (e.g., surface accumulation) there are no general stability conditions at all (Sergienko, 2022).

We fully agree with the above arguments. We adjusted the manuscript carefully to avoid the confusion that retrograde bed slope is the only determinant of marine ice-sheet instability (i.e., bed slope is a sufficient and necessary condition for MISI), however recognizing that it is a necessary condition. We are grateful for the additional sentences and references that we have now incorporated into the paragraph in the introduction where we explain the grounding line stability conditions.

With regards to the comment where we "continue to use bed slope as an indicator of stability throughout the introduction" we have found two instances where we think the reviewer is referring to:

1) *line 43 "Large parts of the Antarctic Ice Sheet have been identified as susceptible to MISI due to their deep inland sloping topography", which is taken from figure S60 in Morlighem et al. 2020*
2) *lines 52-56, beginning with "Parts of the East Antarctic Ice Sheet could also be susceptible to MISI due to their deep marine basins"*

In both cases we had used the word susceptible to suggest that these regions could be prone to MISI due to their topography, i.e., fulfilling the necessary condition. To make it completely clear that we are not suggesting that they are prone to MISI based on bed-slope alone, we have removed the reference to topography in 1). For 2) we have left it as it was originally, because we do believe that using "could" makes clear that having deep marine basins means that they fulfill the necessary, but potentially not the sufficient condition for MISI.

Although it is true that the marine ice-sheet instability hypothesis is widely used to interpret the observed grounding line retreat, there is no need to promote this misconception, or more accurately, misapplication of a concept of stability (or instability). These concepts can be applied only to a steady state. Even though the authors point out (line 75) that "Although stability cannot strictly be defined for non-steady states" they continue "we find that even with transient forcing included, the current grounding-line positions do not show self-sustained retreat." These kinds of statements are confusing, and to some extent, self-contradictory. A possible interpretation of the finding that the grounding line positions do not show self-sustained retreat is that the grounding-line advance and retreat does not correlate with the immediate forcing it is experiencing. For instance, Robel et al. (2022) point out that in places with the appreciable bed topography, the grounding lines can persist at bed peaks under substantial changes in the environmental conditions.

It may appear that it is just a matter of semantics whether to call the present-day ice-sheet configuration "stable" or "close to stable" (either with or without quotation marks) or to have sentences like "Note that in this manuscript we refer to grounding lines as 'unstable' if they are engaged in MISI-driven retreat, and 'stable' otherwise, even if the grounding lines are not in steady state." However, the words matter (especially those in the abstract and conclusion sections), and the basic assumptions of the study should not contradict observational evidence. As currently framed, the manuscript gives a strong impression of misapprehension, despite already mentioned "bracketing sentences".

As mentioned by the reviewer the concept of "stable" or "unstable" can only be applied to a steady-state. In particular, we agree that there are some confusing statements in the manuscript with respect to how we refer to the results of the PISM experiments (conducted using a transient *state) alongside the experiments conducted with Úa and Elmer/Ice, both of which are in steady state. We have reworded the sentences that you have quoted above to alleviate confusion. Furthermore, we now also make use of "unsteady" and distinguish it from "unstable" as we agree with the reviewer that there is some confusion of these words found in the literature. We have now made sure not to use the words unstable and stable when referring to the transient "unsteady" ice sheet state used by PISM. We note that we use control simulations alongside the perturbations so that the impact of the transient forcing in PISM is removed. We then refer to "reversible" in the sense that it arrives at the location in the control simulation.*

In the PISM experiments that include present-day forcing, it is possible that the grounding lines have stopped at a bed peak. In our Part B experiments, applying the present-day forcing for an extended period allows us to test whether the grounding lines will remain at these positions in the long-term (millennial timescales). Indeed we find that in a number of cases, in certain regions, they do not. Importantly, however, we find that reversing the forcing in Part B from present-day (under which grounding lines show large-scale, irreversible retreat in Thwaites) to historic conditions shows that grounding lines remain close to their current position (over 10,000 years). We take this as an additional indication that no irreversible retreat has begun yet.

We want to stress that the basic assumptions of our study are not contradicting observational evidence. The study is designed to be able to perform stability experiments and at the same time draw conclusions about the current state of Antarctic grounding lines, as we explain in the following: To be able to apply the concept of "stable" and "unstable", we adjusted the surface mass balance for the Úa and Elmer/ice initial states in our study. With perturbation experiments (the outlet glaciers are able to recover after small-amplitude perturbations of the sub-shelf melt), we show that a stable steady state with the current geometry of the Antarctic Ice Sheet can be built (with ad-hoc adjustment of the SMB). This means that these grounding line positions are not inherently unstable.

Importantly, we think that these results can be applied to understand the current state of the Antarctic Ice Sheet as follows: that the current grounding line positions are not inherently unstable means that the positive feedback related to MISI is not necessarily at play for grounding lines located in their current position. Inversing the argument, this means that an observed retreat of grounding lines in their current position does not imply that there is a self-sustained component to it. Statements such as "The bed is sloping down towards the interior and the grounding line is retreating, this means that MISI has already begun / WAIS is collapsing" are wrong for the current grounding line positions. As a next step, since we find similar results for both models and also reversibility of the transient state of PISM, we think that our results rather support that the currently observed grounding line retreat has no self-reinforcing, positive feedback component to it at all.

Identifying the possibility for large-scale, internally driven retreat of the present-day Antarctic grounding line is of high interest as pointed out by reviewer 2. Our results drive us to the conclusion that none of the outlet glaciers are obviously engaged in an internally driven retreat. Of course our approach has some limitations that we hope are properly described in this new version of the manuscript. In this version of the manuscript we have been more careful on the semantics, added more explanation of the interpretation of the results and hope to reduce confusion for the reader.

Technical Comments

Presumably the calving front position was held constant during all simulations. It is unlikely that during 100 years no icebergs would calve. As Haseloff and Sergienko (2022) show, the calving front conditions have stronger effects on the grounding line stability than melting.

The calving front position was indeed held constant, but this does not mean nothing is calved, because in order for the position to remain fixed, we are imposing a calving rate which is represented by the flux of ice at the front. When the melt perturbation is applied and the ice shelves thin, we indeed do not impose calving, but instead the ice thickness is set to a minimum value, small enough to apply only a very limited buttressing. This minimum ice thickness could have an impact on rebuilding the ice shelf, and we address this limitation in our discussion.
It would have been challenging, and is currently not possible in all models, to impose a calving law during the perturbation. We agree that this is a limitation and we have added a sentence to the discussion to reflect this, including a reference to Haseloff and Sergienko (2022).

Lines 103 and 167-168 state that PISM uses 8 km horizontal resolution (no need to repeat that twice). Seroussi et al. (2014) find that the accurate representation of the grounding line dynamics requires the horizontal resolution of 2 km or higher. I appreciate that running a spin-up for 400 kyr with such a high uniform resolution is computationally expensive, but at least some comments have to be made in that regard.

We have removed the first mention of 8km resolution. We agree that a higher resolution would be preferable, but as you mention, it is not possible to increase the resolution of PISM further. However, the results for all three models are consistent, despite these differences in resolution. We added: "Seroussi et al. (2014) report that a horizontal resolution of 2km is required to accurately represent grounding line dynamics, Feldmann et al. (2014) find that using a subgrid interpolation of friction, grounding line reversibility in PISM is also captured at coarser ($x >$ 10km) resolution. While a higher horizontal resolution would be wishful, we here employ this interpolation to be able to run PISM over millennial time scales. We find that PISM results are in line with results from Elmer/ice and Úa that employ finer resolution around the grounding lines."

Lines 117-119 PICO is hardly a realistic representation of submarine melting. As studies that used ocean circulation models to compute melt rates show over and over again, melt rates do not correlate with the ice-shelf thickness or the ocean depth (e.g .Goldberg and Holland, 2022). Once again some justification is needed for the use of PICO (an alternative could have been melt rates inferred from observations, e.g. by Adusumill et al., 2020).

We agree with the reviewer that PICO may not be the most realistic way to impose submarine melting in our models. However, conducting perturbation experiments using observations directly may not have been a good solution. Firstly, these observations are a snapshot of the current situation and we want to perturb the current situation, and therefore move away from observations a little bit. One option would have been to apply a factor or an offset to these observations, but we are not sure this would be better than a full parameterisation like the one used here. Secondly, this would have also had the issue of how to apply melt in cells that become afloat during the experiments, where observations are not present. A parameterisation of the melt does not have this issue. We want to note that ultimately the nature of the perturbation itself is not important; we could have chosen to perturb the grounding lines using a number of different parameters, and indeed we did tests perturbing the basal slipperiness field, and found the same results. In addition, all models impose slightly different spatial melt distributions; Úa and Elmer/Ice have a background "balanced melt rate field" due to the correction approach, and apply only the anomaly in PICO on top of this, whereas PISM uses the PICO melt rates directly. Despite these differences in melt, no model shows any sign of MISI driven grounding line retreat, supporting the notion that the melt rate distribution itself does not affect the results.
We have added "While PICO is not a perfect representation of present-day melt rates, it can track the grounding line movement and provides melting for newly ungrounded regions." to discuss this.
Furthermore, we want to note that PICO includes more physics than a simple, depth-dependent parameterisation that is only based on ice draft depth / water column depth as it parameterises the vertical overturning circulation in the ice shelf cavity.

Lines 150-165 This section about mass-balance correction is opaque. It seems that the results whether the grounding line retreats or not depend on this correction. It also appears that the obtained 'steady-state' is contrived, i.e. the configuration is essentially artificially held in this state by means of this correction. If this is not the case then this should be illustrated. With the used approach, making any connections to the actual ice-sheet state is a stretch.

As mentioned above, stability is a property of steady states, and therefore our methodology requires a steady state. To obtain a steady state in the current geometry of the ice sheet we had to apply a correction to the mass balance term. However, creating this steady state configuration in Elmer/Ice and Úa does not mean by definition that this steady state is stable. This was indeed the *purpose of our experiments, to determine whether this steady state configuration of the ice sheet is stable or unstable. By excluding the effect of transient external forcing (starting from a steady state) we are able to understand if the grounding lines in their current geometry are undergoing MISI or not as outlined in our reply to the general comments above.*

We appreciate that this balanced approach is a limitation, in the sense that we are artificially shifting the state of the hysteresis curve (see Fig.1 Part B manuscript) and varying the spatial gradients in surface mass balance. However, they are two reasons we believe this does not affect our conclusions: 1) the correction we apply to the mass balance is small (as we mention in the discussion) and therefore we have not shifted the critical thresholds in the real ice sheet, 2) PISM does not use the mass balance correction approach and is not in steady state, but also shows that the grounding lines retreat when perturbed and re-advance to the control run positions when the perturbation is removed. Thus, our results are consistent despite the individual choices made in each model. We have added some additional sentences to the end of Section 2.2.1 to address these issues.

Lines 223-247 What is used as the surface mass balance during the perturbation experiment? Is the mass-balance correction applied as well?

Yes, we use the corrected mass balance field throughout all (control and perturbed) simulations in Elmer/Ice and Úa, and the RACMO surface mass balance field is used in PISM. We have added a sentence to this section to clarify this:
"In all of our control and perturbed simulations the surface mass balance remains fixed, which in Elmer/Ice and Úa is the corrected mass balance field m described in Section 2.2.1, and RACMO surface mass balance in PISM."

Line 253 "An increase in ice shelf melt, and thus reduced buttressing, will lead to an increased ice flux. "This is an unsupported statement. It could potentially be verified by computing buttressing and demonstrating that it was reduced. If the calving front position is indeed fixed, then the retreat of the grounding line caused by increased melting, leads to the increase of the horizontal extent of the ice shelves, and hence increase in buttressing (Haseloff and Sergienko, 2018, 2022).

Agreed, without computing the buttressing, we do not know that increased ice shelf melting has reduced the buttressing. However, our simulations clearly show that when we apply the melt perturbation, we get a sharp increase in ice flux, which is assumed to be due to reduced ice thickness having a larger effect than the increased length of the ice shelf. Indeed, our profile plots show the ice shelves thin substantially during the perturbation. We have reworded this sentence in the text to read:

"Increased ice shelf melting in our simulations leads to a sharp increase in ice flux across the grounding line, which is assumed to be due to a loss of buttressing as a result of ice shelf thinning."

Lines 258-259 "When the grounding line does not retreat further, it means that it has found a new stable position very close of the previous one."As already mentioned, an alternative could be a non-linear response of the grounding line to the applied forcing (Robel et al., 2022).

We agree with the reviewer that we cannot exclude that the GL will temporarily stop at a position for some time and then start to retreat again as suggested by Robel et al. (2022). Robel et al. (2022) suggest that "[..] the utility of 'stability' as a tool for categorizing observed glacier changes is limited without the critical context of multi-centennial (or millennial) glacier changes, [...]" They appear to decide if a grounding line "stabilizes" by running the model for 1000 years forward in time and testing if further retreat occurs during that time. We here capture almost half of that period, i.e., the centennial time scales, with the 480 year relaxation period for the entire Antarctic Ice Sheet using three different models (and therefore three different bed topographies). Since we find no indication of further retreat during this time we are relatively confident that the non-linear response mentioned by the reviewer is not at play. There are instances where the grounding lines have stopped at bed peaks when the perturbation is removed (e.g. Cook, Ronne, Thwaites), but there are also cases where they do not stop at a bed peak (e.g. Dotson). We further test the millennial timescales in PISM, as discussed in the reply to the main comment where this concern was also raised. We added the study to the first paragraph of our discussion.

Lines 262-265 and eqn(2) It is unclear what is meant by "the recovery of the ice flux" ΔQ, and how it is computed. Presumably the e− folding time could be estimated for the grounding line as well (e.g., Sergienko and Wingham, 2019).

We have rephrased lines 262 to better explain the justification for calculating the e-folding time. We could have also calculated the e-folding time for the grounding line position, but as explained on lines 248-253, we choose to use ice flux as our 'metric of interest'. As stated in the manuscript we chose this because the grounding line/grounded area recovery time is much longer because it first relies on the regrowth of the grounded part of the ice sheet, to recover the ice volume lost during the perturbation.

Summary

The manuscript requires significant modifications in terms of the presentation and of the framing of the problem as well. Also, the main conclusion that the observed grounding line migration is driven by the atmospheric and oceanic (and lithostatic) forcing is based on observations, and not an outcome of this study. Taken at its face value, the study contradicts the observational evidence that the Antarctic Ice Sheet is not in a steady state. This is hardly a good starting point if modeling studies are to be taken seriously. As mentioned above, this study is a collaborative effort of a large group of people, undoubtedly they can find less equivocal ways to use these results.

We don't agree that from observations alone it can be concluded that grounding line retreat is only due to external forcing. Positive feedback mechanisms (such as MISI) can lead to a non-linear, irreversible response of the system that is sustained even when the external trigger is removed. In fact that is the entire objective of this modeling paper, to clearly determine whether the present-day observed retreat of the grounding lines is only due to external atmospheric/oceanic forcing, or is it also supplemented by an internal instability (MISI) that would continue even if the external forcing was reduced. If such a process is at play cannot be concluded from observations.
We agree that this could be clearer in the manuscript and we have made changes to the abstract and conclusion to alleviate any confusion on this point.

We completely agree that the current ice sheet is not in a steady state right now, and we hope that we never stated otherwise, even in the previous version of the manuscript. As we discussed above, we carefully designed the numerical experiments using steady states such that they allow us to make certain conclusions about the present-day state of the unsteady, real Antarctic Ice Sheet. Creating a steady state of the ice sheet in its current geometry is a prerequisite for conducting a stability analysis. In order to obtain a steady state for the models Elmer/Ice and Úa we had to apply a correction to the surface mass balance, which we describe in Section 2.2.1. However, we do not believe that our results are obtained because we used this approach. A strong indication of this is that we find no signs of irreversible retreat in our PISM simulations, for which the initial state was not generated using a correction to the surface mass balance. Also the correction fields applied to both Elmer/Ice and Úa are different from one another. Given that we have repeated our experiments with three different models and found our results are consistent across all models, we can be confident that our results are not dependent upon any particular choices made in each model.
We have added some additional sentences to the introduction to make it clear that: 1) we do not assume that the (real) ice sheet is in steady state, and 2) despite observational evidence that suggests present-day grounding line retreat is driven by external climate forcing, these observations alone are not sufficient to conclude that MISI is not also underway, hence our steady state numerical simulations.

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