Response to Reviewers

Coupled ice/ocean interactions during the future retreat of West Antarctic ice streams

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Black: Reviewer comments. Blue: Authors' response. Where necessary to clarify our response, we have added proposed paper correction in italics, including new text as underlined where we are modifying existing text.

Note to Editor and Reviewers: Correction of a modelling error

In addition to the changes in response to the reviewers, the simulations in this manuscript will be updated due to an error that was found in the model. We realised that the ice viscosity was not correctly averaged over the unevenly-spaced vertical sigma layers used by the WAVI ice sheet model. This led to an incorrectly low depth-average viscosity, due to over-weighting of the warm near-bed layers. Correcting this error required us to repeat the ice initialisation and relaxation procedures, as well as re-running all the projections. During this process, we also took the opportunity to implement a further change in that the bathymetry deepening described in the manuscript is now performed before initialisation and relaxation of the ice sheet model, rather than afterwards at the time of coupling to the ocean model. This change is more of a modelling choice than a correction, but we believe the new approach is more defensible; the deepened bed is now taken into account when the inversion procedure matches the ice state to observations.

We have thoroughly investigated the impact of these two updates and found that they change the results in this manuscript quantitatively but not qualitatively. This will lead to minor revisions throughout the manuscript and figures. In particular, the resultant higher viscosity leads to projected ice changes that are slower, but qualitatively the same. For example, Figure R1 below shows SLR rates over Thwaites area for the original and updated simulations. Note the new longer time axis compared to the original manuscript. The jumps in the SLR rate, which occur upon pinning point ungroundings, are still present in the updated simulations, but occur at a later date. In the revised manuscript, the time over which the simulations evolve is extended to include a similar retreated area to the original simulations and to cover ungroundings from the same set of pinning points. The longer timescales will be reflected fully in the revised paper.

We would like to sincerely apologise to the reviewers and editor for the additional corrections that are caused by the ice model viscosity error, and the additional delay in the review process that resulted from having to rerun the simulations.

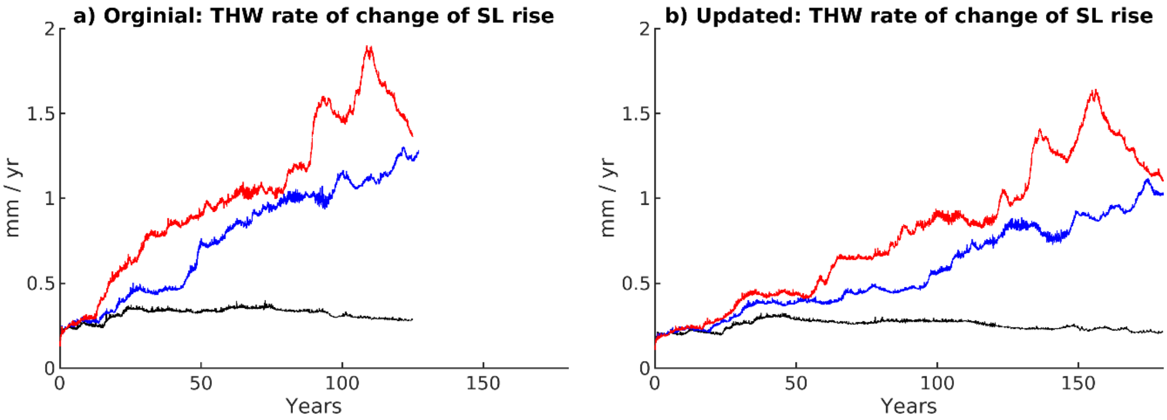

Figure R1 : Comparing the SLR rate from the original Thwaites area for the original simulations (a) and the updated ones after bug fixes (b), for the no melt (black), cold (blue) and warm (red) cases.

Response to Reviewer 1

Bett et al. presented the ice evolution of the Thwaites, Pine Island, and Smith Glaciers in the West Antarctic in a century scale using a new synchronously coupled ice/ocean model. Three couped simulations were conducted with warm and cold forcings and another one with no sub-shelf melting. They found the Thwaites Glacier provides a much higher sea-level contributions with a sea-level rise increasing approximately quadratically with time. The ice mass loss from Thwaites is closely dominated by the formation and duration of isolated pinning points and ocean-driven melting is the key driver behind the loss of pinning points. Overall, the manuscript is generally well written. However, the model setup section and some of the descriptions need more improvements.

We thank the reviewer for the time taken in reading and reviewing the manuscript and providing helpful comments and suggestions that will improve the manuscript.

Here are some general comments:

There are some details missing in the model setup section, especially about how the coupled model handles the grounding line movement. It is also not clear how the model facilitates the information exchange between the WAVI ice sheet models and the MITgcm STREAMCE every coupling period. Grounding line movement is a very important process in the coupled ice ocean models. However, the authors did not explain much about how they handle the grounding line movement in the coupled setup. You mentioned that (P5, L150) 'the WAVI drag field is passed to MITgcm to decide where melting can occur during each coupling period' and 'grounding-line retreat is accomplished naturally'. Do you mean the grounding line position is updated every ocean timestep and then passed to WAVI. Will the grounding line position be fixed in WAVI during each couple period? How do you decide the grounding-line should retreat in the ocean model?

We agree with the reviewer that the manuscript is currently unclear in its description on how the grounding line movement is handled and we welcome the opportunity to improve this. The manuscript will be revised to clarify that first we are explaining how the grounding line evolves in MITgcm only. The MITgcm package 'STREAMICE' is in control of when the grounding line is retreated in the ocean model due to previous coupling work between MITgcm STREAMICE and MITgcm ocean. This is fully documented in earlier papers (Jordan et al., 2018; Goldberg et al., 2018). Briefly, MITgcm solves the ocean free surface equation every ocean timestep in order to conserve mass. This naturally inflates the ocean beneath grounded ice wherever the ice thins so that its overburden pressure decreases sufficiently for the ocean to 'flood in' beneath the ice. A description of this will be added to the paper. In addition, we will expand the explanation of the coupled model procedure to explain that because to the WAVI timestep equals the coupling period, the WAVI state is fixed in between coupling periods (L176) and that the WAVI grounding line is only updated when MITgcm passes the new ice thickness calculated by STREAMICE (L180,182).

The boundaries between Thwaites, PIG, and Smith defined in Figure 1a are not reasonable to me. The following analysis on SLR contributions, melting and grounding ice area loss are all based on these boundaries. I would suggest using the basin boundaries to divide these three glaciers.

The areas used in this study (figure R2a) are modified from the basins in Zwally et al. (2012) (figure R2b), which will now be described in the manuscript. The areas used are motivated by the need to describe the qualitatively different future behaviour of Smith, Thwaites, and Pine Island glacier regions, and this requires us to modify the Zwally basins.

First, the Zwally basins do not separate out the glaciers surrounding Smith Glacier, so an approximate Smith area is defined by taking the far eastern edge of the Crosson Ice shelf down to Mount Takahe.

Second, the Zwally boundary between the PIG and Thwaites areas is inappropriate for future studies because the future Thwaites Glacier drains a significant area of ice that is allocated to PIG. Therefore the boundary between the two was edited by hand in order to address the otherwise mis-assignment of SLR contributions, melting and grounded ice area loss to PIG, as Thwaites retreats (Fig 3) past the original Zwally et al. (2012) boundaries in the simulations (Fig R2b). While this effect is relatively small it causes some qualitative issues with the results. For example, using the unmodified Zwally basins causes the PIG area to incorrectly display an increasing SLR rate (Fig R2e, f) and ice shelf melting (Fig R2 g, h).

This reasoning will be made clear in the manuscript when these areas are introduced, and the Figure 1 caption will mention that areas have been edited by hand.

"These areas are edited from the Zwally et al. (2012) basins, where the Smith area has been separated out. In addition, the boundary between the PIG and Thwaites areas is edited by hand to address the otherwise mis-assignment of SLR contributions, melting and grounding ice loss to PIG, as Thwaites retreats (Fig 3)."

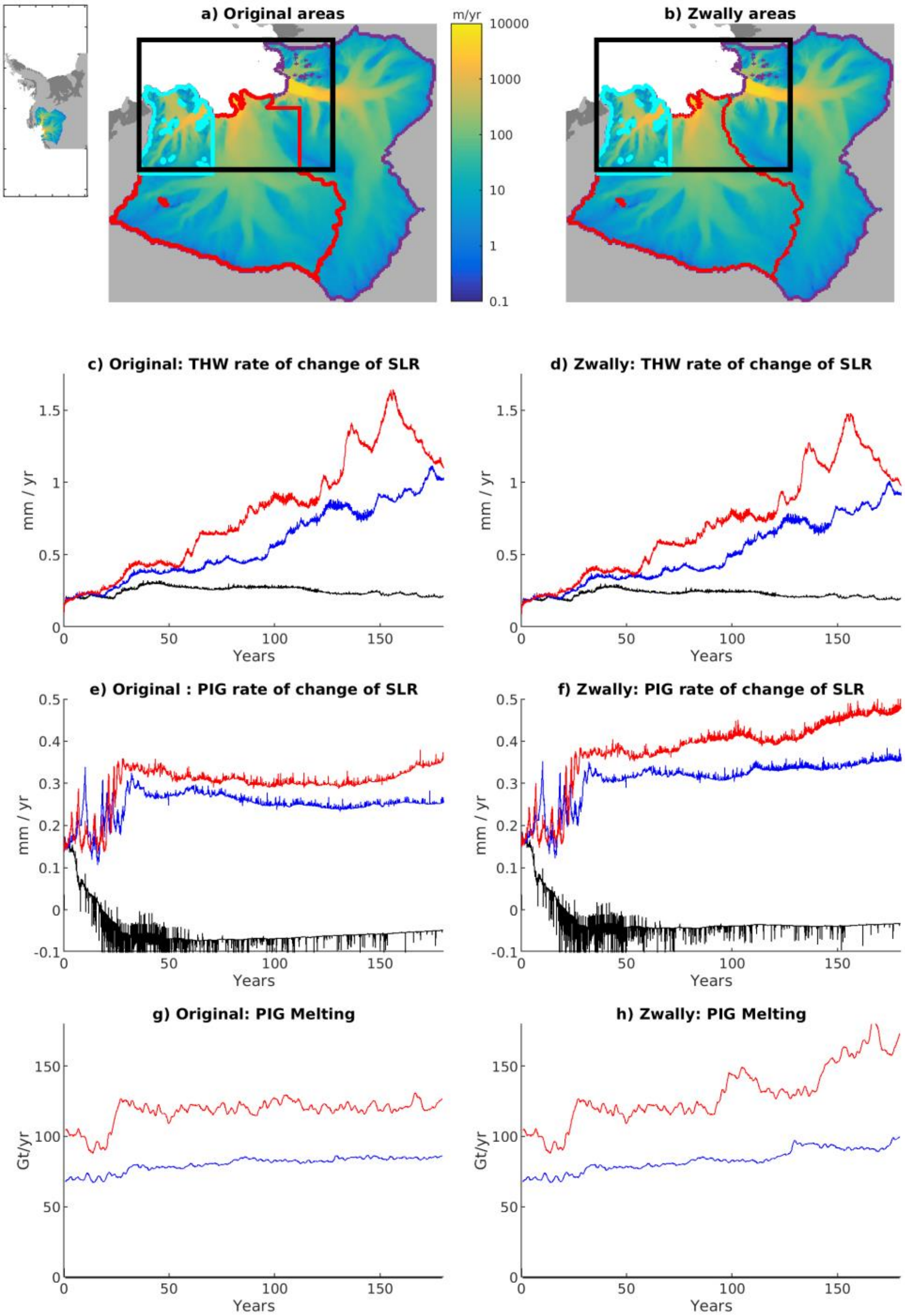

Figure R2: a) Original areas used in this manuscript, b) Zwally et al. (2012) areas with Smith area still separated out. Using the original areas: c) THW rate of change of SLR, e) PIG rate of change of SLR, g) PIG ice shelf melting. Similarly using the Zwally et al. (2012) areas d), f), h). All figures are plotted using the updated simulations after bug fixes.

The authors suggested that the melting rates around the pinning points will be difficult to be captured without a coupled ice/ocean model. I would suggest running a separate ice-only model with parameterised basal melting and compare the difference.

We thank the reviewer for this suggestion and we agree that this would be an interesting comparison to make. However, there are many different types of parametrisations of basal melting and they generally lack a strong physical basis and perform poorly when considered in spatial and temporal detail (Burgard et al., 2022). Furthermore, the parameterisations would have to be subjected to a comprehensive tuning effort in order to obtain a fair comparison. This means that robustly performing this comparison would require an extensive study of multiple parameterisations, and therefore we suggest that it would be out of the scope of the present manuscript, which already covers a substantial amount of material on the coupled model. This would however be very interesting work for a future study. We do however show that the ocean model creates a strong spatially variable melt field around these pinning points which would be hard to replicate with a basal melting parametrisation (Burgard et al., 2022). Text will be added to the discussion to explain this (L520).

"Therefore, as parametrizations of basal melting generally lack a strong physical basis and perform poorly in spatial detail (Burgard et al., 2022), they may be expected to struggle to capture this spatial variability, affecting the evolution of these pinning points and the resultant SLR contribution rate from Thwaites. However, future work is required, running sets of ice only simulations, to perform an extensive comparison of the wide range of basal melt rate parametrizations."

Specific Comments:

P3, L73: please provide the details (data name, version number, etc.) about the datasets (ice velocity and thickness change rates) for the inversion.

These details will be added to the manuscript.

P3, l79: It's not clear to me how you configure the englacial temperature in your ice model. I understand you cite Arthern et al., 2015 to cover the ice model setup but you should at least mention how the ice flow equation is represented in your ice model.

It will be made clearer in the manuscript which dataset is used for the englacial temperature (L85). In addition, a sentence will be added to state the ice flow law used in this study, which is the Glen flow law, along with the approximation to Stokes' equation that is solved (L77).

"The ice rheology is described using Glen's flow law, with an exponent of n = 3. WAVI is a finite volume ice sheet model including a treatment of both membrane and simplified vertical shear stresses as described by Goldberg (2011)."

P4, L97: eastern à western?

Thank you for spotting this and it will be fixed in the manuscript (L111).

P4, L105: When you say the initial melt rates, do you mean the melt rates you gor from the ocean only simulations at end of the 2 years spin-up?

That is correct, and will be made clear in the manuscript, by clarifying with "*after the 2 year ocean only spin-up"* (L120).

P4, L111: About the 'coupling shock', do you mean the sudden changes in the water fluxes across the ice/ocean boundary? Could you explain it in the text when you first mention it?

The 'coupling shock' is the response of the ice model to any mean offset between the MITgcm modelled ice shelf melt rates and those that are implicit in the WAVI initialisation. This mismatch is a model artefact. Given that projections are highly sensitive to their initial state, the response to any such shock could play a leading order role for many decades, contaminating the projections (Goldberg and Holland, 2022). This is why we have taken measures to minimise the coupling shock. This description will be expanded where it is first mentioned in the manuscript (L126).

"For PIG and Thwaites ice shelves combined, this value produces the closest average match between the initial MITgcm ice shelf melt rates and those that are implicit in the WAVI initialisation (see Appendix A). This minimises the 'coupling shock' - the response of the ice model to any mismatch between these two fields -, which occurs when the coupled model simulation commences. Without this calibration, the ice sheet trajectory could be impacted, potentially for many decades (Goldberg and Holland, 2022), by the adjustment of the ice due to the transition from implicit initialised melt rates to arbitrarily different ocean model melt rates at the start of the simulation."

P4, P114: what is the value derived from observation?

This sentence will be updated for the latest simulations and the value derived from observations will be included, which is 0.01 (L132).

P5, L 130: why do you set the subglacial layer to be 4 m thick. Is this a empirical value from previous studies?

The subglacial layer only exists in order to enable the expansion of ocean cells during ice retreat (Jordan et al., 2018; Goldberg et al., 2018). Therefore, this thickness is just required to be small compared to the ocean model vertical grid resolution, and 4 m is chosen for convenience. Text will be added to clarity this point (L 160).

"In regions of ice which are not floating, a thin subglacial layer is specified in order to enable the expansion of the ocean column during grounding line retreat (Jordan et al., 2018; Goldberg et al., 2018). We set it to be 4 m thick but could have been set to any relatively small thickness compared to the ocean model vertical resolution. This small value has been previously demonstrated to have no impact on the evolution of the coupled system (Goldberg et al., 2018)."

P5, L135: that in the 3D ocean grid that would never go afloat à that would never go afloat in the 3D ocean grid.

These will be swapped around as suggested.

P5, L138: how do you decide the couple domain is 'far enough'? Did you justify it based on a previous projection for this region?

The coupled domain was determined by running test simulations to find the extent of the modelled grounding-line retreat during the coming centuries. Text will be added to explain this in the manuscript (L172).

"The coupled domain only needs to extend far enough inland to accommodate the grounding-line retreat occurring during a projection, which for this study was determined using test simulations."

P5, L156: This information about bathymetry and initial ice geometry should be firstly mentioned in Sect. 2.1 and 2.2.

These will be moved as suggested, where the bathymetry deepening edits will be now introduced in Sect 2.2. The initial ice geometry is mentioned separately in Sect. 2.1.

P5, L160: what are 'velocity grid points'?

These will be clarified to be the Arakawa C-grid's velocity grid points (L138, 635).

P5, L161: when you talk about 'no ice basal drag', I understand you mean floating area. But in this study, you have two regions with basal drag: basal drag beneath the grounded ice and basal drag beneath the floating ice. Please clarify it across the text.

We thank the reviewer for spotting this source of potential confusion. The drag parameter used in the ice shelf melting will now be referred to throughout the manuscript as the “ice shelf melting drag” and the drag coefficient from the Weertman C sliding law will be referred to as the “ice sliding basal drag” or “sliding drag”.

P7, L179: delete the first ‘(e)’ here.

This will be deleted.

P8, L182: The colorbar for Fig3c melt rate is not good enough to show the increased basal melt near the new grounding line. Please adjust it to the visible range. Please also add the grounding lines in the caption.

The melt rate scale will be adjusted to better show the melt rates in Fig3c and the black/red grounding lines will be added to the caption description of this figure.

P9, L194: The Thwaites Glacier also shows a sign of deceleration by comparing the snapshots of 100 years and 125 years in Fig. 3b, which corresponds to the drop down in Fig 4b and 4c after year 100. I realise you mentioned this on P12L266-269, but I think you should at least point it out here and leave the explanation later.

This comment will be included as suggested for the new simulations (L242).

P9, L200: There are lots of noise on Figs. 4f and 4j but I did not see this noise in Fig. 4b. You ran the model for the whole domain and extract the rate of change rate of SL rise for each of the glaciers, right? Then why?

Some ‘noise’ is also present from the THW area, but it is harder to see due to this area being plotted on a much larger y-axis scale. Text will be added to highlight this (see below) (L 281).

“In all three areas ‘noise’ is present in the SLR rates, though this is harder to see for the Thwaites area due to the larger y-axis scale.”

P10, L220: For Smith, it is closer to 0.15 mm/yr rather than 0.1.

This will be corrected and updated for the new simulations.

P10, L227: It’s ‘almost’ 600 rather than ‘over’ 600. Actually, it is around 500 at end of 125 years.

This will be corrected and updated for the new simulations.

P10, L236-237: You mentioned that one of the limitations in this study was the idealised constant ocean forcings applied to the boundaries. Not just the decadal variability but also the climatology related changes under different emission scenarios in the future. How could you make the statement that ‘the future SLR from this region is only weakly influenced by variations within the plausible range of ocean conditions’?

The oceanographic ranges in the CDW thickness used in this study represent the range in the observations. However, as the reviewer points out future oceanographic conditions in the regions could be outside of these ranges, hence this statement will be edited to be “within the observed range of present day ocean conditions” (L 294). However, while the forcings used in this study are idealised, they are based on the best available information of present-day extremes in observations and approximate the bounds of the latest projections of future warming in the region (Naughten et al., 2023). Text will be added to the discussion to highlight this point (L535).

“However, while the forcings used in this study are idealised, they are based on the best available information of present-day extremes in observations (Dutrieux et al., 2014). These forcings also agree

approximately with the time average of recent projections of future warming in the region (Naughten et al., 2023), which found linearly rising trends of ocean temperature in all tested climatic scenarios. The steady warm and cold forcings used in this study have an average temperature between 200-700 m depth of ~0.35 ^{o}C and ~-0.45 ^{o}C respectively. The warm case approximates the time-averaged temperatures at this depth over the similar trends projected in 100 year simulations of the Paris 1.5^{o}C, Paris 2^{o}C and RCP 4.5 climatic conditions (Naughten et al., 2023), , while the cold case approximates a cold historical state (Naughten et al., 2023).”

P17, L361: ‘with only more modified CDW’? I think you mean ‘only limited CDW’.

Pure CDW is only found outside the Amundsen shelf sea. The cooler water mass on shelf is modified by sea ice formation and glacial ice melting, and so cooler CDW is referred to as being more modified. This will be changed to “*with only more heavily-modified CDW*” (L437).

P19, L389: the SLR rate did not continue over the 125-year time period based on Fig. 4b if you are talking about the red line.

This will be updated and corrected for the new simulations.

P19, L407: 2 km mesh near the grounding line is not seen as a very high-resolution model. A coarser mesh near the grounding line may have underestimated the mass loss in the marine ice sheet systems.

A study that is currently under review (Williams et al., PREPRINT)(https://doi.org/10.21203/rs.3.rs-3405435/v1) uses a nearly identical configuration of the WAVI ice sheet model and investigates the effect of resolution on grounding line retreat and SLR contributions from the Amundsen Sea Sector, finding that lower resolutions actually lead to an overestimation of SLR contributions (Fig R3). That study finds that the sensitivity is minimal from 4km to 2km resolutions, suggesting the 2 km resolution used in this study is an appropriate resolution. This will now be described in the ice model methods of the manuscript (see below), and text will be added to discussion section to highlight that model resolution is an important consideration and may be one factor that plays a role in differences between studies in the literature (L 482).

“We use a numerical model with a time step of 20 days and a 2 km horizontal resolution covering the whole Amundsen Sea sector domain shown in Figure 1a. <u>This horizontal resolution is found to be appropriate in a recent study, currently under review, testing the impact of resolution on grounding line retreat and SLR contributions, with a nearly identical configuration of the Amundsen Sea sector (Williams et al., PREPRINT) (https://doi.org/10.21203/rs.3.rs-3405435/v1).”</u>

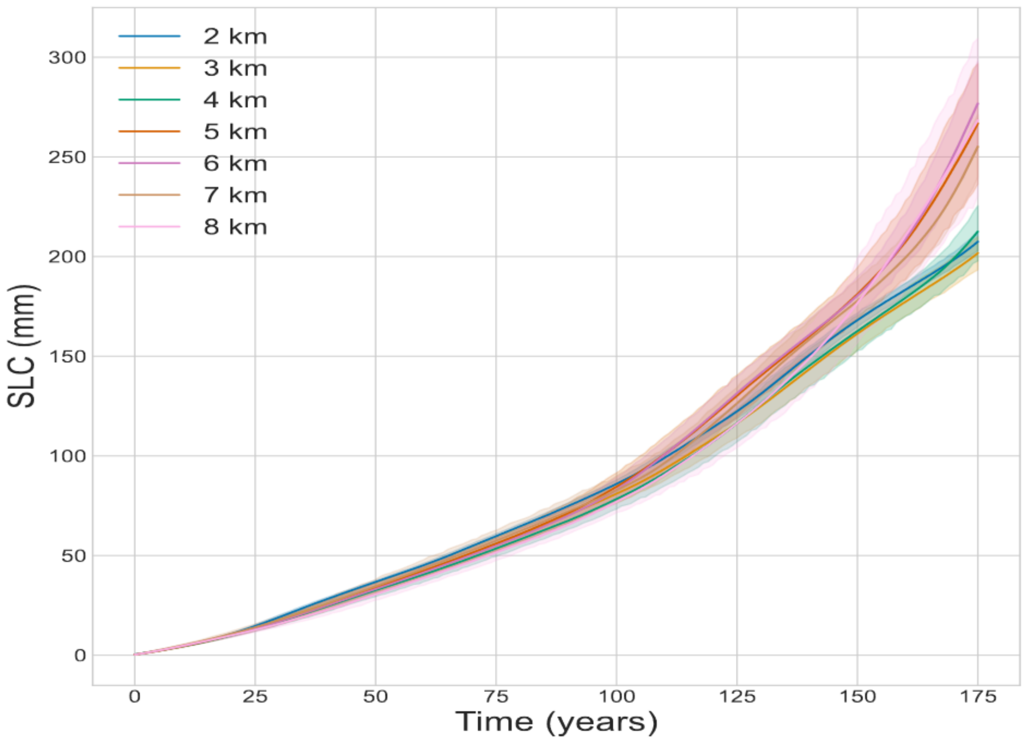

Fig R3: Taken from figure 2 in Williams et al. (PREPRINT)(https://doi.org/10.21203/rs.3.rs-3405435/v1). Sea level contribution (SLC) from the Amundsen Sea sector (mm) over 175 years for a range of model resolutions between 2km and 8km with prescribed average melt rates of 100 m yr^{-1}.

L20, P427: how about the subglacial freshwater discharge?

As suggested, a sentence will be added (shown below) to point out the potential impact of the lack of subglacial freshwater discharge, which could lead to underestimation of local ice shelf melt near the grounding line (L 551).

"These simulations additionally lack subglacial freshwater discharge, which at the grounding line has been found to increase ice shelf melting locally in previous ocean modelling studies (Nakayama et al., 2021), but overall, its affect is small in this region (Holland et al., 2023)."

L20, P438: The structure of discussion could be better organised. You're talking about another limitation here. It looks messy in the structure of the discussion. Why don't you talk about all these limitations together rather than separated by some other points like the paragraph above?

The discussion section will be rearranged as suggested with all limitations together and at the end of this section.

P21, L478: it would be interesting to see the differences by conducting the ice-only experiments with parameterised ocean-driven melting.

We agree that this would indeed be an interesting comparison to make. However, as mentioned above, due to the wide selection of possible melt rate parametrisations, an extensive comparison would be required, which we believe would be outside the scope of this study. A sentence has been added that this would be interesting future work and that more work is required in the comparison of coupled models and parameterised melting (shown with previous comment).

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