Response to Reviewers

Kreuzer, M., Albrecht, T., Nicola, L., Reese, R., and Winkelmann, R.: Oceanic gateways in Antarctica – Impact of relative sea-level change on sub-shelf melt, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2023-2737, 2023.

We thank all three reviewers for taking substantial time and effort to read and comment on our manuscript. The given comments are of constructive nature and very valuable to considerably improve the manuscript.
In order to reduce duplicated comments we grouped and summarized the general comments of all three reviewers and will respond to them in the section below. Furthermore, we respond to all specific comments individually (see further below).
All our comments are displayed in blue color, while the reviewers comments (directly copied or paraphrased) are displayed in black. Original text from the preprint is shown in *black and italics*.

General comments

- Scientific value and goal of study (RC1, RC2, RC3)
    - “The scientific value is difficult to assess, due to: the methodology limits; the application only to the actual grounding line configuration; and a lack of development perspective in the application of the method to more realistic studies.” (RC1)
    - “It is not very clear what the study wants to achieve, since the ‘g’ parameter remains free and the grounding line is kept at the present-day position. If the purpose is to produce a conceptual model I suggest strengthening the methodology to take into account a more accurate present-day oceanographic setting, which is also a key input for PICO.” (RC1)

The goal of our manuscript is to estimate the maximum impact that relative sea level (RSL) change can possibly have on sub-shelf melting of the Antarctic Ice Sheet. So far, this relation has not been assessed or considered in ice-ocean modeling and we want to provide a first estimate on the importance of this mechanism. In our study we are not trying to pin-down exact basal melt rate differences at specific time slices. Instead, the LGM15ka and Icefree scenarios are enclosing the maximum range of expected RSL changes on time scales of glacial cycles, which are of interest to the ice-sheet modeling community. By estimating basal melt rate changes for these end-member configurations on a present-day ice-sheet state, we inferred a sensitivity measure providing upper and lower bounds of this process. However, in more realistic scenarios, when other important features are subject to change (like the grounding line position or the far-field climate forcing), the overall effect of RSL change on melt rates might be reduced. Further below, we will comment more on the reasoning to use a present-day ice-sheet configuration.
We have realized that the current manuscript does not convey this message clearly enough and

apologize for any consequential misunderstandings. Speaking of different “RSL scenarios” for example does not reflect the methodological concept very well and we will therefore change this to “RSL configurations” in the revised manuscript. Furthermore, we strive to adapt the overall framing of the paper and be more clear about the motivation and scope of the study. We are also considering to change the title of the manuscript accordingly.

Concerning the “free evolving parameter g”: Initially we have used critical access depths for a value of g=50% in order to ensure that a “significant” amount of the grounding line is reached by topography controlled inflowing open ocean water masses. However, it is difficult to define a universal percentage of the grounding line that needs to be accessed by inflowing water in order to dominate the overall melting. Therefore, we decided to expand the analysis for the wider range of grounding line coverage from 10-90% and check for the dependency of our results with respect to the percentage of grounding line reached. We believe that this can be useful in order to evaluate the maximum possible effect of RSL change on basal melt rates. However, we agree with the reviewers that this part of the methodology needs more consideration, when trying to determine definite basal melt changes for a specific scenario, rather than merely estimating the maximum possible effect. In the scope of current revisions for Nicola et al. (in review), we also explore possibilities to reduce the g-dimension into an appropriate single value per basin, which will possibly result in an update of the methodology of our paper as well.

- Meaning of Icefree scenario (RC1, RC2, RC3)
    - Basal melt change in icefree scenario is meaningless without ice (RC1, RC2, RC3)
    - useful as a maximum estimate (RC1)
    - Need to specify the usefulness of this scenario for future science (RC3)

The idea of integrating an icefree configuration for RSL was to yield an upper bound for the far-field sea-level rise on the one hand, and superimpose a strong near-field GIA uplift signal on the other hand. At first sight it may seem odd to calculate basal melt rate changes for such a RSL configuration, where the Antarctic Ice Sheet has been melted completely. In fact, this RSL (and bed topography) configuration could be the initial condition for a glaciation of an Antarctic Ice Sheet, where basal melt rates should account for different access depths.
As pointed out above, the intention of using end-member RSL configurations in our analysis is to enclose the maximum realistic change that can be expected on paleo time scales. In order to isolate the effect of RSL change from other mechanisms on basal melt rates, it is necessary to compare to a common baseline. This is why we apply the different RSL configurations to a present-day ice sheet and keep all other processes fixed that are subject to change on these time scales, like grounding line position, cavity geometry, ocean forcing conditions, etc. With this method we are able to derive an upper bound estimate of the RSL change influence on basal melt. For more realistic estimates that correspond to specific time slices, a matching ice-sheet geometry should be consistent with the applied RSL configuration. We comment more about this in the paragraph below.

- Proposal for transient simulations with evolving grounding line configuration (RC1, RC2, RC3)
    - more realistic application like grounding line configuration/position (RC1)
    - More value, if running a transient simulation (deglaciation or extended SSP) with/without relative sea level feedback (RC2)
    - An assessment on the evolution of the importance of certain gateways for the deglacial AIS retreat would make the study of greater scientific value (RC1)
    - Missing perspective for further application (RC1, RC2, RC3)
        - lack of development perspective in the application of the method to more realistic studies (RC1)
        - How applicable is this method for transient ice sheet simulations? (RC2)
        - Required: transient simulations with dynamic adjustment of critical access depths and according modification of ocean temperatures (RC3)

We agree that it would be very valuable to assess the impact of RSL change on basal melt rates for transient evolving grounding lines. This would help to get a more realistic estimate on the importance of the mechanism, which is possibly smaller than the maximum estimate that we assess for the end-member configurations in our study.
For paleo simulations, ice sheet models often use an index method, where ocean temperatures are scaled with a paleo proxy, like δO_{18} or temperature reconstructions from ice cores (e.g. Albrecht et al., 2020). In case of RSL basal melt corrections for transient runs, this temperature correction needs to be combined with the approach for computing RSL induced effects on basal melt rates used in this study.
When preparing the revised manuscript, we are planning to do this for multiple time slices for a transient deglaciation run from LGM to present-day. While doing so, we can then assess what the RSL induced impact is on basal melt rates, not only for a present-day ice sheet configuration, but also for evolving grounding line geometries. With the offline computed RSL correction, we can then also re-run a deglaciation scenario for the LGM configuration and see whether the transient ice sheet response diverges from the run without including RSL induced corrections.

Introduction

- “There is a lack of description of the oceanographic setting and gateways at present, which is a key point of the paper and would inform the reader on how far the method would be applicable to present and past scenarios.” (RC1)

So far, we have not included a thorough discussion of present-day gateways and the corresponding ocean conditions as this is covered in depth in the Nicola et al. (in review) paper. We agree that it would help the reader to judge the applicability and limitations of our methodology if more information/discussion about the different modes of melting is included in the manuscript. At the same time, we want to minimize overlap with the related study of Nicola et al. (in review). We will consider this comment when preparing a revised manuscript.

- Missing: effect of GIA on ocean dynamics (RC3)

Giving more information and context about the effect of GIA on ocean dynamics is a very good suggestion and adds valuable context to the study. We thank the reviewer for pointing this out and will include studies like Rugenstein et al. (2014) or Lowrey et al. (2024) in a revised version of the manuscript.

- transform Figure 1 into a conceptual figure to reduce complexity while illustrating the bathymetric elevation changes and their effect on access depths (RC2)

Thanks for this suggestion. We agree that a conceptual figure with reduced complexity would be more suitable to convey the basic concept and methodology of our study. We will endeavor our creative skills and try to improve Figure 1.

Methods

- Keeping grounding line at present-day location is inconsistent with applied scenarios and therefore lacks scientific significance (RC1, RC2, RC3)

As explained above, we think that it is useful to apply different RSL configurations and their derived changes in thermal forcing on a present-day ice-sheet configuration to roughly quantify the maximum possible effect on basal melt rates in first order. In order to attribute changes to the effect of RSL only, it is important that other conditions that influence basal melt rates do not change within the comparison. In such a sensitivity experiment, it would therefore not be useful to compare basal melt rates derived for a present-day ice-sheet configuration with ones from an LGM state that includes not only RSL induced changes, but also features a different grounding line position, cavity geometry and ocean forcing. In such a case it would be unclear which changes can be attributed to the different ice sheet state and corresponding climate forcing, and which ones are due to changes in RSL.

Instead, one would need to compare basal melt rates for an LGM scenario with and without the effect of RSL. This requires a meaningful correction of the 3-dimensional ocean forcing field (ISMIP6 dataset by Jourdain et al., 2020) we use for present-day to LGM conditions. For the icefree case this comparison is not possible as we obviously cannot compute basal melting for a non-existing ice sheet. When preparing a revised version of the manuscript, we will explore possibilities to use a LGM ice-sheet state to directly compare basal melt rates with/without the influence of RSL.

- Critical access depth definition is not fully understandable/defined including grounding line coverage parameter g (RC1, RC2)

We apologize that the description and explanation of “critical access depth” and the grounding line coverage parameter g was apparently not clear enough in the manuscript. We will revise the manuscript and provide a more detailed explanation.

- Inconsistencies of input T,S between this study and Nicola et al.: Continental Shelf Break vs Calving Front (RC1)

In our study we use the same methodology as Nicola et al. (in review) to compute critical access depths. As the reviewer correctly points out, the further methodology diverges partially. The

preprint of Nicola et al. for example uses the temperatures at the continental shelf break (CSB) and calving front (CF) directly as input to PICO. In our study we apply an anomaly approach instead, where we compute the difference of CSB temperatures at different depths and add this to present-day ocean forcing derived in Reese et al. (2023). The mentioned discrepancies of input temperatures are resulting from different scientific questions in the two manuscripts and are not inherently inconsistent.
In the context of revisions for Nicola et al. we are currently discussing changes to their approach. Nevertheless, we will make sure that in a revised manuscript, we state more clearly where the methodology of Nicola et al. and our study is the same, and where it diverges. We thank the reviewer for pointing this out and apologize that missing information about this led to misunderstandings.

- Adding CSB anomalies to CF/PICO input values is not appropriate especially for shelves that are not “warm” after Thompson et al. (2018) (RC1):
  “PICO would need to be forced by realistic water masses at the calving front, and employing shelf break temperature and salinity, even only as anomalies with respect to the present day, is not representative of the water masses entering the cavities. The only case may be for “warm” type continental shelves (Thompson et al., 2018), where the CDW is actively pushed towards the ice shelf cavities by winds and by dynamical processes in the Along-Slope Front such as an Eastward flowing undercurrent (Silvano et al., 2022). The method could work in specific locations on “fresh” shelves (Thompson et al., 2018), after applying some corrections to take into account mixing of CDW into “modified” CDW (mCDW), which also tilts the isopycnals on the shelf break (may think of extrapolation along isopycnals). As for melting in multimodal cavities (e.g., Tinto et al., 2019), melt by mCDW usually occurs at mid-depth, while the grounding line mostly melts with mode cold salty water (Mode 1, Silvano et al., 2016; Herraiz-Borreguero 2015). These features are not accounted for, and the methodology misrepresents the impact of mCDW in these cases, since there is no direct connection between the mCDW and the grounding line. Also see e.g. Herraiz-Borreguero (2015), usually only the Eastern side in multimodal cavities is affected by mCDW, while here the anomalies are applied to the whole basin. Therefore the method, although simplified, would be fully applicable to “warm” continental shelves found mostly in West Antarctica.”

Thanks for the elaborated comment about the applicability of the methodology. We are aware of the fact that applying continental shelf break anomalies directly inside the cavities is a broad assumption and might overestimate the effect of water mass changes.
As described in Thompson et al. (2018) shelf modes are also controlled by topographic barriers (not exclusively though). Therefore, changes in RSL can also affect the dominant mode of melting in the cavity and how much warm CDW can make it onto the continental shelf. While we don’t account directly for such a change in modes, we assume that the CSB anomalies are the maximum possible changes that can be reflected inside the cavity. Therefore, the approach is suitable when trying to assess the maximum possible range of change.
Reese et al. (2018) show that PICO can produce realistic circum-Antarctic mean melt rates, independent of shelf mode after Thompson et al. (2018). The PICO parameters and present-day input temperatures are re-tuned in Reese et al. (2023) to match melt rate sensitivities and

historic ice loss.

- Thicker layer of CDW intrusion will have an impact, even if d_c is below thermocline (RC1)

Indeed this is a clear limitation of our methodology. We thank the reviewer for raising this point and make sure that this is adequately discussed in a revised version of the manuscript.

- Explain the simplifications of the used methodology (e.g. compared to using dynamic ocean modeling to assess changes ocean temperatures) and evaluate its validity (RC3)
- Lack of evaluation of the validity of the use of a simplified method to study the effect of relative sea level on ocean temperatures instead of using an ocean model (RC3)

Unfortunately we don't have the means to test and validate the findings of our simplified methodology with high resolution ocean modeling. However, we would be really interested whether our results can be confirmed also with other methods. In a revised manuscript, we will add the need for validation of our findings with high-resolution ocean models.

- "Ocean access does not solely work via the deepest gateways but arguably most of the warm water e.g. in the Amundsen Sea is channeled via these gateways, does your approach also reflect the change in basal shelf melt rates at the grounding line for the bulk advection of CDW/mCDW etc. across the continental shelf and into the cavity?" (RC2)

The water channeled through the deepest connection between the continental shelf and the open ocean (oceanic gateway) is represented via the lowest grounding line coverage. In our methodology this is represented by reaching at least the deepest 10% of the grounding line. As shown by the magenta boxes in Fig. 3 in Nicola et al. (in review) the deepest connection can reach vast amounts of the grounding line in case of a "oceanic gateway"/trough, e.g. up to 75% in the Filchner-Ronne basin.
However, of course also shallower connections exist (e.g. above the magenta boxes in Fig. 3, Nicola et al., in review). At these depths, most of a region's grounding line can be accessed by water from the open ocean/continental shelf and thus represented by higher grounding line coverage.

Discussion

- Missing: applicability only to "warm" and maybe "fresh", not "dense" shelves -> PICO limitations (RC1)

Please see our comment about PICO above.

- How would this approach be applicable in time-evolving ice sheet simulations? What are caveats and limitations, e.g. changing ocean forcing over time? (RC2)

Please also see our general comment about transient ice-sheet simulations including RSL induced corrections above.

A big limitation is the availability of time evolving 3-dimensional ocean temperature and salinity forcing in sufficient spatial resolution. For paleo simulations, ice-sheet models often use an index method, where present-day observations of ocean temperatures are scaled with a paleo proxy, like δO_{18} or temperature reconstructions from ice cores (e.g. Albrecht et al., 2020). This can for example be 2-dimensional (spatially) resolved observations on the continental shelf, which serves as input to PICO. However, when accounting for RSL induced changes over time, we need spatial 3-dimensional (depth dependent) ocean data to derive vertical RSL corrections at the continental-shelf break. Thereby the question arises whether the climate index method has a depth dependency or can be used uniformly as a scalar offset to the whole column. For the latter case, we can use present-day ocean data like from Jourdain et al. (2020) and scale it similarly to the PICO input in Albrecht et al. (2020).
If we will use such corrections in an example of a transient simulation in the revised manuscript, we will discuss the caveats and limitations of this approach.

- More assessment of importance of RSL effect on melt-rates, e.g. vs changing ocean forcing (first or second order effect)? (RC2)

In our study we discuss that the maximum effect of end-member RSL configurations can have an effect on the thermal driving in the same order of magnitude as changes in ocean forcing on paleo time scales. Nevertheless, it could well be that the RSL influence is still a second order effect, in case the actual influence of climate induced changes is stronger than the modulation through RSL. For example in an LGM state, when the overall climate forcing produces little to no basal melt compared to present-day, the RSL effect would be negligible and non effective. However, it can become important during the deglaciation period and affect the timing and evolution of the ice mass. As laid out above, we strive to extend our experiment setup in order to make some quantitative statements about this in a revised manuscript.

- Missing sensitivity of Earth rheology parameters on result (RC3)

The 3D Earth rheology used shows a high sensitivity to glacial cycles load changes at the upper end of tested 3D and 1D rheologies (Albrecht et al., 2023, in review). This fits our "maximum sensitivity range analysis" approach. We will add a discussion about the effect of different Earth rheology parameters in the revised manuscript and thank the reviewer for pointing this out.

- How computationally costly is the method? (RC2, RC3)

We will include information about the computational cost of the flood-fill in a revised manuscript. We thank the reviewers for pointing out that this information is missing and agree that it can be of interest to the reader.

Specific comments of Reviewer 3 (Caroline van Calcar)

Line 10-12: It would be easier to understand if this sentence were to split up in two parts. Include in the first part of the sentence why including relative sea level leads to a decrease of present-day sub shelf melt rates at last glacial maximum. Then explain how subsidence of bedrock can lead up to a doubling of basal melt rates. Also include which time period was simulated.
Thanks for the comment. In the revised manuscript we are planning to change this to:
"For the global sea level lowstand at the Last Glacial Maximum, this effect would lead to a substantial decrease of present-day sub-shelf melt rates in East Antarctica. In contrast, strong subsidence of bedrock in West Antarctica, and hence locally a much higher relative sea level at LGM, could lead to a doubling of basal melt rates."

Line 65: Include the reference Gomez et al., journal of climate, 2018, among the references to coupled 3D GIA – ice sheet models.
Thanks for the suggestion. We have added the reference to the manuscript.

Line 82: Figures S1-S3 have not been mentioned yet so Fig. S4 should be renumbered to Fig. S1.
Thanks for spotting this. We will make sure all the supplement figures are mentioned and in the right order as occurring in the text in the revised manuscript.

Line 85: I miss information on what is already known about bedrock deformation in relation to ocean dynamics and the effect on ocean temperature. See general comment.
As mentioned in the general comments, we think that giving more information and context about the effect of GIA on ocean dynamics is a very good suggestion. We thank the reviewer for pointing this out and will include studies like Rugenstein et al. (2014) and Lowrey et al. (2024) in a revised version of the manuscript.

Line 98: Since the basin numbers are used to discuss the results it would be useful to include a figure showing the basins and the number of each basin.
The basins are shown in Figure 2c and are referenced with the corresponding number. The last paragraph of the introduction gives a brief outline about the rest of the manuscript. This is why we did not include the reference to Fig 2c there. We will resolve this in the revised manuscript.

Line 104: This line states that ice-shelf basal melt rates are estimated from relative sea-level changes, whereas that is not the case. I suggest to change "from" to "including".
This study basically provides an upper estimate of the sensitivity of (PICO) ice shelf melt to possible vertical bathymetry changes. We changed the phrasing now to:
"*This section describes the methods, scenarios and workflow we use to derive ice-shelf basal melt rate estimates by applying different relative sea-level change configurations."*

Line 120-122: The use of the present day ice geometry to compute basal melt rates is stated in line 120-122 but the implications of this simplification should be discussed. Discuss how this effect would be different if the method were to be applied with an evolving grounding line.
Thanks for the comment. The discussion currently features a section where we explain the use of a present-day ice sheet configuration. But we agree that it should be expanded by the expected differences when featuring an evolving grounding line and ice-sheet geometry. We will include this in the revised version of the manuscript.

Line 129: The computed regional sea level is highly dependent on the Earth rheology used in the GIA model so these parameters should be described in more detail and the reference should state which rheology from Bagge et al. is used. A figure of the lithospheric thickness and the viscosity should be included in the supplementary materials.
Thanks for the comment. We agree that the used Earth rheology parameters are important information for the reader. This is all described in detail in Albrecht et al. (in review), which was not submitted yet at the time we submitted the manuscript. So in the revised manuscript, we will refer to Fig. 5 in Albrecht et al. (in review) and add the rheology parameters used from Bagge et al., 2021.

Line 131: Is the coupling interval also 100 years to compute relative sea level for the yr2300 scenario? If so, include in the text how this time step choice effect the results (since GIA feedback could occur on much time scales than 100 years in regions with low mantle viscosity and thin lithosphere).
The coupling time step between PISM and VILMA was 1 year in the yr2300 case. We will include this information in the manuscript. A detailed discussion of this coupled sea level projection until 2300 will be subject to a follow-on study..

Line 131-133: I suggest to include in this sentence that the coupled simulations are conducted for Antarctica. The ice loading history of the northern hemisphere is mentioned later in section 2.5 about LGM15k. I would remove it from line 132-133 because it is confusing that northern hemisphere loading is only mentioned for the LGM15k scenario and not for the other two scenarios.
Thanks for the suggestion. VILMA always has a global setup and needs an ice-sheet history as input, which affects the far-field RSL change. We have added a note that PISM is used to simulate the Antarctic Ice Sheet and refrain from mentioning the ICE6G_C reconstruction already in Section 2.1.

Line 134: The Earth's rheology determines relative sea level and can differ significantly dependent on the rheology used. Please include a figure of the lithospheric thickness and the mantle viscosity that is used for this study. Also state the resolution of VILMA and PISM here.
Thanks for the comment. As mentioned above, we will take care of this in the revised manuscript. Model resolutions are discussed in Albrecht et al., (in review): "*The default resolution in our coupled simulations is 16km in the Antarctic Ice Sheet and 0.2° for the global sea level equation (n512), which corresponds to 20km in latitude and to about 6km in longitude*

at 71°S. The viscoelastic deformation is resolved with 0.7° (n128), which corresponds to about 78km in latitude and 25km in longitude at 71°S."

Line 136-138: Please explain in more detail what is done in this step. It is unclear why present-day topography would need to be updated instead of 15ka, 2300 and the final ice free time step.
Thanks for the comment. We apply the bed topography correction as follows:

bed(LGM) = bed(PD) - delta(RSL(LGM))

We will include this equation in the manuscript, as this is useful for clarification.

Line 149: Define "deepest grounding line fraction".Line 161-162: Please include how the present day control conditions exactly determined for each scenario?
We calculate the critical access depths d_c by looping through the depth column in steps of 1m, starting at -1800m until reaching the surface (0m). For each depth level and basin, the algorithm checks which percentage of the grounding line can be reached by evaluating the access depth map at the grounding line. Hence, for g=50% for example, the critical access depth d_c(g=50%) is the level, where 50% of the grounding line points in the entire basin have an access depth at the same level or deeper. We do this calculation for all basins and the whole range of g values (10%, 15%, 20%, …, 85%, 90%).
We will propose a reworked explanation of this methodology in the manuscript.

Line 180: Please clarify how the initialization of PISM is related to the computation of basal melt rates. Also include over which time period PISM is run to compute basal melt rates. It is stated that the results are regridded to 4 km resolution but more interestingly would be to indicate at which resolution PISM is run.
Here "initialization" refers to the diagnostic PICO simulation within PISM for a given geometry. So we are initializing PISM with the Bedmachine topography and ice sheet thickness on a 4km resolution. Then we run PISM for only one diagnostic time step (the geometry remains constant), in order to compute basal melt rates with PICO under the applied oceanic boundary conditions. Our methodology does not include any dynamic ice sheet simulations for the evaluation of basal melt rates. We will rephrase the related paragraph to avoid confusion.

Line 183: Please include in this section at which moment in time step 2, 3, and 4 of the method are computed for each scenario.
Section 2.5 introduces the different RSL configurations used for the analysis. On the basis of these different configurations the steps 2-4 (method section) are performed. We will adapt the beginning of the subsection in the manuscript as follows:
"This subsection provides more information about the RSL configurations (step 1) that are used as input to the subsequent methodology (step 2-4)."

Line 185: This line states that the scenario is preceded by a spinup of two glacial cycles, but it is not discussed over which time period the coupled model is run to produce the final results used to compute the critical access depths. Also explain if the initial topography was inverted for differences with present day observed topography when conducting the glacial cycle runs. Furthermore explain which forcing has been used.
The RSL changes are extracted from coupled PISM-VILMA simulations over two glacial cycles with six iterations to correct for initial topography. This is further explained in Albrecht et al., (in review), but we will give more information in the revised manuscript. Climate forcing for ocean and atmosphere is analogous to Albrecht et al., 2020a.

Line 195: Include over how much time the ice load is removed or if it was removed instantly.
For the icefree configuration, we remove the ice instantly and then compute the solid Earth response for 86 kyrs to account for long-term equilibration.
We will clarify this in the manuscript.

Line 195-199: Please also mention the initial conditions for this scenario in terms of ice geometry and topography.
The icefree simulation has been initialized from the Bedmap2 geometry and topography. We will add this information to the revised manuscript.

Line 199-200: Indicate the initial conditions of the coupled model at the year 1850. Indicate for how many years the historical run last. Also, please show the RSL change rates over the historical period in a figure.
The historical simulation initialized from a state at 1850, which has been obtained in an equilibrium spin-up (100kyr thermal spin-up, 25kyr full physics) for pre-industrial climate conditions and is run for 175yr until 2015 (Reese et al., 2023). As these experiments will be described in detail in a separate study, we have not shown many results in the manuscript so far. But we will add a plot showing the RSL changes for the historical period in the supplementary material (see Fig. 1 below).

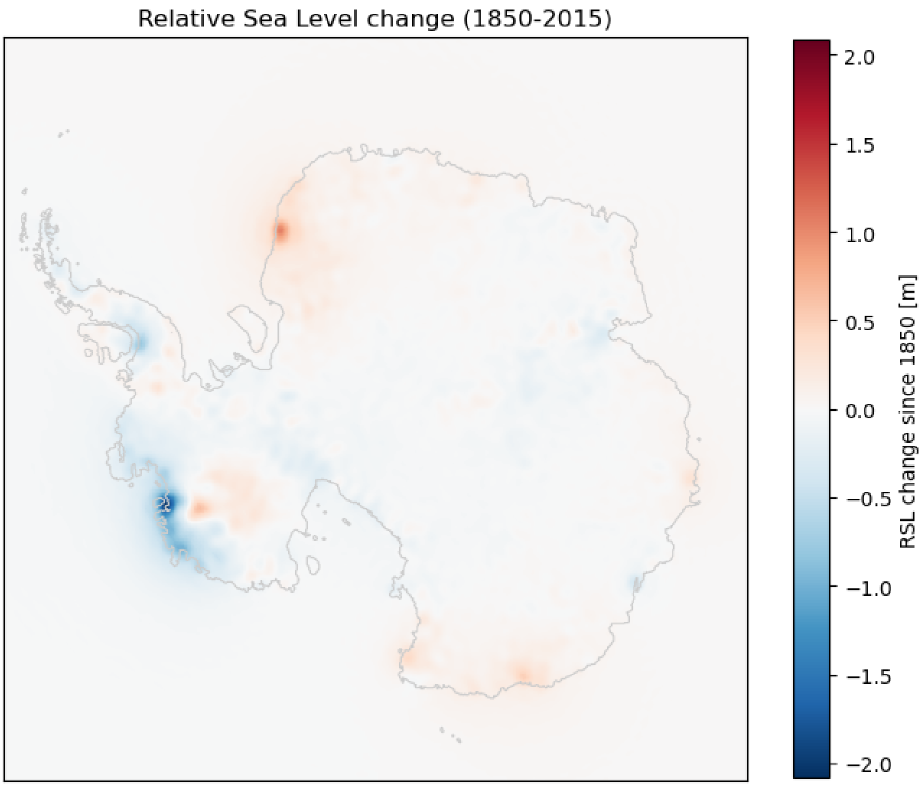

Figure 1: Change of relative sea level computed by PISM-VILMA for the historic period 1850–2015

Line 205-210: Is the GMSL contribution excluding the AIS component added linearly? Please state in the text how this contribution is applied.

We first compute the RSL change by the coupled PISM-VILMA simulation, where PISM is forced by the ISMIP6 2300 extension forcing. The coupled PISM-VILMA simulation is unaware of any cryospheric changes that are not covered by the Antarctic ice sheet instance of PISM. Therefore we add the uniform +3.68m in global sea level rise to the RSL change pattern derived from PISM-VILMA afterwards, in a post-processing step. We will clarify this in the revised manuscript.

Line 226-228: Clarify which additional ice load is meant.

Thanks for the comment. We are referring to the increased ice load of the Antarctic Ice Sheet during the LGM configuration here.

Line 279: Is the change a decrease? Please specify.

In the described case (*LGM15k*) there is a far-field sea-level fall (due to increased ice masses on land). But regionally in the Amundsen Sea region, the local relative sea level still increases by several hundred meters due to the grounding line advance and subsequent bedrock depression. By “counteracting the far-field sea-level fall” we mean that the bedrock depression overcompensates the far-field drop in sea level, which leads to a net increase in relative sea level. We have clarified this in the manuscript by replacing “counteracting” with “overcompensating”.

Line 286-288: I suggest to include a detailed description in the method section.
Thanks for the good suggestion. We will adapt the manuscript accordingly.

Line 288: Please add a reference to figure S3. Does the horizontal adjustment change the shelf area over which basal melt takes place? If so, include whether increased basal melt rates could be caused by an increase in ice shelf area.
Thanks for the comment. The horizontal grounding line adjustment changes the ice-shelf area only slightly, which won’t have a significant effect on the mean basal melt rates. We have added the references for Fig. S3 to the manuscript.

Line 293: Please define “overflow depth”.
Thanks for the comment. We will make sure that this term is defined properly if we still use it in the final manuscript.

Line 300: Is this signal an increase? Please specify.
In the far-field we have at LGM a sea-level lowstand, while local bedrock subsidence leads to an increase in RSL.

Line 336-339: This line states that the cooling effect supports more refreezing but the effect of the change in salinity is not mentioned. Could you reflect on the importance of the temperature changes versus the salinity changes? Dominates a temperature change in the range of 0.5 degrees Celsius over a salinity change in the range of 0.21 psu or are both changes equally important on the basal melt rate changes?
Thanks for the comment. According to the melt rate estimate based on the equation of state (Eq. 8; Reese et al., 2018), the range of changes in salinity are far less important than the considered range of changes in temperature. For the range selected by the reviewer, we would expect a factor of 40 (=0.5/(0.0572*0.21)) in melt sensitivity with respect to temperature as compared to salinity. We will add this information in the revised manuscript.

Line 425-427: This is not necessarily the case, it depends on the forcing. A very weak rheology has a stable bedrock or shows even uplift during short periods of a warming climate during the glaciation phase. The bedrock therefore subsides less than using a stiffer rheology that does not respond to short periods of warming (van Calcar et al., gmd, 2023)
In the simulations by Albrecht et al. (in review), they also discuss timescales for low and high viscosity end-members. The here mentioned effect is likely caused by a visco-elastic forebulge feedback, acting on rather long time scales during glaciation, which is less pronounced for higher viscosities. We will refer to the discussions in van Calcar et al. (2023) and Albrecht et al. (in review).

Line 431-436: This must be more clear from the beginning to be able to understand the method (see also comment on line 286-288). Also, I assume the grounding is evolving the RSL simulations. Please indicate precisely over which steps the grounding line does not evolve.
Thanks for the comment. We will try to be more clear about the methods and underlying assumptions right from the beginning.

Figure 1: Please indicate the sill depth in the figure. Furthermore, different contour line representing salinity have the same number (34.65). Is this correct? If so, could you explain how to interpret this?
The multiple isohalines for 34.65 psu result from small variations of salinity corresponding to depth where the retrograde slope yields the continental shelf from open ocean water. These variations are most likely a result from the regridding/extrapolation mechanism done in Jourdain et al. (2020). This is not of great importance for our methodology, as we are only interested in the variations of ocean properties with depth at the continental-shelf break (see small circles marked with T_csb, S_csb in Figure 1). We will consider this issue, when preparing a revised version of the figure.