07. August 2024

Dear editor Florence Colleoni, dear reviewers,

in response to the valuable feedback of the reviewers, we have made several significant changes to the manuscript:

- giving more significance to our analysis by assessing the impact of relative sea level change on basal melt also for a time evolving ice-sheet, including a transient deglaciation scenario (only for present-day ice sheet configuration before)
- reducing the complexity of our results by taking only the deepest grounding line access depth into account for computing changes in continental-shelf break ocean properties and the resulting changes in basal melt (instead of the full grounding line fraction g% before)
- simplifying the methodology by removing the horizontal grounding line adjustments due to its minimal effect, but additional complexity
- using a new implementation for calculating access depth maps, which is orders of magnitude faster than before and enables processing of more data in high resolution (500m, previously: 8km)
- reworking large parts of the text, especially the explanation how access depth maps and grounding line access depths are calculated
- adapting the manuscript title to "*Bathymetry-constrained impact of relative sea-level change on basal melting in Antarctica*"

We believe that these revisions address the reviewers' concerns and enhance the overall clarity and robustness of our findings. We appreciate the time and effort that you and the reviewers have dedicated to reviewing our manuscript, and we look forward to your feedback on the revised version.

Please find the point-to-point response to the reviewers comments below, as submitted on 31.05.2024.

With kind regards,
Moritz Kreuzer on behalf of the author team

Response to Reviewers

composed on 31.05.2024

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 1 (anonymous)

In order to respond to the comments by RC1, we have copied the annotations from the pdf document. The original text in the manuscript is formatted in *black italics*, while the specific words marked by the reviewer are underlined. Our comments are given in blue.

[Abstract, l.4-5]

On glacial time scales, these changes can be in the order of several hundred meters, modulating the access of ocean water masses at different depths …

- This is not yet demonstrated. Substitute "modulating" with "potentially affecting"

Thanks for the comment. We changed this in the manuscript.

… to Antarctic grounding lines.

- Some water masses do not reach the grounding line, but may be still impacted by changes in sea level. I would add "and ice sheet margins"

Thanks for the comment. We changed this in the manuscript.

[Abstract, l.10-14]

Under Last Glacial Maximum sea-level conditions, this effect would lead to a substantial decrease of present-day sub-shelf melt rates in East Antarctica, while the strong subsidence of bedrock in West Antarctica can lead up to a doubling of basal melt rates. For a hypothetical globally ice-free sea-level scenario, which would lead to a global mean (barystatic) sea-level rise of around +70 m, sub-shelf melt rates for a present-day ice sheet geometry can more than double in East Antarctica, but can also decrease substantially, where bedrock uplift dominates. Also for projected sea-level changes at the year 2300 we find maximum possible changes of ±20 % in sub-shelf melt rates, as a consequence of relative sea-level changes only.

- Fine. Perhaps could be useful to say a few words about which water masses are more involved in the changes in basal melting, to explain very briefly how such large variations are possible.

We agree that warm Circumpolar Deep Water could be mentioned here and the temperature profile with depth. However, it is always a fine balance to keep the abstract concise on the one hand, while not being too short on the other hand. We will consider this comment when preparing a revised manuscript.

- It is not very clear how these hypotethical experiments are performed, whether the GIA adjustment from the "would be missing" ice sheets is taken into consideration in the ice-free and the 2300 experiments.

In our simulations only relative sea-level changes from coupled ice-GIA simulations with varying ice sheets are considered, but ice thickness distribution remains the same (with a small correction of present-day grounding line) in the further analysis. While we think that explaining the methodology details is out of scope for the abstract, we hope that this gets clear in the method section later. We will try to be more precise about this in the revised manuscript.

[Introduction, l.23]
The global distribution of the sea level aligns according to an equipotential surface, also called the geoid (Gregory et al., 2019), which is determined by the gravity field of ice, water and the Earth's mantle material, with a feedback on Earth's rotation.

- add citation (e.g., Mitrovica et al., 2001)

Thanks for the suggestion. We added the additional reference Mitrovica et al. (2005).

[Introduction, l.23]
Variations of sea-level height through ocean currents and winds are not covered by the geoid definition.

- included in

Thanks for the comment. We changed this in the manuscript.

[Introduction, l.29-30]
The mass redistribution between ice and ocean also affects the Earth's rotational axis, such that the global sea-level pattern adjusts to the change in centrifugal acceleration.

- add "change" before "pattern"

Thanks for the suggestion. We changed the formulation to "global sea-level fingerprint", which we think is more clear.

[Introduction, l.31-32]
The gravitational force exerted by ice masses on the surrounding ocean masses leads to variations in local geoid height near ice sheets when there is a gain or loss of ice mass.

- following gains or losses of

This is a more elegant formulation. Thanks for the suggestion. We have adapted the manuscript accordingly.

[Introduction, l.35]
The reverse signal in RSL, which occurs in the vicinity with smaller magnitude is called a 'forebulge'.

- Rephrase: "Due to the elastic properties of the lithosphere, an increase in ice load would produce an uplift at some distance from the centre of the load, yelding a reversed (negative) signal in RSL; this is called a "forebulge".

Thanks for the suggestion. We have adapted the phrasing, which now reads:

Due to the flexure of the lithospheric plate and the viscous flow of upper mantle material, an increase in ice load would produce an uplift …

[Introduction, l.67-69]

Albrecht et al. (2020a) use a temperature-index method and linear response functions to scale present-day ocean temperature observations on the continental shelf, which is the shallow ocean area surrounding the Antarctic Ice Sheet, with climatic variations derived from ice core data.

- already defined as AIS at line 48

Thanks for the comment. Due to readability we prefer to sometimes, but not always refer to the Antarctic Ice Sheet as AIS. We keep the formulation for now, but are aware that this might also change in the typesetting later.

[Introduction, l.85-88]

The typical depth of the continental shelf around Antarctica (approx. 500 m) is in the range of the thermocline layer, such that small RSL changes can have a comparably large effect on the available ocean temperature and heat on the continental shelf, assuming no changes in flow pattern resulting from RSL changes.

- I expect this to be true only for continental shelves where the Antarctic Slope Front allows CDW to enter, ("warm" type continental shelf Thompson et al., 2018).
- At this point this statement is not supported.

Warm shelves are the most intuitive cases for applying the CSB anomaly to the PICO input temperatures. But as we have explained above, also the continental-shelf water masses of dense and fresh shelves (after Thompson et al., 2018) can possibly change with varying RSL. We think that the methodology is suitable to calculate maximum possible changes due to changes in RSL.

As depicted in Fig. 1, sea-level changes in the order of 100 meters can have significant impacts on the heat available for melting at the ice-shelf base, even with no further climatic temperature changes or grounding line migration considered.

- Figure 1 is not showing this and the statement is poorly supported at this point in the text.

Figure 1 shows how continental-shelf break salinities and temperatures are depth dependent and therefore subject to change with different RSL configurations. This is indicated by the three little circles at the continental-shelf break (bedrock elevation=-1800m) at different critical access depth levels, representing the present-day, LGM15k and icefree RSL configuration.
RC2 suggests to move lines 326-332 (explaining the vertical profile of CSB temperature and salinity in more depth) to the introduction. We think this is a good idea, and hope that it also gives more clarity to the point raised by this comment.

[Figure 1 caption]

- You should mention where these data are taken from and what processing was made on them (WOA18p and EN4 databases regridded and extrapolated in Jourdain et al., 2020).

The reference Jourdain et. al (2020) is already included in the figure caption. We will add more information about how they obtained the data in Section 2.3, which we think is a more suitable location than in the figure caption.

- I see several isohalines that have the same value at 34.65. Is this a feature, the extrapolation, or an error? How can PICO simulates overturning with basically homogeneous water masses?

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.
PICO parameterizes the overturning flux underneath the ice shelf base as a function of the density gradient between the given ocean forcing (box B_0) and the grounding line (box B_1, see eq. 3 in Reese et al., 2018).

- The Eastern Weddell Sea, as shown in the figure, is a continental shelf of the type "fresh" according to the classification of Thompson et al., (2018). Is the conceptual scheme shown working the same for "fresh", "warm" and "cold" continental shelf types? I would add a figure for each type.

Thanks for the suggestion. As the methodology is the same, independent of the shelf classification of Thompson et al. (2018), we think one figure is enough to explain the overall concept of the study. In a revised version of the manuscript, we will explore possibilities to transform Figure 1 into a more conceptual figure to reduce complexity and convey the methodological concept more clearly (see also answer to RC2 comment above).

[Introduction, l.92-94]
[...] topographic features such as troughs and sills act as oceanic gateways as they <u>*provide or block access of warm CDW*</u> *into the ice-shelf cavities …*

- Please provide references?

Thanks for the comment. We initially thought that it is enough to refer to the Nicola et al. (in review) manuscript, but we agree with the reviewer that it is useful for the reader to add the direct references here. We have added the following references now:
Thoma et al., 2008; Nicholls et al., 2009; Hellmer et al., 2012; Pritchard et al., 2012; Tinto et al., 2019; Sun et al., 2022

… towards <u>*deep-lying grounding lines*</u>*.*

- Towards all deep-lying groundin lines or only some? Where does it happen?
- Also, first troughs provide access to the continental shelves, then CDW may flow to the cavities and then maybe can reach the grounding line.

The influence of bathymetry is most prominent where such “oceanic gateways” exist, namely in regions where troughs are providing channels for open ocean water access to grounding lines at same depth or lower than the overflow. Of course such a topographic connection is just a prerequisite and doesn’t imply that warm water has to reach the grounding lines through these channels.

[Section 2.2, l.143]
First, we compute access depth maps, which indicate for every location on the continental shelf, the deepest possible topographic connection to the open ocean.

- This method was not particularly clear, but becomes clearer after seeing Figure 4a vs Figure 4c and after reading later parts in the paper.
- A map of the topography vs access depth map in the Supplementary would be helpful.

Thanks for the comment. Providing a map of how access depths and topography differ is a very good idea, and we will provide this in a revised version of the manuscript. We have also included this in the Author Responses of Nicola et al. (in review), see Fig. 1 in https://doi.org/10.5194/egusphere-2023-2583-AC2.

[l.146-151] *We perform this analysis on 8 km horizontal bathymetry resolution and iterate vertically in steps of 1 m. Then, we evaluate the resulting 2-dimensional field of access depths at the present-day grounding line position for all scenarios. This results in 'critical access depth' scalars dc (s, b, g), which indicates the lowest possible connection between the open ocean and the deepest grounding line fraction g, ranging from 10 % to 90 %, varied in steps of 5 %. We calculate critical access depths for each of the scenarios s described above and for each of the 19 basins b as defined in Zwally et al. (2012), with some of the basins being merged as in Reese et al. (2018).*

- [general comment] This part is not clear, and it's a foundational part of the method. It is not clear how d_c it is calculated and what do you mean by it in relation to the grounding line fraction, this phrase: "which indicates the lowest possible connection between the open ocean and the deepest grounding line fraction g" (Also in Nicola et al., 2023 it is not clear). Add the formula for the calculation if possible.

Thanks for pointing out that the description of our method is insufficient. We will comment about the specific questions below, and will improve the manuscript accordingly.

- *[evaluate]* what do you mean with "evaluate"? "Calculate", "compare"?
- *[d_c(s,b,g)]* Some parts of the approach described here and in Nicola et al., (2023b, subm.) are not clear to me in particular: How do you define the critical access depth in function of "g". Why do you call it "critical"? It should be a kind of threshold? How do you define "g" the portion of grounding line reached at a certain access depth (is it the lenght of the grounding line along the horizontal dimension?)
- *[lowest]* lowest means? Deepest?
- *[connection]* what does it mean syntactically to connect the open ocean with a fraction "g"?
- *[grounding line fraction g]* "g" is not defined and is a foundational element. (Also in Nicola et al., 2023). Also it is worth making some examples to understand the connection between d_c and g. How do you consider flooding of the cavities if the grounding line is not reached by the warm water mass?

The *access depth map* is a 2-dimensional horizontal product that shows the deepest possible connection to the open ocean for every grid point on the continental shelf. We compute this map with the flood-fill algorithm described in Nicola et al (in discussion). As we are interested in the access depths at the grounding line, we "evaluate" the 2D access depth map at the grounding

line position, by using only grid points that are marked as grounding line points for the further analysis. We take this sparse map of access depths which only has values along the grounding line to compute *critical access depths*.
The critical access depths (d_c) are defined as the deepest bathymetric connection between the open ocean and the grounding line. As the individual grounding line grid points show a wide range of depths, we need to decide what part of the grounding line we are interested in. However, it is difficult to set a uniform threshold (e.g. 10% or 50%) to define the amount of grounding line that needs to be reached, in order to be significant for the overall melting in the cavity.
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 percent of the grounding line can be reached by evaluating the sparse access depth map described above. So 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 map entry 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%).

[Section 2.3, l.158-159]
In order to assess the induced changes, we evaluate …

- what do you mean? maybe evaluate -> consider?

… the vertical column of ocean temperature (T_csb) and salinity (S_csb) at the continental-shelf break (horizontal position where topography follows z = −1800 m isobath).

- you mean the temperature of a vertical column of water at the continental shelf break?

We take the dataset by Jourdain et al. (2020) and "read" temperature and salinity values at certain locations. The horizontal position is determined as the continental-shelf break mask (z=-1800m), whereas the vertical position is given by the critical access depth d_c (see also little circles in Figure 1). "Extracting" the values at these locations results in horizontal data vectors, which we reduce to scalar values by taking the horizontal mean (T_csb bar). In our opinion the verb "to evaluate" is a better formulation to describe this than "to consider".

[Section 2.3, l.160-161]
We define the mean continental-shelf temperature T_csb(s, b, g) as the horizontal average of T_csb in basin b at critical access depth d_c(s, b, g), …

- You mean the mean continental-shelf "break" temperatures? The mean temperature on the continental shelf would be very different

Thanks for spotting this mistake! Indeed we mean the continental-shelf **break** temperatures here. We corrected this in the manuscript.

… similar to Nicola et al. (2023b, subm.).

- Where exactly? eq. (3) (4)? Again the average computed there is only on the continental shelf break

Yes, we are referring to Eq. 3 here of the current manuscript of Nicola et al (in review). We added this reference to our manuscript. As pointed out above, Nicola et al. (in review) compute the temperatures/ salinities at the continental-shelf break *and* the calving front, we are only interested in the continental-shelf break position for this work.

[Section 2.3, l.165-162]
As in Nicola et al. (2023b, subm.), we use the ISMIP6 climatology dataset (Jourdain et al., 2020), which contains potential temperature and practical salinity data points averaged over the period 1995-2017, available at a 8 km x 8 km horizontal and 60 m vertical resolution.

- Understanding how reliable your ocean input is is key. State briefly where the data of the ISMIP6 database was obtained from and state the main biases and limitations in the discussion (@ line 393)

Thanks for pointing this out. We will add this to the discussion in a revised version of the manuscript.

[Section 2.4, l. 171-173]
PICO parameterizes the vertical overturning circulation in ice-shelf cavities driven by melt-induced buoyancy fluxes, extending the box model by Olbers and Hellmer (2010) to two horizontal dimensions. The module takes ocean temperature and salinity at continental shelf depth as input, averaged horizontally per basin.

- Doesn't the module also neet T_cf, S_cf along the calving front as well (as stated in Nicola et al., 2023b)? So are you really using mean shelf-break temperature and salinity to drive PICO? This seems wrong.

Our baseline for PICO input temperatures (present-day case) is the same setup as in Reese et al. (2023), where temperatures and salinities **on the continental shelf** are used (modified from Schmidtko et al. (2014)). For the different RSL configurations (LGM15k, icefree, yr2300) we add inferred **anomalies at the continental-shelf break** to the PICO baseline input. We therefore never apply ocean data at the continental-shelf break directly as PICO input.

[Section 2.5, l.203-205]
While the climate forcing reflects an upper end estimate, the dynamic ice-sheet response does not include structural uncertainties of ice sheet behaviour such as the Marine Ice Cliff Instability (MICI), which can potentially increase Antarctic ice loss by a multiple (IPCC AR6 WG1 Ch. 9.6.3.5, Fox-Kemper et al., 2021)

- use another expression

We have changed the phrasing to

> *[...] which can potentially increase Antarctic ice loss by a factor up to 4 [...]*

[Figure 6, caption]

- These panel plots are really interesting but a little bit small.
- I'd suggest, if possible, to add bars to indicate which basins are in East Antarctica, in West Antarctica or in the Amundsen Sea and in the Ant. Peninsula to give a bulk idea of where we are looking at.
- Alternatively add a legend next to the panel to indicate the names of the basins so there is no need for the reader to switch to Figure 2

We thank the reviewer for the nice suggestions and will consider them when preparing the revised manuscript.

[Section 3.3, l.353-354]

The icefree scenario also shows the important influence of critical access depth with respect to the thermocline depth and vertical gradients: …

- I would say this is a limit of the method. A ticker layer of CDW on the shelf break could potentially affect basal melting, as well as a bigger reservoir of shelf water. Relying only on the extracted temperature and salinity at a specific depth does not necessarily reflect the actual interaction with the ice shelves.

We agree that this is a weakness of our methodology and thanks for raising this point. As written in the general comments above, we will add this to the discussion in a revised manuscript.

[Section 3.4, l.381-384]

This is a relevant but much smaller effect than the changes induced by climate change as expected in the upcoming centuries, e.g. with an average increase of basal mass balance by +450 % projected until 2300 by Greve et al. (2023), or the 13-fold increase of mean basal melt rate in Mathiot and Jourdain (2023, in discus.)

- in Greve et al., 2023 they say "increase in magnitude" because the balance is negative.

Correct. Some define the basal mass balance negative when ice is lost, some positive as a vector (as we do). To be consistent within our manuscript, we think it is appropriate to transfer the definition used in Greve et al. (2023) to our convention.

[Section 4, l.400-402]

Instead, we add the RSL induced changes in ocean properties at the continental-shelf break as anomalies to present-day ocean forcing located inside the ice-shelf cavities.

- Applying the anomalies directly inside the ice shelf cavities without some parametrisation of water masses transformation seems wrong.

PICO provides ice-shelf melt (and refreezing) estimates for given ocean temperature and salinity estimates (outside the cavity) for one set of parameters valid for the whole Antarctic Ice Sheet. PICO assumes a vertical overturning with entrainment on the way from the grounding line to the calving front, but no dynamical transformations on the way from outside the cavity to the grounding line are considered. Only cavity-resolving ocean models can account for those processes. Considering the scope of the study (estimating the *maximum* effect of RSL impact on basal melt rates), we think that the chosen approach is still valid, despite the given limitations.

[Section 4, l.442-445]

For the present-day ice-sheet geometry and the different RSL change patterns, we do let the grounding line position adjust to associated changes in bedrock topography, as a static re-evaluation of the floatation criterion, while we neglect the ice-dynamical adjustment to this change in boundary conditions.

- Why not reference Figure S3 here? It seems not cited anywhere.

Thanks, that's a very good idea. We have added the reference in the manuscript.

[Section 5, l.492-493]

We compare our estimates to similar effects induced by shifts in climatic boundary conditions, associated with altered wind patterns, sea ice and ocean dynamics.

- I wouldn't put it in the conclusion since the comparison is very briefly outlined in the discussion and only mentions the orders of magnitudes of the effectes.

Thanks for the comment. We removed the statement from the conclusions.

[Supplement, Figure S1 caption]

The global Last Glacial Maximum has been reached at ca. 26 ka BP (left), but Antarctica's LGM was at around 14.5 ka BP.

- Is this a new generally accepted view or just the outcome of your simulations? In the first case, I would add some references.

The stated timing refers to our simulations here. But as pointed out in Bentley et al. (2014) it is commonly accepted that the global LGM is not aligning with the local Antarctic LGM:

"The use of dated timeslices also has the advantage of avoiding terms like 'the LGM', which has been used rather variably both to refer to local ice sheet maxima, and as a global chronostratigraphic term to refer to the period c. 26.5e19 ka BP (see Clark et al., 2009 for discussion). This has led to some confusion in ice-sheet syntheses. Whilst the 20 ka timeslice can be a useful rough proxy for the global LGM, it is clear from Anderson et al. (2002) and this volume that the Antarctic Ice Sheets did not reach a synchronous maximum extent, and that Local Last Glacial Maximum (LLGM; (Clark et al., 2009)) positions differ widely in timing."

We have added some more information in the caption to make this clear.

[Supplement, Figure S3]

- This figure should be cited in the text, around line 443 (it seems it is not cited at all.) However please, explain better what the figure is showing. The legend and caption

Thanks for the comment. The figure is now cited in the discussion. We will add a more verbose caption and legend in the revised manuscript.

[Supplement, Figure S4]

- Include the continental shelf for comparison with the continental shelf break. Add standard deviation both in time and space for each basin. Also in Figure S5.

Thanks for the suggestions. The ISMIP6 dataset (Jordain et al., 2020) is not time evolving, so we cannot show temporal variations. We will try to find a useful visualization for the spatial variability.

[Supplement, Figure S6]

- increase a little the label size.

Thanks for the comment. We will try a bigger label size.

[Supplement Figure S7]

- Make the plot bigger if possible. also Figure S8

Thanks for the comment. We have increased the figure size.

Specific comments of Reviewer 2 (Johannes Sutter)

L46 during the LGM
Thanks for the comment. We have corrected the phrasing.

L58 «Antarctica loses up to 3.13 m of sea-level equivalent ice» 3.12 m is a very precise upper estimate and I think it is alright to write ca. 3.1 m here given the substantial uncertainties associated with these projections.
Thanks for the comment. We agree that 3.1m is sufficiently precise for the context here and have corrected the manuscript.

L68 I suggest to rephrase to something like “has to be designed/parameterized/prescribed in a robust manner”. “… appropriate way” is quite subjective given the scarce proxy-constraints for paleo climate states and evolution.
Thanks for the good suggestion. We have adapted the phrasing accordingly.

L77 “As (positive) values of RSL indicate … ”
I assume changes in RSL can go both ways (positive and negative), therefore same for bedrock topg. Or do you refer to only positive anomalies?
Relative Sea Level (RSL) comprises changes in water column thickness. We interpret a change in RSL as negative topography change (both uplift and subsidence), while the ocean surface elevation aligns with the geoid (z=0). We have chosen to add “(*positive values of)*” in brackets in the manuscript, as negative RSL values technically don’t represent a water column depth, but rather the land elevation above sea level.

L86 do you refer with “flow pattern” to ice flow or ocean circulation changes? I assume ice shelf flow?
We are referring to changes in local ocean circulation here, which we assume to be unaffected by bed topography changes and simply remain constant (depth layers) relative to the surface. We have adapted the manuscript to clarify this.

L86-88 if I follow your argument correctly this is assuming that ocean circulation does not change right? A real scenario with 100 m RSL changes would, I presume, be associated with ocean circulation changes as well. Not so straightforward to disentangle the actual effect in a coupled system, but I’m aware that this is not what you are discussing here. However I’d suggest to include this caveat somewhere in the discussion.
Correct. We are assuming that the ocean circulation does not change. We will cover the missing effect of ocean circulation changes through RSL in our methodology by including it in the paragraph about *present-day ocean observations* in the discussion.

L109 “a configuration with all continental ice masses transformed into liquid water (GMSL ≈ +70 m).” repeating myself here, but what relevance has such a scenario? If all ice is gone, the concept of basal melt rates is rather meaningless? Except for glaciation scenarios after such a complete de-glaciation. However, it would be anyone’s guess how ocean conditions would look like in such a scenario. I am a little unsure how informative this high-end member is.
Thanks for raising this point. As explained above, the idea here is to evaluate basal melt sensitivity for a maximum range of RSL changes induced by ice-mass changes. We have realized that speaking of an icefree “scenario” is actually not really appropriate here, as the RSL configuration is rather a theoretical upper limit that we want to test (see also our general comment above). If a new ice sheet would form for such a bed topography it may be subjected to similar boundary conditions.

Section 2.2. is this the algorithm developed by X. Davis for ISMIP6? If so, please reference. Nevermind, just saw in Nicola that this is similar to ISMIP6.
Yes, the method is comparable to the extrapolation of ocean properties (temperature, salinity) in the Jourdain et al. (2020) paper. But we are applying the method to bathymetry data. We have added additional information to the manuscript to make this clear.

Figure 1. This is a nice figure. I’d suggest to restrict the top left inset to the FRIS region/continental shelf otherwise it’s a bit small to read.
Thanks for the good suggestion. We will consider this when revising the figure. In case we are transforming the figure to a conceptual one, we will probably delete the inlay entirely.

P5 L129 VILMA solves the global sea-level equation self-consistently,
Thanks for the comment. We have adapted the phrasing.

L136 same question as above, for sea level drop (LGM scen.) I understand the negative offset to bathymetry. For regional/local SLR this should be a positive offset right?
We apply: delta(RSL) = - delta(topography). So when the RSL increases, the bedrock is deepend, while for a decreased RSL, the bedrock has been uplifted accordingly. The delta(RSL) signal is a combination of different mechanisms (possibly with different signs), e.g. a far-field sea level rise (by melted ice) and a local sea level drop by bedrock uplift. In the LGM case, the far-field signal is a drop in sea level, while locally RSL increases where bedrock subsidence dominates.

L145 again, difficulties to understand this, you start the flood fill algorithm in the open ocean, i.e. beyond the continental shelf break and then work your way forward towards the same or lower bathymetry? In this case you would never reach the continental shelf. I seem to misunderstand something here, but maybe consider to rephrase this. For me “lower bathymetry” means deeper ocean bed.
The flood-fill algorithm is repeated for each vertical level, starting at bottom and subsequently “filling up” the topography that can be reached from the open ocean. This is done in vertical steps of 1m. Please see also the appendix to Author Comment 1 from Nicola et al. (in review, https://doi.org/10.5194/egusphere-2023-2583-AC1) where we give a more detailed explanation

of how the algorithm works. We are sorry that our explanation in the manuscript seems to create confusion. We will rework this and provide a more detailed explanation in the revised manuscript.

Section 2.4 If I understand correctly, you derive a 2D forcing field for PICO from averaging over the thermal forcing acquired over the continental shelf taking into account critical access depth of pathways instead of simply averaging over a continuous depth range? Maybe state this more explicitly somewhere.

Not exactly. The "default" forcing for PICO is acquired by averaging the Schmidtko et al. (2014) data over the region of the continental shelf. In Reese et al. (2023) these basin average temperatures are tuned together with the two PICO parameters C and gamma. We take their adjusted Schmidtko temperatures as baseline forcing.

Then we compute anomalies at the continental-shelf break for different RSL configurations and apply these anomalies as a modification to the baseline forcing.

We apologize if the manuscript does not communicate this clearly enough. We will try to improve this in the revised manuscript.

L190 suggest to rephrase this. E.g. : this agrees well with e.g. Clark et al, 2009 suggesting an Antarctic delay of 4.5 – 12 kyr with respect to the global LGM sea level lowstand.

Thanks for this good suggestion. We have adapted the manuscript accordingly.

L197 does that mean you integrate PISM for 86 kyrs at 4 km resolution (in L181 you mention 4km resolution in your initialisation)?? That would be very impressive indeed.

This is somewhat misleading in combination with l.180. The coupled PISM-VILMA simulations have been performed with 16km resolution, but the diagnostic PICO simulations with altered bed topography on 4km resolution. We have added a small paragraph to Sect. 2.5 to clarify this and avoid misunderstandings.

L200 "plausible RSL change rates as observed by GNSS measurements" I suggest to show this in supplementary materials.

We understand the interest in this comparison. We are currently describing those coupled PISM-VILMA experiments and data comparisons in a separate publication.

"Then, from present-day onwards" assume this means 2005?

The historical period is from 1850 to 2015. We have added this information to the manuscript.

L205 "which can potentially increase Antarctic ice loss dramatically but is poorly constrained …"

Thanks for the suggestion. We have added the proposed phrasing.

L206 "To also include …"

Thanks. Done.

L206.. “To include also non-Antarctic cryospheric changes and reflect redistributions in the global water budget, we add a uniform GMSL contribution of 3.68 m on ” is this contribution added in a timeseries or all at once? While being a secondary effect later on it would affect your results at least somewhat if you already add this during the 21st and 22ndcenturies.
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. and all at once.

L215-220 I find your methodological approach very intriguing, however I am missing a caveat paragraph mentioning that ocean circulation amongst other things would change in light of these large scenario differences which might actually make the critical access depth a secondary effect (or vice versa enhance it even).
Thanks for the comment. In the current version of the manuscript, we discuss that the maximum order of magnitude of RSL induced basal-melt changes is comparable to other processes (like changes in external ocean forcing). We agree that this can still mean that the RSL effect is dominated by other processes. We will extend the discussion accordingly.

Also, maybe I missed this in the introduction or methods, but how do you force your LGM15k scenario? ESM-time slice, parameterized, …? What ocean conditions do you provide as baseline before correcting for bathymetry changes?
The coupled ice-solid Earth simulations for deriving the LGM15k RSL configuration are forced by an ice load history (ICE-6G_C) for the northern hemisphere and climate forcing and initialisation for the Antarctic Ice Sheet as described in Albrecht et al. (2020a): both surface air temperature forcing as well as ocean forcing (PICO default input) is scaled by ice core data (EPICA Dome C and WAIS Divide core). From these coupled ice-solid Earth simulations, we take the RSL change to correct the ocean bathymetry in our further analysis.
Concerning the baseline ocean forcing that we use before correcting for bathymetry changes: The “default” forcing for PICO is acquired by averaging the Schmidtko et al. (2014) data over the region of the continental shelf. In Reese et al. (2023) these basin average temperatures are tuned together with the two PICO parameters C and γ_T*. We take their adjusted Schmidtko temperatures as baseline forcing for our study.
It is important to note that we only changed the bed topography (and accordingly the ocean conditions at critical access depth), but the climate boundary conditions remained the same in all ice-sheet experiments when calculating the basal melt rate changes, in order to get an estimate on the RSL effect alone.

Figure 2 b) I do not understand this figure, if you’d remove all ice you’d get a ca. 1/3*H (ice thickness) bedrock rebound due to the missing ice load. That would put most of East Antarctica far above sea level. How is sea level defined in this case? East Antarctica would mostly be above sea level? This comes back to my general comment about rather meaningless impact on basal melt rates where you neither have ice nor contact with the ocean. Or do you always consider a present-day ice sheet configuration and compute the offset in thermal forcing due to difference access depths which are caused by LGM or future changes in ice load/RSL? What

does “adjusted grounding line” mean in this case? Comment: this all becomes clear later on in the manuscript but I’d recommend clarifying this much earlier.
Relative sea level change is defined relative to present or in terms of bed topography, relative to present-day observations. The ⅓*H estimate is not far off the realized bed uplift (RSL drop) in the coupled model. The geoid basically defines the ocean surface, such that RSL change can be meaningfully defined also in regions above the actual sea level (geoid).
In our experiments, however, only the RSL changes are considered for present-day ice thickness. Of course, this is not at all consistent, but it provides an upper theoretical estimate of its potential influence. This study is a sensitivity study and just focuses on one aspect.
The adjustment of the grounding line to the changed bed elevation but same ice thickness is only a relatively small correction and has only little influence on the results.
Thanks for letting us know that the structure of the manuscript can be improved. We will try to explain the methodology more consistently from the beginning onwards to avoid confusion.

Figure 2: Wouldn’t it make more sense to compute the change in thermal forcing for the actual ice sheet configuration used to compute the RSL changes?
Thanks for the comment. Indeed, this would be a useful comparison, but it is not straightforward.
The transient PISM-VILMA simulations are based on external ocean forcing (LGM15k), where 2D Schmidtko data is scaled with ice core reconstructions to represent climatic variations (see comment above). In the yr2300 case, the ISMIP6 extension forcing is applied as a spatially 2D time-evolving anomaly. Both methods are not suitable to calculate CSB anomalies, as the data has no 3D spatial resolution. It is at least questionable whether it is appropriate to compute CSB anomalies from the (present-day) Jourdain et al (2020) dataset, and add it as an additional anomaly to the scaled climate forcing used in the ice-GIA climate forcing parameterisation.
We will explore whether we can add a comparison with/without the impact of RSL also for non present-day ice-sheet states and will propose a discussion of this point in the revised manuscript.

Figure 2: Generally, the figure is quite hard to read due to the small subplots.
Thanks for pointing this out. We have changed the figure from one column to two column format, which makes it much bigger.

L239 amount
We have changed the phrasing to *“..., which can be more than 400 m locally.”*

L271 potential access of ocean currents to the grounding line?
We have adapted the phrasing to *“As the concept of critical access depth relates to the potential access of off-shore water masses to the grounding line, an estimated shift of critical access depths for given changes in relative sea level is not trivial.”*

L286 “For comparability we use a grounding line position corresponding to the present-day ice thickness for all scenarios, which has been horizontally adjusted to obtain the floatation criterion for applied bedrock changes ” I suggest to mention this definition already in the method section.
Thanks for the good suggestion. We will adapt the manuscript accordingly.

L289-291 again, while it is interesting to see what such a shift would mean for basal melting it is still a bit hypothetical as the grounding line would be far advanced for the LGM-state and thus most of the area you are discussing here would be covered by grounded ice. If I understand correctly (and maybe I don’t) you compute basal melt rates for a present-day ice sheet configuration (albeit with a horizontally adjusted grl, see comment above) given an offset in the critical access depth due to a completely different scenario of ice cover.
It is true that the applied RSL configuration is derived from different ice-sheet states than the present-day configuration that we use to compute changes in melt rates. While not ideal, we think it is appropriate to do a first order estimation of the maximum effect of RSL on basal melt rates. We do the horizontal grounding line adjustment in order to still have a physically consistent ice sheet state, as the bedrock is modified but not the ice thickness. However, this has little influence on the results.

Figure 4+5 what’s the second black line (not the continental shelf), ice shelf front?
Yes, the black line encompasses the continental shelf area. As we exclude the ice shelf areas, the line towards the ice sheet represents the ice shelf front.

L326-332 this is a nice summary and could be well positioned in the introduction already.
This is a good idea, thanks for the suggestion. We will move it to the introduction in the revised manuscript.

L333 what do you mean by “the implied potential change of present-day temperatures”? temperature change due to change in critical access depth or applied lgm anomaly?
We are referring to the computed anomalies based on changes in critical access depth using the RSL change in the LGM case. We will rephrase this to avoid confusion in the revised manuscript.

Figure 6. This is a nice figure but also contains a ton of information, I had to look at it for quiet some time to understand what’s going on. Maybe the caption could be a little more explicit and expanded to guide the reader through.
Thanks for the comment. We will extend the figure caption to give the reader more guidance.

L345 “The icefree scenario shows a maximum difference of ±0.5◦C in continental-shelf break temperatures” again I presume this relates to delta T due to changes of access depths?
Exactly. We will rephrase this to be more explicit.

L398 Colleoni et al. (2018) discuss how oceanic heat supply to AIS margins (shelf edge?) can operate …
Thanks for the comment. We changed “AIS margins” to “Antarctic grounding lines” in the

manuscript.

L401 do you mean exchange between different shelves or do you mean “along-shelf transport”
With “cross-shelf exchange” we were referring to water mass transport and transformation from the continental-shelf break (or further offshore) onto the continental shelf towards the grounding lines. We have adapted the phrasing accordingly to be more specific.

L392-404 this discussion/clarification/caveats should occur already in the intro/motivation. E.g. while reading the manuscript it wasn’t clear to me that you employ a present day ocean to compute the basal melt rate changes due to scenario dependent changes in bathymetry.
Thanks for the comment. We agree that the overall scope and idea of the paper can be better understood, if we already give an outlook about the applied methods and underlying assumptions in the introduction. We will rework the introduction to include this.

L409 Why do you not assume this? Please elaborate. On might wonder if considerable higher grl depths don’t change the outcome why would changes in critical access depth at coarser resolution matter? A short explanation would be nice here.
As the deepest grounding line percentages typically correlate with fast flowing ice stream regions, we assume that the melting there is of greater importance than in shallower areas, which are covered by higher grounding line percentages (g>50%). As we only see small differences for the deeper grounding line parts with respect to the flood-fill resolution, we assume that the overall findings of the study is independent of resolution. We will provide more information about this in the manuscript.

L438 topographic structure
Thanks for spotting this error. We have corrected it.

L439-441 very true (see my general comments). I am missing the reason why to include such a scenario as it wouldn’t mean anything for an actual ice sheet (or better to say absent ice sheet).
As we have outlined above, the idea of an icefree RSL configuration is to derive an upper bound for RSL induced basal melt changes. We will rephrase the overall storyline of the paper to be more clear about the sensitivity character of our study.

L443 we adjust the grounding line position (I don’t follow the choice of bold face font in the discussion).
Our intention was to give the reader a better overview about the aspects of different paragraphs in the discussion without introducing additional structures like subsections. We can either explain this in the beginning of the section or refrain from using boldface for keywords.

L446 here I don’t know why you correct for the RSL effect on floatation while ignoring the fact that the critical access depths are due to completely different extreme ice sheet geometries.
The idea of correcting the grounding line position to the applied RSL configuration is to avoid artifacts that can lead to unrealistic results. For example: if the applied RSL change is high at the present-day grounding line (e.g. +100m bedrock uplift), then the critical access depths

would be 100m shallower (in the absence of any oceanic gateways that can yield the access of open water), when using the same grounding line position as before. However when the bedrock is uplifted, the grounding line would advance on a prograde slope to maintain a physically consistent floatation criterion (even in the absence of any changes in the ice thickness). By the advancement of the grounding line, the depth of the grounding line would be less then the 100m uplift applied initially, which also has an effect on the critical access depths. We agree that it seems odd to apply a small scale correction for the grounding-line position, while using a present-day ice configuration for RSL change scenarios that are derived under different ice sheet states. We will reconsider whether this adjustment is necessary when preparing the revised manuscript.

L481 Maybe also cite Hellmer et al 2012 here who where I think amongst the first to point out this possibility.
Good idea. Thanks for pointing this out.

L493 “We compare our estimates to similar effects induced by shifts in climatic boundary conditions, associated with altered wind patterns, sea ice and ocean dynamics.” Where is this comparison except for noting it in the discussion?
We only mention it in the discussion. As this was also noted in RC1, we have removed this sentence from the conclusion.

L495 the relevance
Corrected. Thanks.

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).

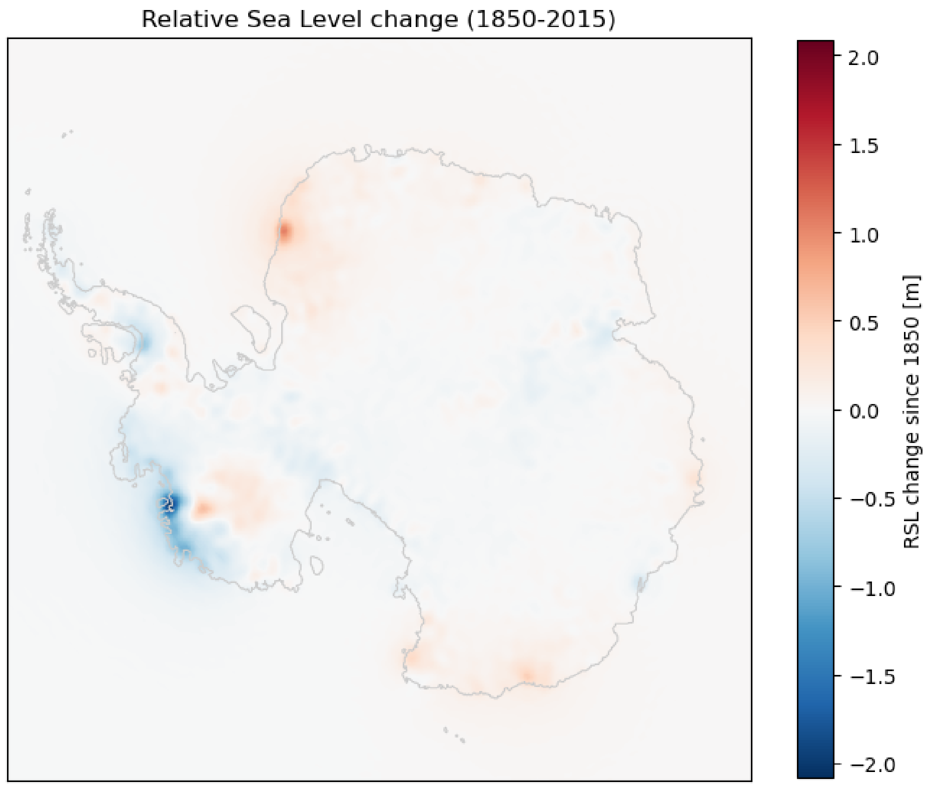

Figure 1: Change of relative sea level computed by PISM-VILMA for the historic period 1850–2015

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.

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