We thank the three reviewers for carefully reviewing our manuscript and are happy to strengthen our study according to their suggestions.
In addition to our initial reply to RC1, in the following, we provide a general response to the overall and key aspects mentioned in the three reviews together, as well as a point-to-point response to all reviewer comments respectively.

Our point-to-point responses to the individual comments are given below in blue and italics compared to the reviewer comments which are given in black without italic font.

Author response to key aspects common to all RCs

With regards to the comments made on ***novelty and relevance*** *of our study, in the revised manuscript, we try to be more direct about* ***our goals*** *and* ***main motivation****. Namely, we aim to estimate the Antarctic Ice-Sheet's vulnerability to changes in ocean thermal driving. To get a rough estimate of potential changes in thermal forcing, we identify features from continental shelf bathymetry that are similar to Filchner Trough which could provide warm water masses access to the grounding line once the cavity switches into a "warm" state. Furthermore, we calculate the connectedness of Antarctic grounding lines to the open ocean. Based on this, we derive an upper-bound of temperature (and thus basal melt rate) changes in ice shelf cavities around Antarctica.*

We strengthen the relevance of our study by additionally providing the derived access depths data for a 500m x 500m grid spacing in the supporting material as well as the code to update the access depths when new data becomes available. In a revised manuscript, we will further extend the section on how our findings may influence PICO tuning in future studies and how this relates to the current settings and other melt parameterizations. To this end, we will now also include an updated delineation of basin boundaries for PICO in the ocean based on our findings (taking up the suggestion made by RC1).
As suggested by RC2, we will also revise the title of our manuscript to strengthen the main message i.e. that we look into bathymetric constrained warm mode melt estimates derived from analyzing Oceanic Gateways in Antarctica.
As suggested by RC2, we will overall stream-line our manuscript to fit our target audience, namely stand-alone ice-sheet modelers (using PICO) and those working on the coupling to coarse grid ocean models, as our study provides bathymetry-informed estimates for temperature and salinity as used in basal melt parameterizations such as PICO.

Concerning the comments made towards our ***terminology and methodology,*** *we will include a more in-depth and more comprehensive explanation of the concept of access depths, through a revised Figure 1 and by rephrasing 1) how access depths are defined and derived, 2) how we define critical access and 3) by showcasing how we conducted the analysis, providing estimates on the computational cost and the numerical code of the flood-fill.*
In the revised manuscript we will also add further explanations how our approach relates to the ***ISMIP6 protocol,*** *i.e. that we foremost analyze bathymetry and subsequent grounding line connectedness and that our analysis can be adapted to whatever ocean dataset there is available.*

To reduce complexity, we will rework the use of ***our parameter "g"*** *that we introduced to describe how much percentage of a basin grounding line can be accessed by open ocean* *water masses at depth. When revising our quantitative results, we will now additionally exclude all parts of the ice sheet where ice is grounded >0m (as correctly pointed out by RC2). We will propose a new Figure visualizing the distribution of access depth at the grounding line (formerly Figure 3).*

*Generally, **more than one ice shelf** can be included in one PICO basin. We will make sure to discuss related caveats in the revised manuscript (which was especially raised in RC2). We will clarify the use and definition of a **prominent gateway**, that we had initially defined as "one or several deep troughs that provide access to most of a region's grounding line". Here, "most of a region's grounding line" had referred to 10% or more of the grounding line accessed at one distinct access depth level in our preprint and was highlighted by the magenta boxes in Fig. 3. In regions where we do not see this feature, but a rather gradual increase in access with grounding line portion/fraction, we actually cannot state that an oceanic gateway is present.*

*In the revised manuscript, we will update our **temperature estimates** and **change the overall narrative on our two scenarios.** We will refrain from speaking about "warm-water intrusions"; instead, we will give an "upper-bound" estimate, as we rather consider a macro-scale/basin wide transition in melt mode associated with prevalent access of ocean water from off-shore. To this end we will also assess how the T_CSB estimates change when taking the maximum (instead of the mean) temperatures along the CSB. This would follow our refined intention of a bathymetry-derived upper bound to melt rate changes. Further, we will clarify the paragraph in which we define the temperature estimates and add further discussion on how ocean temperatures may change (e.g. CDW->mCDW) when intruding onto the continental shelf (what we do not resolve). While we take the temperature estimates T_CSB as proxy for mode 2 melting conditions, we will state in the revised manuscript more clearly that T_CF is representative for mode 1 melting (after Jacobs et al., 1992). To this end, we will consider the bottom temperatures at the calving front, instead of averaging them at the overflow / access depth of that basin.*

In order to resolve the discrepancy of our estimated "present-day" melt rates to observations (as pointed out by all three reviewers), we will change our methodology for the melt estimates as follows:

Our used PICO parameters from Reese et al., 2023 were tuned to represent bulk present-day melt rates as well as to match the melt sensitivity at Filchner-Ronne (cold based) and Amundsen Sea ice shelves (warm based). In the tuning process, the input temperatures from Schmidtko et al., 2014 for each basin were adjusted so that melt rates, as well as melt rate sensitivities, would be in line with observations. These necessary, yet to a point unphysical, temperature corrections can hence be seen as an additional factor in the tuning. To be consistent with the tuned parameters, we will propose to take the forcing field from Reese et al. 2023 as present-day baseline temperatures. For estimating "upper bound" estimates of bathymetric-constrained warm mode onset, we will then add the difference of T_CSB minus T_CF (both derived from ISMIP6 dataset) to the existing forcing field. This follows the same "anomaly idea" taken in Kreuzer et al. (in discussion, doi.org/10.5194/egusphere-2023-2737)). We will make sure to expand the explanation of these temperature adjustments within the PICO tuning process in our method section.

Once we have new estimates we will include a more thorough comparison of our temperature as well as basal melt rate estimates to findings from previous literature, specifically in the key regions that the reviewers mentioned e.g. at Ross Ice Shelf and in the Amery region (both mentioned in RC1 on page 12), the gateways we find in the Amundsen Sea (esp. Abbot Cosgrove Trough, mentioned in RC3) as well as our temperature estimates in this region (mentioned in RC2), and subsequent melt rate estimates.

We will clarify the sign convention of z vs. depth and align it with commonly used definitions (in reply to RC1) and we will provide melt estimates in Gt/yr (in reply to RC2). We will further rework our Figures as suggested by all reviewers.

We will further gratefully take up the suggested language changes to specific wording within the text (see respective point-to-point response below).

For the specific comments made by the individual reviewers, please consult the respective point-to-point responses.

Response to RC2

Review of: Oceanic gateways to Antarctic grounding lines – Impact of critical access depths on sub-shelf melt
Lena Nicola et al.
Date: 26 Feb 2024
Assessment: Major revision
Reviewed by: Erwin Lambert

The authors have assessed oceanic gateways for deep water access to the grounding lines of 19 Antarctic basins. Ice shelf melt rates were computed using the PICO parameterisation based on two input forcings: T and S at the calving front, and at the continental shelf break. The melt difference between these two forcings was interpreted as an upper limit for the potential melt increase due to a change in oceanic circulation on the continental shelf (i.e. an onset of an 'optimal mode 2' system).
The idealised approach to representing deep water access and conversion to melt rates is an interesting way to achieve a 'first order' estimate for potential melt increases. Overall the manuscript was an easy read due to a good writing style. I do have a number of concerns though, particularly regarding the methodology, interpretation, and relevance, that in my eyes should be resolved before considering publication. Hence, I recommend a major revision of the manuscript. Below I list my major and minor comments.

Dear Erwin Lambert,
Thank you very much for your detailed review of our manuscript and your feedback.
In addition to our general comment to all reviewers, please find our detailed point-by-point responses (written in blue and italics) to your comments (in black) *below.*

Major comments

1. Motivation / scope. At first (partly based on the title), I thought the main aim was to define the oceanic gateways, and was puzzled by the focus on CF and CSD

temperatures. Only when reaching section 3.2, it became clear to me that computing the difference in melt rates (T_CSD – T_CF) was the ultimate goal to quantify the impact of a hypothetical 'mode 2' onset. To better clarify this, I suggest:

a. Revising the title, with a focus on, in the author's own words, quantifying the melt increase due to 'mode 2' onset, as constrained by bathymetry.

When revising the manuscript, we will revise the title (see general comment above).

b. Throughout introduction and methods, clearly write towards this goal. In particular in section 2.2, this should be very explicit.

We will stream-line our manuscript accordingly.

c. Include a clear narrative how a switch from CF to CSD forcing may occur (e.g. based on 3D modelling) and why, according to the authors, their approach is the optimal way to quantify the 'first order' impact.

We aim to include more details to this in the revised manuscript.

2. Possible errors. I detected two results which the authors should critically reassess, as I believe them to result from errors in the methodology.

a. In the caption of Fig 3, the authors state that 'parts of the grounding lines are situated above 0 m'. This should not be possible and makes me wonder whether cliff faces (boundary between grounded ice and open ocean) are included. In some regions (5, 10, 11, 17), this appears to be the case for >20% of the assessed grounding line.

Thank you for pointing this out to us. This will be addressed and changed. In our initial analysis we considered the contiguous continental ice mass and its contour as the "grounding line". In the revised manuscript, we will refine the definition of grounding line when deriving the input for PICO and only consider the parts of the ice sheets where melting is applied (hence the correct definition of grounding line in this context here - the triple point of bedrock, ice and ocean inside ice-shelf cavities).

b. Present-day temperature at CF in the Amundsen Sea was determined at -0.88 C. This is substantially colder than the +1.1 C observed by Dutrieux et al (2014) at the Pine Island calving front. Note that this is very close to the +1.26 C the authors find at CSD, implying a present day state which is very close to the 'optimal mode 2' state. I also recommend that the authors double-check their tuning parameters, as 20.5 m/yr with a deep water temperature of -0.88 C appears very high to me.

When revising our manuscript, we will update our temperature and melt rate estimates to be more in line with previously tuned PICO parameters from Reese et al., 2023. We plan to change to an anomaly approach, as laid out in the general comment above. When discussing our temperature estimates we will, especially in the Amundsen Sea region, discuss this discrepancy between the basin average temperature T_CF and the locally observed temperatures that exist to date, especially within those basins incorporating more than one ice shelf. As we use the ISMIP6 dataset for our analysis, we depend on the, in part newer, observations that have been incorporated when creating the forcing fields. As said, we will make sure to include a thorough discussion of this aspect in the revised manuscript.

3. Methodology. A few methodological choices have been made which I believe require more scrutiny / quantitative assessment / reconsideration.

a. By design, PICO takes one value for the deep water temperature and applies this to the first box near the grounding line. Hence, even in cases where g = 10%, the associated temperature at this depth is applied to 100% of the grounding line. The authors should be transparent about this limitation, and ideally give a quantitative assessment of its impact on their results.

It is correct that the associated temperature at access depth is applied to the entire grounding line box. Yet, the melt rates in each ice shelf cell of this box consider the cell-specific pressure melting point and therefore the depth of each cell. In the revised manuscript, we will try to more clearly state the limitation and discuss the implications. As laid out above in the general comment, we will refrain from the use of the full range of grounding line access (g) and focus on the deepest access depth we find in the different Antarctic regions.

b. The results are highly dependent on the choice of PICO, making it difficult to interpret the results. I would recommend a comparison to the 'quadratic parameterisation' (Favier et al 2019), also used in ISMIP6. This parameterisation can be applied uniformly (as PICO), and regionally (accounting for the limited access to the grounding line). Hence this would allow the authors to quantitatively reflect on point 3a above, and quantitatively assess the impact of the choice for PICO.

In the revised manuscript we will additionally provide basal melt rate estimates using the min, best-fit and max parameters presented in Reese et al 2023 to address the impact of PICO parameters on our estimates. Using a quadratic melt parameterization is for sure helpful to address the uncertainty arising from (the choice of) PICO, but we see a full comparison between different existing parameterizations beyond the focus of our study, as e.g. the ISMIP6 quadratic parameterization is not implemented in PISM to date yet.

c. It is unclear how the authors treat individual cavities. For example, Totten/Moscow University. The authors focus on a trough providing access to the Totten cavity. Does their method implicitly assume that the same oceanic access exists to the Moscow University cavity? If so, the authors should elaborate on this and estimate the quantitative impact of this implicit assumption. If not, the authors should explain how they deal with separated cavities within the Zwally regions.

In our study we follow the common practice of using one temperature and salinity input per PICO basin. In our initial analysis we have used the 19 PICO basins from Reese et al., 2018. In the revised manuscript, we will put a stronger focus on this limitation of averaging over basins that cover more than one ice shelf and make sure to provide more individual results where appropriate e.g. at Totten/Moscow University ice shelf. As mentioned above, we will also propose new basin boundary delineations in the ocean for the future use of PICO.

4. Relevance. It is always tricky to explain the relevance of a relatively idealised study (trust me, I've been there!). I have some suggestions though to enhance the relevance of this study.
    a. Coming back to the motivation, the authors should have a clear take home message (in abstract and conclusions).

We will include our take-home messages, e.g. that only some regions have pronounced oceanic gateway structures that could potentially redirect warm water masses off the continental shelf into the ice-shelf cavities. And if so, melt rates would increase by a certain Gt/yr per basin, assuming that these warm water masses remain unmodified on that pathway. Another valuable outcome of our study is that the PICO boundaries in the ocean could be re-adjusted according to bathymetric features, permitting the connectedness of the grounding lines to the open ocean.

Also, it is a bit unclear who the target audience is: the oceanographic community (providing guidance for further research on circulation changes) or the ice sheet community (providing an upper limit to melt increases)? The manuscript would benefit from having a clear target audience and a clear message to this audience.

With our study we had the ice-sheet modeling community in mind, namely stand-alone ice-sheet modelers (for instance those using PICO) and those working on the coupling to coarse resolution ocean models.

b. Where possible, the results should be compared quantitatively to more realistic studies. For example, the authors cite previous studies on Filchner-Ronne (e.g., Naughten et al 2021), but do not compare their quantitative results to those.

   Throughout section 3.3, I suggest the authors maximise the quantitative comparison to previous numerical modelling studies to place their results in perspective. In addition, this quantitative comparison should be reflected on in the discussion.

As mentioned above in the general comment, once we have our new estimates we will include a more thorough quantitative comparison and discussion of our temperature- as well as basal melt rate estimates to findings from previous literature, if available. Thank you for pointing out this short-coming to us.

c. Also, the authors should mention and reflect on the discrepancy between present-day melt rates (Fig 5a) and observed ones (Fig 5c), and how this impacts their results. In some regions, the relative difference between observations and T_CF is larger than that between T_CSD and T_CF. Can the authors explain this, and convince me and other readers that this does not impact the trustworthiness of their results and conclusions?

We hope the discrepancy between present-day melt rates and observed ones can be addressed by our change in methodology as laid out in the general comment above.

In the initial manuscript, we have used the PICO parameters from Reese et al 2023 that were tuned to represent bulk present-day melt rates as well as to match the melt sensitivity at Filchner-Ronne (cold based) and Amundsen Sea ice shelves (warm based). In the tuning process (as described in Reese et al. 2023 in detail), present-day input temperatures were adjusted i.e. that the needed temperature correction was another tuning parameter to match melt rates and sensitivities. Using an un-adjusted temperature field, like the T_CF values in our initial manuscript, thus produced large discrepancy to present-day melt rates. This was not clear to us when preparing our initial manuscript and we plan to correct for that when revising.

To be in line with the previously tuned PICO parameter, we have therefore decided to take the adjusted temperatures from the tuning process (cf. Reese et al., 2023) as present-day baseline temperatures in the revised manuscript. For estimating the effect of the mode 2 onset, we will thus change to an anomaly approach. For mode 1-representative temperatures from the ISMIP6 dataset, we will extract the bottom-most temperatures at the calving front, T_CF. For mode 2-representative temperatures we will, as done in our initial manuscript, extract temperatures near the continental-shelf break at the determined overflow or access depth. To derive the warm mode onset/"upper bound" estimates, we will then add the difference of the *two estimates onto the tuned forcing field from Reese et al., 2023 and see how melt rates will change if we have a large-scale inflow of warm CDW into the ice-shelf cavity.*

d. As the authors state in their introduction, they aim for a ‘first order assessment of the maximum changes in temperature and melt’. To interpret this maximum, the reader requires some estimate of the magnitude of uncertainties associated with made changes. For example, how important is it that off-shelf temperatures are assumed to be constant regardless of the circulation change? In fact, the authors provide a narrative for a thermocline shoaling to provide CDW access. How significant is the assumption of constant off-shelf hydrography? The same holds for assumptions like fixed cavity geometries, and the choice for PICO (see points above). The question that remains in my head: could the author’s assessed ‘maximum’ melt increases be twice as high due to unconsidered processes, or is it a reasonable estimate?

We see our study as a sensitivity analysis to gauge the effect of the presence of oceanic gateways to Antarctic grounding lines, thus how melt rates change, if a transition from mode 1 to mode 2 occurs, while we explicitly take into account connecting features in the bathymetry around Antarctica. When revising our temperature estimates, we will discuss the uncertainty related to that by, for instance, not only taking the mean along the continental-shelf break, but also considering a wider perimeter in the open ocean and evaluate where the maximum temperature will lie. For sure, this is just an idealized analysis, i.e. a thought-experiment. We know that for precise projections of potential future regime shifts in the Antarctic ice-shelf regions, more sophisticated approaches are needed. These approaches at best have a coupled ice-ocean-atmosphere representation, with interactive ice sheets and ice shelves at high resolution in space and time.

e. The authors focus on g=50% in their figures and most quantifications. What is the logic behind this? I would assume that access to the deepest grounding line parts is most important, and **would think that g=10% may perhaps be more relevant**. Does it make sense to have a fixed g for all regions?
Can the discrepancies between present-day melt rates and observations (Fig 5) be (partly) explained by the choice of g? And can a relevant value for g per region be determined from this comparison to observations?
How should future research treat these values? Stick to 50%, or optimise it per region?
Dedicating one or two paragraphs to this in the discussion would significantly enhance the relevance of this work.

These are very important points and we see that our concept of g(%) has been confusing or arbitrary. In order to reduce complexity, initially we wanted to discuss the results for each basin using a certain access depth and we picked d(50%). As laid out above we will change this approach and reduce the g(%) dimension to the deepest depth that provides access to the grounding lines. We will make sure to include an adequate discussion of this aspect in the revised manuscript.

Minor points

l. 10. the 200-fold larger melt rate is highly dependent on the uncertain reference state.The authors should pick a more relevant metric for their abstract, such as the total increase in BMB (Gt/yr).

We gladly take up this suggestion. Will be changed in the revised manuscript.

l. 16. The concept 'mode 1 and 3' is used, but not really explained. Either stick to more generally known concepts, or give a brief explanation here if it's important.

Thank you for pointing this out to us. We will rephrase this part in the revised manuscript.

l. 25. 'tens of metres per year'. Regionally yes, ice-shelf average, this is only possibly the case for Thwaites (and perhaps some tiny ice shelves). Revise this statement and provide a reference.

Thank you. Will be rephrased in the revised manuscript.

l. 30. Here the transition to a warm cavity is described, without mentioning it explicitly. As this is a central aspect in this study, the authors should be more explicit here that they are talking about a qualitative change in hydrography and not a smooth warming.

This point will be taken up and strengthened in the revised manuscript.

l. 52. Provide a reference for these statements. A highly biased suggestion from my side would be Lambert et al. (2023).

This is definitely a worthwhile reference and will be added in the revised manuscript.

l. 55. "For instance, Wouters et al. (2015) find a strong link between surface-lowering and an increase in the dynamical ice loss in the Southern Antarctic Peninsula since around 2009." Is this related to near-grounding line melt? If so, mention explicitly, if not, remove this statement.

Yes, we believe it is related to near-grounding line melt, as Wouters et al. state thinning rates near the grounding line down to –4 m/year, with the average observed elevation lowering rate at –0.42 m/year. We will include this detail in the revised manuscript.

l. 80. (and other places). I don't think 'diagnose' is the correct verb. Replace it with 'parameterise' or something equivalent.

Thank you for pointing this out to us. We will be more coherent in our wording, as we used "diagnose" to refer to the melt rates given a temperature and salinity estimate to PICO (in comparison to calculate transient changes in melting). We will change it to "compute".

l. 82. Does this dataset include the latest version of IBCSO? If so, reference this explicitly, as Bedmachine uses external sources for its bathymetry. If it's not the latest version, mention this explicitly in the methods/discussion.

The BedMachine v3 uses IBCSO v2 for ocean bathymetry according to https://nsidc.org/data/nsidc-0756/versions/3 from which we have obtained the dataset. We will add this detail in the revised manuscript.

l. 84. I don't understand this sentence 'The grounding lines…'. Please revise.

We here want to give a definition on what we considered as "grounding line" in our analysis i.e. at what contour we evaluate our 2D field of access depths. This touches on your point 2a). We will revise this sentence.

Fig. 1. The straight grounding line is odd, as it does not follow the 'triple point' between ice, bedrock, and ocean. It should deepen in the trough.
Thank you. We will take up this point when revising Figure 1.

l. 98. The access depth is implicitly determined in ISMIP6 as well. Explain clearly what the added benefit is of your methodology in reference to ISMIP6. This difference/overlap is a bit unclear to me.
Our study extends the work by the ISMIP6 focus group who created the ocean climatology dataset, since our approach takes into account the depth of the grounding line to derive the access depths, which is then used to determine ocean temperatures, salinities and ultimately melt rates with PICO. We hope to clarify this point in the revised manuscript.

l. 110. The equations and text here do not make the whole methodology very clear (to me..). Consider visualising this, either in Fig 1 or in a new schematic figure, so the reader fully understands what's going on. I'm strongly in favour of a new (schematic) figure which also illustrates the parameter g.
Thank you for this feedback. We will include a new version of Figure 1 in the revised manuscript (see general comment above).

l. 131. 'Input is based…' Does this refer to Reese et al '23? If so, mention explicitly, if not, explain why you deviate from the ISMIP6-based forcing here.
Thank you for highlighting this point. We will change it to "The input (T,S) in Reese et al. (2023) is based on temperature and salinity observations" in the revised manuscript, as it refers to the input in the tuning process described in Reese et al., 2023.

l. 134. Most ice sheet models have PICO included, so I think this is an irrelevant statement (which causes more confusion than clarifying anything). So rather remove it.
We will change it to "PICO was first implemented in PISM….and has been meanwhile used in many other ice sheet models" as we, technically speaking, use the PICO version within PISM, run on the BedMachine geometry (using topography, ice thickness, mask etc) we found it necessary to mention PISM.

l. 159. First T_CF is 'generally lower' than T_CSB, in the next sentence, T_CSB is 'much warmer than T_CF. Align these two statements.
Will be clarified in the revised manuscript.

Fig 2. For the bars, T=0 is used as a reference, which is a bit arbitrary. I'd use freezing temperature as a reference for visualisation (so that the bar heights reflect the Thermal Forcing).
While we acknowledge that providing the temperatures relative to the pressure-melting point would directly show the thermal driving, in PICO, the freezing point is evaluated for each grid cell depending on its depth, such that we cannot define a basin-wide freezing point. We therefore would stay with providing the absolute temperatures for each basin.

l. 172. ‘highest grounding line depths’ -> ‘shallowest/deepest grounding line depths’
Will be changed to “deepest grounding line depths” in the revised manuscript (see also our general comment above on the sign convention on z).

l. 218. Compare 4.65 m/yr to numerical modeling studies.
When updating our melt rate estimates we will update this section in the revised manuscript.

l. 233. Is this other pathway deeper? How is it (or can it be) relevant to your study?
Willams et al., 2016 indicate an outflow of Dense Shelf Water (DSW) through Prydz Channel and an intrusion of mCDW over Four Ladies Bank, which is much shallower than Prydz Channel. Here our core assumption that CDW always takes the deepest entry / gateway towards the ice shelf is challenged. We will rephrase this part in the revised manuscript.

l. 318. *“Our study could therefore be improved by considering specific ocean circulation patterns informed by high-resolution ocean models.”*
These kinds of sentences are fine, but compare to other studies where possible.
Thank you for this remark; we can include some references to the high-resolution ocean modeling studies in the revised manuscript at this point and where else appropriate.

l. 328. Is it possible to quantify the difference between CDW and mCDW? For example looking at Amundsen Sea (observed T_CF = 1.1; T_CSB = 1.26). Does this example mean that the difference is negligible?
When updating our estimates, also using the revised PICO boundaries, we can add a short assessment / comparison of our estimates (T_CSB) to the observed temperatures in ice-shelf regions where we know that mCDW is present at depth.

l. 335. Good point regarding the trough width. What ocean dynamics control this minimum width? Are there specific troughs you highlighted in this study which are very narrow where this may have an impact? How can you/future researchers incorporate this concretely?
We will have a look at the trough widths, also in the smaller Antarctic regions that we find feature oceanic gateways in our analysis. We can include this aspect to a greater depth in our discussion thereafter. The identified oceanic gateways in our analysis are based on the native grid resolution of BedMachine, so that troughs/depressions, if existing, are at least 500 m wide. For our purpose, we assume that water can flow through the gateways i.e. a channel of 500m width is wide enough to fill the water with warm water offshore during a sustained inflow.

l. 397. MISI is new info, which should not appear in the conclusions. If it’s relevant, include it in the intro/discussion. Conclusion should only contain previously presented information that is specific to your study.
Thank you for pointing this out to us. We do not consider the dynamical response of the ice sheet in our study, but MISI provides a motivation to check for melt perturbations. We will move this part in the revised manuscript.

l. 409. Again, geoengineering is new info. Put this in the discussion if you want to include it; stick to your own work and its implications/relevance in the conclusions.
In order to stream-line the revised manuscript, we propose to leave this part out.

References:

Dutrieux et al. 2014. https://doi.org/10.1126/science.1244341
Naughten et al. 2021. https://doi.org/10.1038/s41467-021-22259-0
Lambert et al. 2023. https://doi.org/10.5194/tc-17-3203-2023