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 RC1

Nicola et al. use the BedMachine bathymetry product and ISMIP6 dataset (observations + extrapolation of temperatures (T) and salinities (S) around Antarctica and its continental shelf) to discuss some features of these datasets, for example the relevance of different troughs in delivering heat to the grounding lines (for BedMachine) and the spatial differences in ocean temperatures and salinities (for ISMIP6 dataset).The authors then calculate ice shelf basal melt rates using the box model PICO, where the T/S input is a) conditions at the calving front and b) conditions at the continental shelf. The latter is presented as the "upper limit of melt rate changes".

Dear Anonymous Reviewer 1,
Thank you very much for your detailed review of our manuscript and your feedback.
In addition to our initial reply to your comment (https://egusphere.copernicus.org/#AC1), please find our detailed point-by-point responses (written in blue and italics) to your comments (in black) *below.*

General

I have to admit that I have struggled with the aims and the novelty of this manuscript.
We are sorry that the initial manuscript lacks to convey the aims, novelty and relevance of our approach clearly enough. In the revised manuscript, we will more clearly lay them out, as mentioned also in the general comment above.

The first half of the paper introduces new terminology of 'oceanic getaways' and 'critical access depths' but it is really just talking about bathymetric features, specifically troughs that have received a lot of attention in the past decades as the sea floor around Greenland and Antarctica have become better mapped.

Yes, we analyze the bathymetric features in the used dataset. However, we consider those features in context of a whole basin geometry and further perform a systematic circum-Antarctic assessment of bathymetric access and potential pathways of ocean water to the grounding lines of the Antarctic Ice Sheet, which we believe is not included in publications related to new bathymetry surveys or ocean cruises that focus on specific regions.

The same flood-fill algorithm that the authors use here was also used to produce and extrapolate the ISMIP6 dataset, so I don't think that part is anything new. The next bit of the paper that discusses ocean properties at the calving front vs the continental shelf doesn't do much more than stating the differences in the fields in ISMIP6 dataset and some generalities.
Our study extends or rather complements the work by the ISMIP6 focus group. Our approach of quantifying the connectedness of the grounded ice to the open ocean takes into account the depth of the grounding lines. While the basic concept of the flood-fill is the same, their code is different, as it serves a different purpose. Their code can be found, to our knowledge at https://zenodo.org/records/3997257 under ismip6-antarctic-ocean-forcing-1.0/ismip-ismip6-antarctic-ocean-forcing-7ed686c/ismip6_ocean_forcing/extrap/ (last access 08.04.2024).

In our study, we do not extrapolate temperature and salinity values into ice-shelf cavities, but use the flood-fill algorithm to derive the access depths, to the goal of having a grounding line / geometric informed depth value, at which we can derive our PICO input from.

The section "Oceanic gateways to major Antarctic ice shelves" is a mixture of literature review, and speculation about potential impacts of high melt rates (as calculated by using shelf break instead of calving front temperatures) - but the impacts are not modeled here so just a brief mention in discussion would be enough for that part.
This section was meant to put our regional results into perspective to the existing literature, whether the main gateways we have identified correspond to the main inflow regions that we can observe/model today or in the future. We consider the potential impact of a regime shift by stating the change in melt rates. As we see our study as a sensitivity study of melt rates to a regime shift in all Antarctic regions, we would consider modeling the long-term ice-dynamical response (and eventually disintegration) of the ice sheet beyond the scope of our study.

For the literature review - a lot of this refers to studies about ocean circulation beneath the specific ice shelves, but all that is ignored in PICO, so I don't see why that is reviewed here, since the box model doesn't know anything about horizontal circulation.
We have included the mentioned literature in an attempt to discuss the limitation of our simple approach, e.g. using PICO that does not take into account the horizontal circulation inside the ice-shelf cavity. When reframing our manuscript (see comment above) we will re-evaluate which literature is needed to support our storyline and consider moving this specific part into the discussion section in the revised manuscript.

Other references serve to show that bulk present day PICO melt rates are reasonable, but that was already tuned elsewhere in previous publications, so not sure why that is necessary here again.
We have included these references in our manuscript to justify the use of PICO and the chosen parameter settings.

I think that especially with the simple box model it is really easy to produce large increase in melt rates for any given ice shelf, all that is needed is a change in input temperature that is fed into box 0. Since there are no oceanic processes accounted for that would be allowed to mix or divert away this change, the model essentially by construction contains a tunnel that conducts outer T and S directly to the grounding line. So the result that large change in input (which is chosen here but not really physically justified) causes large change in output, which is the result of this manuscript, is definitely not a surprising one.
It is correct that melt parameterizations like PICO assume a direct connection of ocean conditions outside the cavity and conditions at the grounding line, which neglects the mixing and entrainment processes. However, there are two PICO parameters, associated with the overturning strength and the turbulent temperature exchange respectively, to be tuned for whole Antarctica to produce a large range of realistic present-day melt rates (and different melt modes) for given outer temperature and salinity inputs. As the PICO tuning also covered the basin-specific melt rate sensitivities, we have some confidence in melt rate estimates for the range of temperature changes we find from the different input regions/depth levels.

The question is whether the numbers produced here for the increased melt rates are realistic or otherwise useful in some way. I don't think the authors have even tried to make a case for either usefulness or realism of these high melt rates. The only argument that was provided in the paper is that this approach here is "straight-forward and easy to run" but without it being realistic or useful, simplicity on its own, is not enough of an argument for publication.
Once we have our updated temperature, salinity and basal melt rate estimates we will include a more thorough comparison to findings from previous literature. In the revised manuscript we will also propose options for applications in prognostic ice-dynamical model studies.

A general characteristics of this paper is that the authors state assumptions but don't justify them. A good example of an unjustified assumption is the one that grounding lines are always accessed via 'prominent getaways' - that clearly doesn't hold in present day for the cold ice shelves Ross and Filchner-Ronne and others, yet this inconsistency is not at all addressed.
This was clearly a misunderstanding: We do not assume that ice-shelf regions are at present accessed by a prominent gateway. The first part of our study tries to identify gateways in all regions to find out whether or not there is a potential of an inflow at depth (and at what depth). In a second step, we want to estimate the potential change in melt rates assuming the continuous inflow of warm water masses from the continental-shelf break would be channelled through these gateways in all regions (if gateway-structure exists). We are sorry that the preprint did not convey this clearly enough. In a revised manuscript, we will make sure that all our assumptions are more clearly laid out.

Also there is some misuse of terminology. For example the temperature the authors have chosen for the CSB T and S is quite arbitrary, yet they call their perturbed melt rate result "upper limit". Surely not everywhere is this arbitrary point the temperature max along the shelf break, so even higher melt rates could be reached with PICO.
For a revised manuscript we will provide updated temperature estimates, with further explanations on where we would find the highest temperatures adhering to our upper-bound narrative.

I don't think the term warm-water intrusion is accurate for the use in the context of a long term, large scale and lasting change. Intrusion is an intermittent oceanographic feature. The sensitivity study here assumes that oceanographic conditions within the cavity change, that is warm water from the open ocean comes across the continental shelf break and stays and that is something very different and more difficult to establish than an intrusion which would largely mix in with other water masses on its way to the grounding line and become much cooler and fresher by the time it comes in contact with the ice.
Thank you for this comment. This is a good point and we will refrain from the term "warm-water intrusion" in the revised manuscript, as we rather consider a macro-scale/basin wide transition in melt mode associated with prevalent access of ocean water from off-shore.

The PICO model has some clear biases compared with the observations of Adusumilli et al. 2020, namely it overemphasizes a melt rate pattern of high melt at grounding line and low melt towards the front and does not take into account the 3D structure of the circulation, which results, for example, in omitting mode 3 melting features near ice shelf fronts. Accordingly, the melt rates 'assuming warm water intrusion' have the same biased melt rate pattern as the original PICO melt rates except now the melting is higher. Can you comment on the bias and its implications? For example in the context of Reese et al 2018 - if grounding line are most sensitive to melt rate change, overestimating melting there could be problematic, yet it is probably happening since the bulk melt rates are tuned to agree with observations and mode 3 is absent - resulting in freezing or low malt rates near the front - positive bias in grounding line melt rates is clearly visible in sectors 3-5.
In the revised manuscript we will include a more thorough discussion about the biases introduced by PICO. Please also refer back to our explanation in our initial reply to your comment that we have posted on February 2nd, 2024:
https://egusphere.copernicus.org/#AC1

Specific

Access depth seems to be a key concept here but it is not clearly defined (I think the language is the problem). Figure 1 doesn't help - it is stated in the text that access depth is a field defined everywhere (and provided on a certain discrete grid) but the figure only points to a single point in the image, which is confusing. Further on Fig one - what is the -1800 m in the image showing horizontal distance? Shouldn't that be depth for the purposes of your continent definition?
We will provide a new schematic Figure 1 to highlight key concepts used in the study. In this new Figure it will be shown how, from a 2D access depth field, we can extract the access depth at the grounding line. The z=-1800m contour is indeed used for the purpose of our continent shelf definition.

Similarly g is not clearly defined. From the paper it is sort of clear what the authors mean from the context but that is relying on the reader being on board with the writers.
A more comprehensive explanation will be given in the revised manuscript.

Sign convention of z vs depth needs to be consistent.
Will be clarified in the revised manuscript.

Fig 7 and similar - x axis needs to be labeled on each subplot to make clear what distance is meant for each case
Will be clarified/changed in the revised manuscript.

other specific comments are in the attached pdf

Please find below the individual replies to the comments that we extracted from the provided PDF using the function "Print comment summary" in Adobe Acrobat 2017. If necessary, we have cited the corresponding text passage in black.

Supplement comments (in attached PDF)

Page: 1
l. 14: Reviewer Subject: Comment on Text: "Sub-shelf melting around Antarctica varies by orders of magnitude"
the term is typically "ice-shelf basal melting"
Will be changed in the revised manuscript.

Page: 2
l. 40: Reviewer Subject: Highlight
in terms of sign convention, I think z is typically defined up from sea level positive and down negative, but depth (in oceanography) is positive down (defined as negative of z)
Will be clarified in the revised manuscript. In the revised manuscript, we will use positive values when referring to "depths".

l. 43: Reviewer Subject: Highlight
do you mean bathymetry here?
In this manuscript, we look at bathymetry solely, but the data product (BedMachine) also covers the Antarctic subglacial topography, hence we said "topography data". We can call it bathymetry to make it more clear what we are focusing on in the study.

l. 50: Reviewer Subject: Highlight
is "effective erosion" a technical term different from "erosion" or do you just mean the effect of erosion?
We used it to describe that erosion is very effective over long time scales. The word "effective" will be deleted in the revised manuscript for clarity.

l. 50f.: Reviewer Subject: Highlight
but then higher near the very front again due to mode 3, and the reasons for this pattern are different in different types of cavities so maybe includign a line or two elaborating on this statement might help a more general reader
Thank you for pointing this out to us. We will rephrase this part in the revised manuscript.

l.51: Reviewer Subject: Highlight
not just further modulated but that is also how it is set up to begin with
We will omit the word "further" in the revised manuscript.

l. 52: Reviewer Subject: Highlight

Coriolis effect
Will be changed to "(e.g. the Coriolis effect)" in the revised manuscript.

Page: 3
l.59f: Reviewer Subject: Highlight "..., but only a few studies investigate the bathymetric access points or pathways to the grounding lines in detail and…"
this is definitely untrue, the role of bathymetry in local sub-ice-shelf circulation and warm water access is the subject of many studies, typically that happens as soon as new bathymetry survey or ocean cruise takes places in the follow up publication
We perform a circum-Antarctic assessment of bathymetric access and potential pathways of ocean water to the grounding lines of the Antarctic Ice Sheet, which we believe is not included in publications following up on new bathymetry surveys or ocean cruises that focus on specific regions. We will change the statement in the revised manuscript.

l. 62: Reviewer Subject: Highlight *"assuming that water follow this pathway"* this is a strong assumption, and in case it is not like that reality, then I don't see a justification for this assumption
In at least two modeling studies focusing on the Filchner-Ronne Ice Shelf we see an inflow through Filchner Trough (Hellmer et al., 2012, Naughten at al., 2021). Our study provides an analysis for a sensitivity-experiment, where in case of a trough-like feature, we assume the access of off-shore ocean water is possible (as in the case of Filchner Trough), but our model cannot tell under which conditions this access could be realized. Our assumption that, once warm CDW is flowing onto the continent, it will eventually reach the grounding line can be motivated by the fact that CDW is not only warmer but also saltier and therefore denser than on-shelf waters, such that we expect it to sink from the shallowest overflow point, eventually towards the grounding lines, and filling up the cavity basin, replacing the less dense waters below access depth.

l. 68: Reviewer Subject: Highlight
which current?
This was meant as "present-day" estimates, which we took as synonyms of T_CF conditions. Will be rephrased in the revised manuscript.

l. 68: Reviewer Subject: Highlight
potential with respect to what?
This was meant as the potential change in melt rates with respect to the estimate derived from the calving front (T_CF). Will be rephrased in the revised manuscript.

l. 70ff: Reviewer Subject: Comment on Text: "Our approach of identifying relevant water masses that drive melting in cavities is also useful to improve the input for parameterisations of sub-shelf melt rates such as the ice-shelf cavity model PICO as suggested in Burgard et al. (2022)."
it is unclear what you mean here at this point, maybe more relevant for discussion section
This sentence serves as motivation and framing of our study, as in some basal melt rate parameterizations depth levels for each basin have to be selected over which ocean temperatures and salinities are averaged to feed into the model. Here the concept of access depths for warm water masses off the continental shelf might be of interest for the ice-sheet

modeling community using such simple models. We will rephrase this section in a revised manuscript.

l. 87f: Reviewer Subject: Highlight
I don't understand this definition, how are the routines defined, and what
decides which value is assigned to each cell?
The flood-fill algorithm iterates through the grid and from a seed point spreads out in all four directions (the code checks the four neighbors of the current point: up, down, left, and right), fills adjacent cells with a specific value e.g. "flooded". until it reaches boundaries or encounters obstacles (cells that are not below the threshold, i.e. shallower bathymetry).
For clarity, in the revised manuscript, we will add additional explanations on the used flood-fill algorithm, provide a new Figure 1 explaining the key concept, include the flood-fill code, as well as explanatory animations (see https://zenodo.org/records/10599774) in a supplement.

l.88: Reviewer Subject: Highlight
Here is where your sign convention of depth becomes complicated - deepest means largest positive depth, which according your definition would be zero, as depth is negative below sea level - I don't think that is what you mean here
Will be changed/clarified to "the deepest level (largest positive depth)" in the revised manuscript.

l.88: Reviewer Subject: Highlight
from?
Yes. Will be changed in the revised manuscript.

Page: 4
l.89: Reviewer Subject: Highlight
how do I know which one?
This can be done by comparing the bathymetry value from the BedMachine with the value from the Access Depth array (which results from the flood-fill): If the bathymetry at point x,y is deeper than the derived access depth at point x,y; one knows the point is "shielded" by shallower topography "blocking" the way of e.g. warm water onto the continental shelf.
The difference between access depth and BedMachine (on a 8km x 8km grid) is plotted hereinafter:

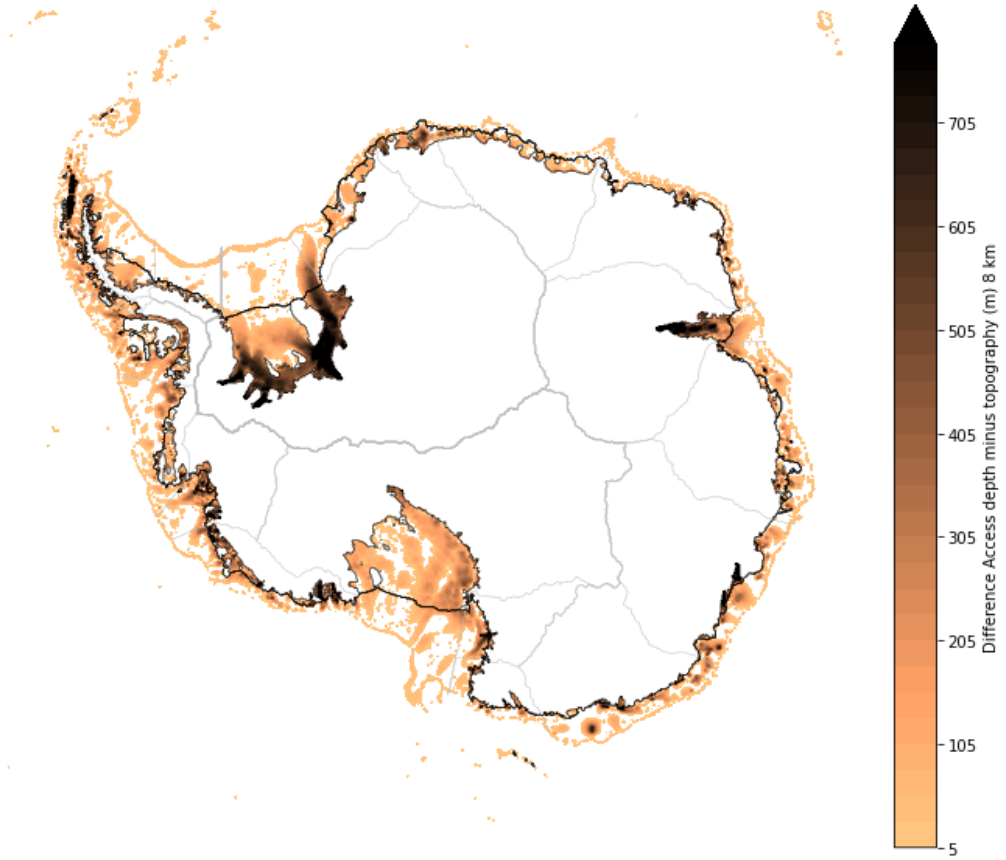

Fig. 1: Difference between the access depth field from the initial manuscript and BedMachine bathymetry input on an 8kmx8km grid spacing.

We plan to include such a figure in the supplement of the revised manuscript.

l.93: Reviewer Subject: Comment on Text
this needs a more precise definition, it might be good to define d_c and g in two different sentences, two definitions in one is too much to be clear
Thank you. We will rephrase this in the revised manuscript.

l.94: Reviewer Subject: Comment on Text
Not sure what you mean here by this, g can take any value between 0 and 1 right? - not sure what these bounds mean and where they come from.
what kind of steps are taking?
We evaluate the length of the grounding line of each basin as a whole. From the 2D field of access depths, generated through the flood-fill, we take the access depths at the grounding lines and see which values are present. For example, 50% of a basin Y's grounding line is connected to the open ocean at -550m, so d(Y,50%)=-550m. We aim to clarify the concept of g(%) in the revised manuscript, see overall comment above.

l.101: Reviewer Subject: Comment on Text
similar or the same?

See above. The basic concept of a flood-fill is the same. The code and used data are different. We only use the bathymetry to infer the access depth and do not extrapolate temperature and salinity estimates into the cavities. We will clarify this.

Page: 5

l. 109f: Reviewer Subject: Comment on Text
according to this definition the ice shelf base would also pass s a calving front
Will be changed to "the horizontal (map-view) boundary at the surface between floating ice and the ocean" in the revised manuscript for clarity.

l.130: Reviewer Subject: Highlight
capture?
Yes, we will change this in the revised manuscript.

l.131: Reviewer Subject: Comment on Text
which variables are the input?
We feed temperature and salinity estimates into PICO. We will clarify this in the revised manuscript.

Page: 6

l.141: Reviewer Subject: Underline
a
Will be changed in the revised manuscript.

l.142: Reviewer Subject: Cross-Out
Will be changed in the revised manuscript.

l.144: Reviewer Subject: Comment on Text: ", we assume that the gateway(s) provide access to a significant amount of the grounded Antarctic Ice Sheet e.g. for dc(g = 50 %, b) in Fig. 2." why does that need to be an assumption? can't you calculate that? or are you referring to things you omit here such as ocean circulation? Please describe what that assumption entails.
Yes we did calculate the access depth but we do not simulate the actual ocean inflow. We also do not simulate the ice-dynamical response, such that we have to make assumptions about what portion of the grounding line needs to be accessed by waters at a certain depth to be "significant". This was displayed in Preprint Fig. 3 i.e. what fraction of the basin's grounding line is connected to the ocean at which depth. For visualization purposes we picked g=50%, to reference a value in Preprint Fig.2 or Fig. 5. We aim to clarify the concept of g(%) in the revised manuscript (see overall comment above), and refrain from displaying the values only for an arbitrary value of g=50% for each region.

Page: 7

Figure 2: Reviewer Subject: Sticky Note
What are the straight white lines along on the continental shelves?
The straight lines stem from the used boundaries of the 19 PICO basins. We will add the basin boundaries as a new legend entry and in the figure description, so that it will be clarified in the revised manuscript.

Figure 2 (caption): Reviewer Subject: Highlight
maybe visualize it with a contour?
We plan to leave the reference values of g=50% out of the figure.

l.170: Reviewer Subject: Highlight
shallower? or narrower?
This was meant in terms of 'provided access to the grounding line'. We see that the sentence could have been misunderstood and we will rephrase it to "regions with less pronounced gateways" in the revised manuscript.

Page: 8
Figure 3: Reviewer Subject: Sticky Note
for Totten the -370 is 3 boxes, not 2 as highlighted
Thanks for this hint, will be corrected in the revised manuscript.

l.171: Reviewer Subject: Highlight
I don't understand what you mean here
We mean that the difference between T_CSB and T_CF is very pronounced, when only considering a small amount of the grounding line. Will be clarified in the revised manuscript.

l.171ff: Reviewer Subject: Highlight "If those parts of the grounding line also have the highest grounding line depths, warm water intrusion at depth could cause significant melting in the region."
I am not following here - but either way this sentence sounds more like discussion and not results so perhaps better placed there with some context?
We propose to rephrase this part to "If those parts also coincide with thick and thus deep-lying ice at the grounding line, access of warm water at depth could cause significantly more melting in the region (i.e. as the pressure melting point of ice decreases with depth)".

l.173: Reviewer Subject: Cross-Out
Will be changed in the revised manuscript.

l.176: Reviewer Subject: Highlight
I don't think you defined what critical T and S are
We here again apologize for the imprecise handling of our terminology. The term "critical temperature/salinity" was used by us to indicate the temperatures/salinity related to a specific value of dc(g) which was derived from the access depth. Since we discuss the access depths based on grounding line access (=g, in different % of total grounding line in a specific region), we see that the word "critical" might not be appropriate here. We will clarify this in the revised manuscript.

l.177: Reviewer Subject: Highlight
can you explain why shallow depths are a problem?
In our initial analysis, some of this region's continental ice is grounded above sea-level and has no access to the (open) ocean, only 20% (displayed in Preprint Figure 3). To be consistent with the ice shelf parts, where PICO parameterizes melting, we will leave out all parts of the grounding ice that are > 0m.

l. 178: Reviewer Subject: Highlight
both Ross and Drygalski?
No, for both basin 17 and basin 11. Will be clarified in the revised manuscript.

Page: 12
l. 233f: Reviewer Subject: Comment on Text
How do the numbers change if you use that one instead?
We will include this in a revised manuscript.

l.236: Reviewer Subject: Comment on Text
here and elsewhere, see major comment regarding terminology "intrusion"
Will be clarified in the revised manuscript (see general comment on the terminology above).

l. 261f: Reviewer Subject: Comment on Text
Can you justify why this is a good assumption rather than just stating that you are making it?
In our study we identify the deepest trough (Glomar Challenger) and assume that warm water could be redirected through it to the region's grounding line. This assumption is based on our scientific question for this study: By how much would the melt rates increase around Antarctica if warm water masses off the shelf would intrude onto the continental shelf through the deepest trough / at depth?

l. 262f: Reviewer Subject: Comment on Text
I don't understand what new and independent you found that you are comparing to the finding of Tinto et al here.
Our findings are in line with Tinto et al. that find that the bathymetry constrains sub-ice-shelf ocean circulation, protecting the ice shelf grounding line from moderate changes in global ocean heat content.

Page: 13
l.283ff: Reviewer Subject: Highlight
How well does the model resolve the region, does it have ice shelves and if so what melt rate change it produces?
Gómez-Valdivia et al., (https://doi.org/10.1029/2023GL102978) employ the UKESM1 Earth System Model with a relatively coarse resolution (1° ocean model) according to the paper. Their paper focuses strongly on the evolution of the Ross Gyre. As far as we can tell, the paper does not state melt rates and does not give any explanation whether or not interactive ice shelves are included in the model. We can add "employ a global climate model on a relatively coarse resolution" to this part in the revised manuscript for clarity.

l.293ff: Reviewer Subject: Highlight
Those two studies aren't comparable as one uses small number of point measurements and the other number is an area average
Thank you for pointing this out to us. We will correct this in the revised manuscript. As we compare basin wide average we will omit citing the Vaňková et al., 2023 study and rephrase the sentence to: "Rignot et al., 2013 find melt rates at Totten Ice Shelf to be 10.47±0.7 m yr−1".

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l.310: Reviewer Subject: Cross-Out

Will be changed in the revised manuscript.

l.312: Reviewer Subject: Comment on Text

"assume that ocean waters in front of the ice shelf serve as valid proxy for water masses that currently drive melting underneath the ice shelf, which is generally valid for cold-mode ice shelves (Silvano et al., 2016)."

can you comment on warm ice shelves also?

We will reword this sentence and include "which is not true for warm-mode ice-shelves".

l.313: Reviewer Subject: Cross-Out

"assuming that flow simply follows the ~~bathymetry~~"

isobaths

We assume that the water intruding into the cavity is following the bathymetry at the same and lower depth. Using "Isobaths" would in our view hence not be appropriate here. As stated above, we assume that the warmer/saltier/denser CDW sinks from the shallowest overflow point eventually towards the grounding lines, fills up the cavity basin and replaces the less dense waters below the access depth.

l.313: Reviewer Subject: Highlight

"Second, we estimate the continental-shelf break temperatures at the same depth, assuming that flow simply follows the bathymetry, and not, e.g., isopycnals (Drijfhout et al., 2013)."

what flow are you talking about actually?barotropic? baroclinic? When speaking about intrusion - it would typically be along ispycnals

In the mentioned text passage we exactly wanted to acknowledge that warm water intrusions occur along isopycnals, and that we do not account for that due to the simplicity of our approach. Here, our assumption is, that the CDW is not only warmer but also saltier and therefore denser, such that we expect it to sink from the shallowest overflow point on the continental shelf, eventually towards the grounding lines, and filling up the cavity basin, replacing the less dense waters below access depth. We can rephrase the sentence in a revised manuscript for clarity.

l. 314: Reviewer Subject: Comment on Text

As you admit, ocean circulation is a crucial to sub-ice shelf conditions and melt, how relevant is your study in light of this assumption?

Our study aims to provide circum-Antarctic estimates for a potential mode 2 onset. Our estimates only take into account the bathymetric constraints. More sophisticated, high-resolution, coupled ice-ocean modeling is needed to assess the potential and boundary conditions for mode 2 onset in all regions. While we do not include ocean circulation, we here provide a first-order assessment, in case of a transition from mode 1 to mode 2 melting conditions.

l.317: Reviewer Subject: Comment on Text

“Our study could therefore be improved by considering specific ocean circulation patterns informed by high-resolution ocean models.”
what do you mean by our study - can you be specific here about what your study achieves?
For instance, high-resolution ocean dynamical models could suggest that access is more likely through the second deepest channel, or that even deeper ocean levels that at access depth should be considered. In a revised manuscript, we will elaborate on this.

l.323: Reviewer Subject: Comment on Text
“our identified gateways could be an entry point to cross-cut the density barrier in front of the continental shelf (Hirano et al., 2023)”
what do you mean by a a density barrier? the tilted isopycnals? the slope current? the pycnocline?
Thank you for pointing out that this sentence needs rewording. We refer to the Antarctic slope current in this part. Will be clarified in the revised manuscript.

l.324: Reviewer Subject: Highlight
it is density, not temperature alone that is the dynamically important quantity
Yes agreed, we just wanted to highlight which quantity is more relevant for melting. Will be rephrased in the revised manuscript.

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l.330: Reviewer Subject: Highlight
limitations would be more accurate - uncertainty has a specific well defined meaning
Yes, we can agree on this; will be changed in the revised manuscript.

l.332f: Reviewer Subject: Highlight
More than that, the bathymetry at most places is not even well known at this resolution
This is true, but as ice-sheet modelers we need to work with what is given, and we aim our analysis to be useful for us/them. A resolution of 500m in the BedMachine Dataset is the current state-of-the-art and incorporates most recent findings/discoveries.

l.345: Reviewer Subject: Comment on Text
I don't know what you mean here
*The word “altered” was used here as a different word for “changed”/”modified”/”different”. Will be changed to “our melt rate estimates could **differ** when using an alternative melt parameterisation” in the revised manuscript.*

l.346: Reviewer Subject: Sticky Note
It would be fair to state explicitely that this particular study did not find good agreement between PICO and reference coupled model, since you say that the earlier Favier et al does
We can take this up in a revised manuscript, but the PICO implementation in that study used a completely different PICO parameter tuning.

l.350: Reviewer Subject: Comment on Text

I don't know if full ensamble but a few endmembers might be useful to provide some sort of uncertainty

In a revised manuscript, we aim at providing the obtained basal melt rate estimates using the minimum, best-fit and maximum PICO parameter values from Reese et al., 2023.

l.352ff: Reviewer Subject: Sticky Note

This whole paragraph is unclear - explain what is being adjusted where, and why it is no longer needed and why it was needed earlier. Or alternatively leave out as this paragraph doesn't seem to be connected with the paper much - sounds more like an outcome of Reese et al 2023 since no new modifications of PICO were introduced in this paper

Thank you for your feedback. We will rephrase this part in the revised manuscript (see also general comment above).

l.353: Reviewer Subject: Highlight

formerly meaning when/where?

It was meant as "in earlier studies". Will be changed to "A comparison of input temperatures ***used in earlier studies*** *with PICO and the temperatures extracted in this study" in the revised manuscript.*

l.354ff: Reviewer Subject: Comment on Text

This statement needs some context for those who don't know what adjusted temperatures are

Will be clarified / extended on in the revised manuscript.

l.356ff: Reviewer Subject: Comment on Text

This should probably be in the methods together with where PICO is introduced

We agree. Will be changed in the revised manuscript.

l.364: Reviewer Subject: Highlight

Can you just propose a suitable subdivision in stead of speculating about one?

Thank you for pointing out this aspect. Yes, considering the access depth and pathways of the major ice-shelf regions (namely FrIS, Amery and George VI) we will propose following new PICO boundaries:

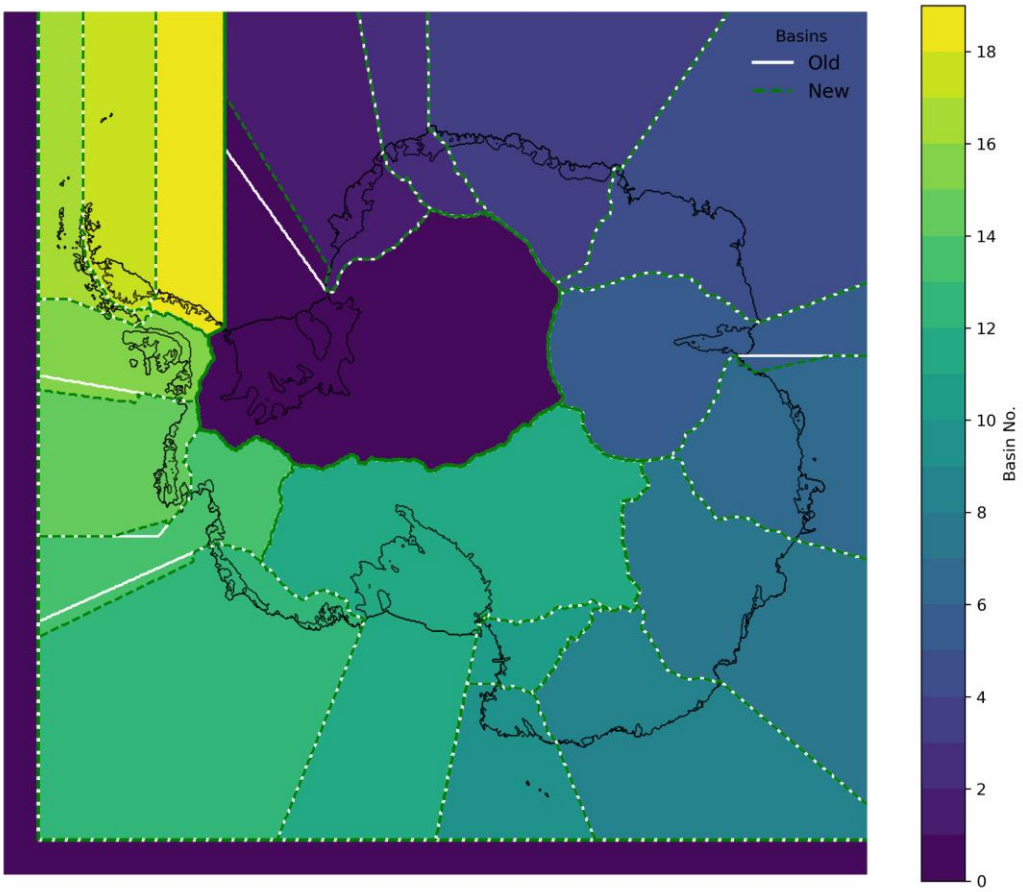

Fig. 2: New proposed PICO basin boundary definitions based on Oceanic Gateway analysis in Filchner-Ronne, Amery and Amundsen Sea region.

We will provide this updated basin mask on an 8kmx8km grid spacing.

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l.371: Reviewer Subject: Highlight

do you mean sectors? since you don't actually analyze properties in cavities?

Yes, we meant the sectors or basins, we differentiate based on the existing 19 PICO regions. Will be clarified to "in some regions" in the revised manuscript.

l.378: Reviewer Subject: Highlight

"In some basins, warm water masses accessing g = 30 % of the region's grounding line could be sufficient to reach all *fast flowing ice* to cause significant ice loss, but in others g > 50 % is required."

is it necessarily true that change of thickness of already fast flowing ice produces more retreat than change of thickness of initially slow flowing ice? If not, why is it significant to reach fast flowing ice

We apologize for any confusion caused regarding this point. Fast ice flow does not directly contribute to sea level rise, but it often coincide with ice stream structures in deep-laying fjords. Reese et al., 2018 (The far reach of ice-shelf thinning in Antarctica. Nature Clim Change 8, 53–57 (2018). https://doi.org/10.1038/s41558-017-0020-x) show that the highest response of grounded ice towards the same amount of thinning is found downstream of fast-flowing ice.

l.379: Reviewer Subject: Highlight
“For those regions the most vulnerable parts of the grounding line may be located in shallower parts”
what is the exact meaning of vulnerable here?
We apologize again for our vague wording. In our manuscript, we have argued that regions with deeper access depth are more vulnerable to warm water inflow from the continental-shelf break i.e. have a potential of losing large portions of upstream ice. Warm CDW resides at the continental-shelf break at mid-depth. Shallow parts of the grounding lines are hence less vulnerable. We will correct this passage in the text.

l.394: Reviewer Subject: Highlight
what you use is to a large extent an extrapolation of observations, isn't it?
Yes, it is true that, for the ocean dataset, the observations are extrapolated. We evaluate these with the bathymetry data and grounding line positions from the BedMachine dataset. If a newer dataset becomes available one can update our estimates, as we use the ISMIP6 climatology as present-day temperatures. We will clarify this point in the revised manuscript.

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l.407: Reviewer Subject: Comment on Text
what do you mean by this?
It means that this method is not as complicated as running a coupled-ice ocean model. It can be done by running a few python scripts on a high-performing computer. Our analysis can also be done on a simple personal computer. We will extend on the “feasibility” in the revised manuscript.