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 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.
Amendments to the first point-to-point response from the initial upload are written in green (last update: July 30, 2024).

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.
We have changed the title of our manuscript to: "Bathymetry-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 method section we have extended the information about the used algorithm including a reference to the implemented code. Please find the new Figure 1 in the revised manuscript.

*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. cf. l.122f in the revised manuscript.*

*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). cf. Figure 4 in the revised manuscript.*

*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. We have addressed this point in the revised manuscript.*

*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.*

cf. lines 115ff. in the revised manuscript.

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.

cf. lines 156ff. in the revised manuscript.

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.
cf. l.121f. in the revised manuscript.

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 reevaluate 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 Antarcrtica 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. cf. for example l.506f in the revised manuscript.

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 channeled 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.
cf. lines 114ff. in the revised manuscript.

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.
We have addressed this comment throughout the revised manuscript.

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
For this point, for example, compare lines 471ff. in the revised manuscript.

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. Please find a new Figure 1 in the revised manuscript.

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. We have addressed this comment throughout the revised manuscript.

Sign convention of z vs depth needs to be consistent.
Will be clarified in the revised manuscript. We have addressed this comment throughout the revised manuscript. All affected figures are changed accordingly.

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. All affected figures are changed accordingly.

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. Done.

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". We have addressed this comment throughout the revised manuscript. All affected figures are changed accordingly.

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. Changed to: "While large data gaps still exist, recent **Antarctic bathymetry data** incorporate major glacial troughs, ridges or other features of basal topography crosscutting the continental shelf".*

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. Done.

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. Done. Added: ", if mode3-melting is absent".

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. Done.

l. 52: Reviewer Subject: Highlight

Coriolis effect

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

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.*

Changed to "but previous studies do not systematically investigate the bathymetric access points or pathways to the grounding lines with regards to ice-sheet modelling and focus only on specific regions"

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. cf. for instance lines 140f. in the revised manuscript.

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.

Changed to: "We combine observations of bedrock topography and ocean water masses to assess present-day pre-conditions for enhanced melting in all Antarctic regions."

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. Rephrased. See above.

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. Done.

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?

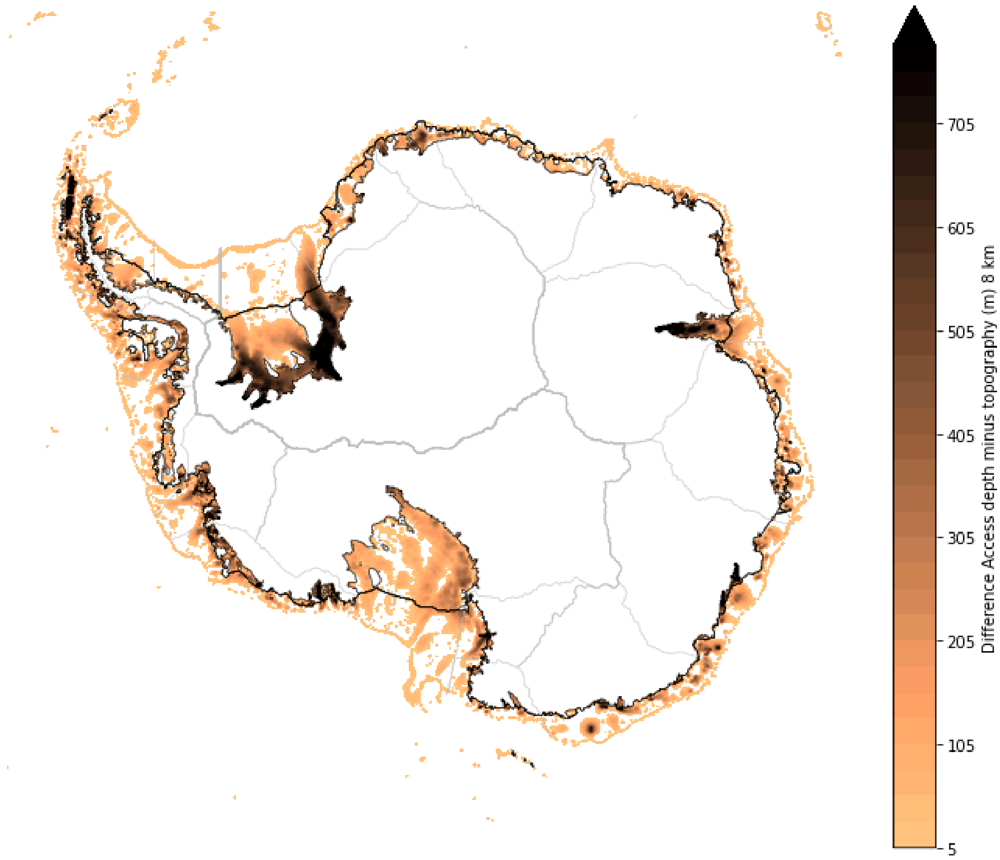
The flood-fill algorithm iterates through the grid and from a seed point spreads out in all four directions (the code checks the four neighbors of the current i,j point: up, down, left, and right), fills adjacent cells with a specific value e.g. “flooded”. until it reaches boundaries or encounters obstacles (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.

In the revised method section we have extended the information about the used algorithm including a reference to the implemented code. Please find the new Figure 1 in the revised manuscript

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. Done.

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

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:



Review Fig. 1: Difference between the access depth field from the initial manuscript and BedMachine bathymetry input on a 8kmx8km grid spacing. ~~A more detailed version is attached as Fig. 3 in the revised manuscript.~~

~~We plan to include such a figure in the supplement of the revised manuscript.~~
Please find such a figure as Fig. 3 in 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. We have addressed this comment by reworking our methodology.

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. We have addressed this comment by reworking our methodology.

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 is 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. cf. again l.122f in the revised manuscript, where we address the differences to the ISMIP6 approach.

Page: 5

l. 109f: Reviewer Subject: Comment on Text

according to this definition the ice shelf base would also pass s a calving front

We have changed this to: "The calving front (CF) is defined through the native BedMachine mask as the horizontal boundary between floating ice and the ocean". Done.

l.130: Reviewer Subject: Highlight

capture?

Yes, we will change this in the revised manuscript. Done.

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. Changed to: "Input (T,S) to PICO in Reese et al. (2023) is based on temperature and salinity observations compiled by Schmidtko et al. (2014)."

Page: 6

l.141: Reviewer Subject: Underline

a

Will be changed in the revised manuscript. We have rewritten this section in the revised manuscript.

l.142: Reviewer Subject: Cross-Out

Will be changed in the revised manuscript. We have rewritten this section 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. We have addressed this comment by reworking our methodology.

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.
Done.

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. We have revised this figure entirely.

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. We have rewritten this section 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.
As we have restructured our manuscript and updated our analysis, a revised version from Figure 3 was moved into the Supplement.

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.
We have rewritten this section 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)".
We have rewritten this section in the revised manuscript.

l.173: Reviewer Subject: Cross-Out
Will be changed in the revised manuscript. We have rewritten this section 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.
We have addressed this comment by reworking our methodology.

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.
We have addressed this comment by reworking our methodology.

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

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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.
Using adjusted basin boundaries for the different 19 PICO regions now incorporates a wider perimeter of the continental-shelf break near Amery Ice Shelf. We thus capture a different inflow in Amery as well.

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).We have addressed this comment throughout the revised manuscript.

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?
We have slightly rewritten this section of the revised manuscript.

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. We have slightly rewritten this section of the revised manuscript.

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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.
cf. l.381 in the revised manuscript.

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". Done.

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l.310: Reviewer Subject: Cross-Out
Will be changed in the revised manuscript. Done.

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".*~~
We have changed this part to: "which is generally valid for cold-mode ice shelves, but not for shelves with warm-mode melting"

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.
We have changed it to: "assuming that water masses simply follow the bathymetry when flowing onto the shelf, and not follow isopycnals"

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.

We have included this aspect from e.g. l.139f in the revised manuscript.

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.

cf. l. 427f. in the revised manuscript.

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. Done.

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. We have added "and resulting changes in density".

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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.
Changed.

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.
To this point, we have added in l.451: A resolution of 500 m in the BedMachine Dataset is the current state-of-the-art and incorporates most recent findings.

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. Done.*

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.
We have added the following sentence in the revised manuscript: "It is to note here that Burgard et al. (2022) does not find good agreement between PICO and a reference coupled model, but the PICO implementation in that study also uses 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. We have addressed this comment by reworking our methods.

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.

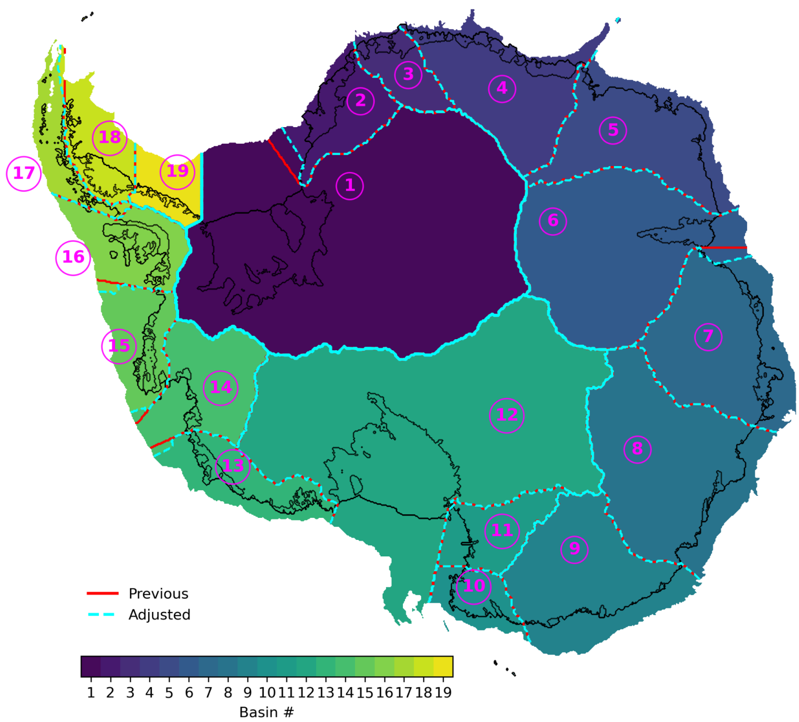
Thank you for your feedback. We will rephrase this part in the revised manuscript (see also general comment above). We have rewritten this section in the revised manuscript.

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.~~ We have rewritten this section 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. We have left out this part 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. Done.

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 PICO boundaries. We will provide this updated basins mask on a 8kmx8km grid spacing. Please find the new PICO boundaries as Fig. 2 in the revised manuscript.



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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. Done.

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. We have addressed this comment by reworking our methodology.

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.
We have addressed this comment by reworking our methodology.

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.
We have changed this part to "We combine latest available bathymetry data with present-day ocean temperature and salinity data".

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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.
We have changed this part to: "While high-resolution ocean modelling could provide a more detailed estimate on the effect of oceanic gateways on melting, our first-order approach is instead straight-forward and easy to run, meaning that only a few analysis scripts are necessary to (re-)produce our results. When new bathymetry or ocean temperature data

becomes available, our study can be repeated in an instant, even on a 500mx500m grid spacing."

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).
Thank you for your suggestion. We have changed the title to: "Bathymetry constrained warm mode melt estimates derived from analyzing Oceanic Gateways in Antarctica"

    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). Done.

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.
We have addressed this point in the revised manuscript from l.357f.

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. We have addressed this point by revising our methodology.

   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. We have addressed this point by revising our methodology.

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. cf. Figure 2 in the revised manuscript.

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. cf. the revised manuscript.

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. We have addressed this point in e.g. l.288f in the revised manuscript.

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. cf. l. 161f in the revised manuscript.

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. cf. the revised manuscript.

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. cf. e.g. l. 469f. 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. cf. the new Figure 6.

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.Changed to "They experience melt rates up to the order of tens of metres per year (cf. area-average basal melt rates for Pine Island and Thwaites in Rignot et al., 2013).”

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. Done, see for example l.70.

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. Done.

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. In order to stream-line the manuscript, we have deleted this part 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”. Done.

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. Done.

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. Done, cf. l.110f.

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. Please find the new Figure 1 in the revised manuscript.

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. cf. l.122f 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). Please find the new Figure 1 in the revised manuscript.

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. cf. l.168 in the revised manuscript.

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.~~ We have changed it to: "We use the PICO implementation in the Parallel Ice Sheet Model".

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. We have rewritten this part 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. In the supplement we have now included a Figure (Fig. S5) providing the temperature estimates relative to the pressure-melting point i.e. the Thermal Driving (estimated at the deepest grounding line depth per 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). Done.

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. We have rewritten this part 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. Done.

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. Done.

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. Done.

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 500 m 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. We have deleted this part in the revised manuscript in order to stream-line the 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. We have deleted this part in the revised manuscript.

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

Response to RC3

General comments

Nicola and coauthors identify key oceanic pathways in the major Antarctic glacier basins and estimate current and future melt rates based on the present water properties, assuming that warm waters from the shelf break reach the grounding lines. I found it interesting to see the range of access depths and ocean water properties around the Antarctic Ice Sheet in one simple figure, and it is interesting to quantify the upper limit of future melting assuming warm water intrusion through these pathways. The data were clearly visualized, making it easy to synthesize the wealth of information presented. The analyses and profiles shown for the major ice shelves were especially nice to see and will make this study of interest to a range of groups who study various components of the Antarctic ice/ocean system.

I do not have any major concerns about the approach or the conclusions, as the caveats associated with this methodology are clearly noted in the discussion. I have several comments mostly relating to the presentation of the study, so I recommend this study for publication after minor revisions. I hope the authors find these comments helpful.

Dear Anonymous Reviewer 3,
Thank you very much for your review of our manuscript and your feedback. We will revise our manuscript keeping your suggestions close in mind. In addition to the general comment to all reviewers, please find our detailed point-by-point responses (written in blue and italics) to your comments (in black) *below.*

Specific comments

1. The motivation of the study could be more clearly laid out. Ocean gateways are important for ice shelf melting, but why does this analysis help the scientific community? Rather than giving a lengthy overview of why troughs and gateways are important and then explaining the approach, I suggest making a more concise overview of the importance, state (more clearly) what the critical uncertainties are, what the approach is, and what the hypothesis is.

Thank you for pointing this out to us. We will propose a more stream-lined introduction in the revised manuscript.

2. What are the criteria for classifying something as a ‘prominent gateway’? In L145 the authors state that “large portions” of the GLs need to be reached, but how was this determined?

We apologize for any confusion regarding this definition. We will clarify this in the revised manuscript (see general comment above). We have addressed this remark by revising our methodology.

3. Section 3.3.4 (Amundsen Sea) – the main gateway is identified as the Abbot Cosgrove Trough. Is this distinct from the ‘Eastern Trough’ commonly referred to in studies about the Amundsen Sea (e.g., Dutrieux et al., 2014 already cited in this study)? Does it feed into the PIG-Thwaites Trough shown in Fig 9a, or is it separate?

It would be helpful to clarify which of the oceanic gateways identified are/aren't in agreement with the main pathways identified in previous studies.

We appreciate you bringing this to our attention. We will elaborate on this in the revised manuscript.

4. The mismatch in melt rates between the approach in this study and the melt rates of Adusumilli need to be addressed, as that is critical for gauging how reliable the estimates of future melt rates are. Some regions do better than others, perhaps due to different reasons, so I'd suggest explaining this for each of the major ice shelf regions.

Thank you for this suggestion, when revising our basal melt rate estimates (see general comment above) we will make sure to add a more in-depth discussion to it in the revised manuscript.

5. I found Section 3.3 rather hard to read. Rather than explaining the results and then reviewing various relevant literature, I'd suggest reviewing the findings from the literature and then presenting the new results within that context. It feels very scattered in its present state.

Thank you for this feedback. We will restructure this section in the revised manuscript.

Other suggestions (to improve the presentation, not necessary for publication in my opinion):

1. In Figure 2, it would be nice to see the temperature relative to the pressure melting point.

We agree that stating the temperatures relative to the pressure-melting point would be insightful, as it would 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.
In the supplement we have now included a Figure (Fig. S5) providing the temperature estimates relative to the pressure-melting point i.e. the Thermal Driving (estimated at the deepest grounding line depth per basin).

2. Several figures likely need larger text to for readers to see the numbers in each of the small boxes – probably fine for PDFs, but not printing. Perhaps the numbers are not critical for understanding the figures, as the colors also reflect the values.

When possible, we will increase the fontsize of the respective figures in the revised manuscript. Done.