Point-to-point response to reviewer comments - Major Revisions (2. Iteration)

Dear Editor, dear reviewer,

Thank you again for finding the time to handle and review our manuscript as well as providing such constructive and thorough feedback to rewrite and restructure our manuscript. Following the reviewer's suggestions, we have revised the text, for instance, by re-ordering the different sections as proposed, we have sharpened our definition of what an "oceanic gateway" constitutes and tied our results more strongly together to provide further interpretation and coherence of our results.

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. The Editor comment is given in *orange with italic font. The line numbers given in the responses refer to the revised manuscript of this round (without highlighted/tracked changes).*

Editor comment

Dear Nicola et al. the reviewers noted considerable concerns with the original manuscript and in particular the interpretation of the results. This has been partly answered in the revisions. However, given that there are still key remaining issues I will only reconsider the manuscript after the comments from the second round of review have been addressed in full. Please make sure to respond to all the points, in particular, make sure to work on the interpretation of the results and the coherence of the writing.

Reviewer comments

Second review of: Bathymetry-constrained warm-mode melt estimates derived from analysing Oceanic Gateways in Antarctica by Lena Nicola et al.

Date: 1 Oct 2024, Reviewed by: Erwin Lambert

The authors have put in a great effort to address my concerns raised in the first review round. The possible errors have been resolved, the methodology has become more logical and several metrics are now presented in a clearer and more concise way. Additionally, the authors have modified their figures to address several concerns.

I do, however, still have some concerns regarding the interpretation and writing which I believe should be improved before I can recommend this manuscript for publication. Although these concerns can be resolved by rewriting solely, without the need for new analysis, I still recommend a major revision. This is because I believe the depth of interpretation and coherence requires a considerable effort to raise the level of the manuscript to the level needed for publication in The Cryosphere. That being said, I am confident that the authors can reach this level based on the current state of the work and the suggestions below.

Major comments:

1. Coherence.

At points, the manuscript reads as separate individual sections/paragraphs without a strong coherence. This makes it difficult to interpret the results. In particular:

a. The authors provide a quantitative distribution of access depths (Fig 4). These distributions are referred to in the main results section (Sec 3.3). However, the impact of these distributions is not tied to the results in terms of temperature/melt changes. In fact, I believe through the use of PICO, there is no impact and the authors appear to agree (l. 467). Would the main results for Filchner-Ronne change if only 1% of the GL were connected to the access depth? If indeed there is no impact of this distribution, what is the role of this analysis in the author's aim to quantify an upper limit of warm mode melting? If there is no impact, the authors should state this clearly and state clearly why they include this quantitative assessment. If there is an impact, the authors should explicitly discuss this in the results section.

We have included the access depth analysis in our manuscript (1) to identify oceanic gateway features and (2) by this, gauge the validity of our melt results for the grounding line in each basin. PICO uses one value of temperature and salinity as input and applies the generated melt to the entirety of the region's grounding line. In all regions, we use the deepest access depths found along the grounding line to derive temperature changes in PICO.
The analysis of the quantitative distribution of access depths helps us to identify regions with "major" gateways (deepest access depth represents the biggest step in grounding line coverage). In those regions, similar to the case of Filchner Trough, the inflow of warm water masses could be channeled through this major 'oceanic gateway', i.e., where a change in circulation can lead to a full cavity switch from cold to warm conditions. As you note, whether such a gateway exists or not does not impact the melt quantification in PICO, we have added this to the text.

Specifically, we have changed/added the following:
In the methodology section l. 106ff:

> *"We define the deepest access depth found along the grounding line of each basin as d_GL,0 and express the fraction of how much the grounding line at that depth is connected to the open ocean with values ranging from 0% to 100%. If* ***a comparably large part of the grounding line is reached by only a small increase in vertical access level, an 'oceanic gateway' is present*** *i.e. a deep trough connecting the (overdeepened) ice-shelf cavity to the open ocean past the continental-shelf break. We thus interpret an oceanic gateway to be the horizontal pathway from the open ocean to the grounding line of the ice sheet along the deepest possible ocean-connection between the two. For each region, we* ***ascribe an oceanic gateway as 'major', if a global maximum (highest peak) in access depth along the grounding line is found at d_GL,0.***

In Section 3.1, l. 252f.:

> *"Note* ***that the existence of an oceanic gateway does not influence the melt rates*** *calculated with PICO in Section 3.3, as we use d_GL,0 for all regions. However,*

it allows us to gauge the validity and limitations of our assumptions in each basin."

In the discussion (l. 487ff):

> *"Please note that our results* ***are not directly dependent on the grounding line coverage*** *at the deepest access depth, but it enables us to contextualize the results.* ***Our temperature and melt rate changes would not differ if at d_GL,0 only 1% instead of a higher percentage were horizontally connected to the open ocean.*** *PICO uses one temperature (and salinity) estimate per basin to compute sub-shelf melt rates. However, the existence of a major oceanic gateway means that a substantial portion of the grounding line is reached at d_GL,0 and the PICO input values are a good representation of potential results. In the case of the Filchner--Ronne basin for instance, d_GL,0 reaches more than three quarters of the cavity and is thus, in our conclusion, adequately representative for the entire shelf e.g. for estimating a bathymetric-constrained 'warm'-mode melt estimate. In the other case, that no major oceanic gateway exists, PICO input values represent an upper bound on the oceanic properties that would reach the grounding line.*

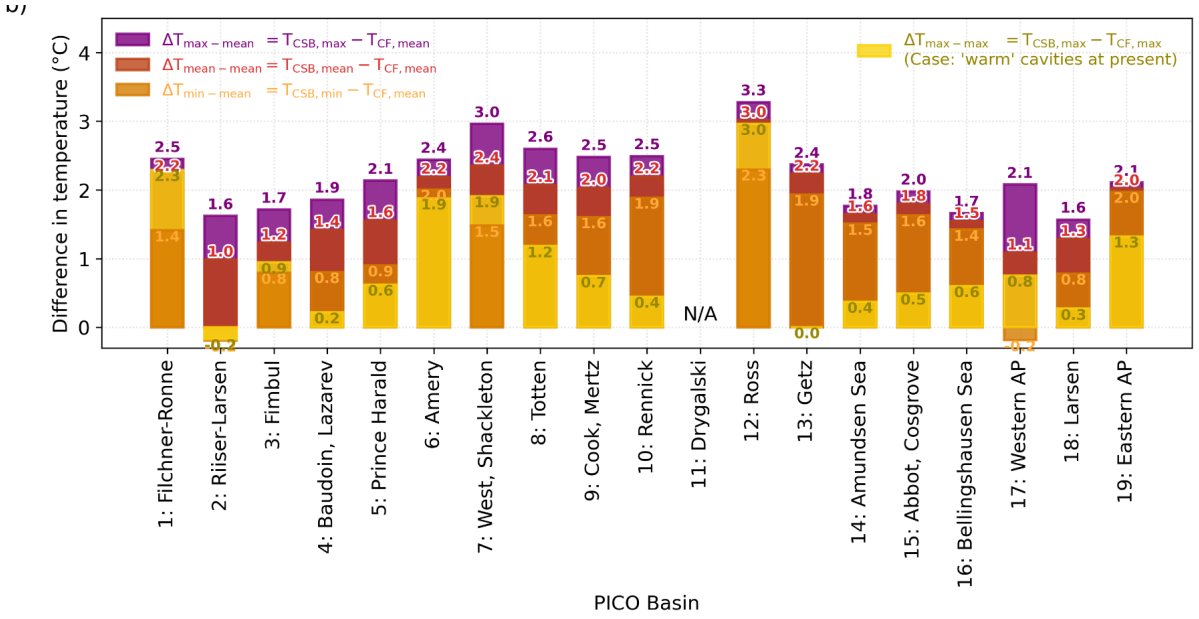
b. The discussion of the Amundsen Sea (Sec 3.3.4) is difficult to follow. The authors present Fig 10, showing present-day warm cavities, and in a single sentence in the middle of the section, point out this well-known characteristic. However, they only half address this when discussing temperatures. Clearly from Fig 10, T_CF,mean is an irrelevant metric for the Amundsen Sea. This should be stated explicitly. From there on, no more results based on this irrelevant metric should be included. Instead, the authors present their temperature changes and melt increases as if they are unaware of their own finding. If the authors do not draw insights from their own results, it is difficult for a reader to do so. In this Section, I believe the authors should start from the common knowledge (already warm cavities) and reflect on what this means for their core narrative (assess change to warm cavities). Next, after quantifying T_CF,mean, they should conclude that this temperature is irrelevant, and hence refrain to temperature changes based on T_CF,max.

We have rearranged the section so that we start, as suggested, by stating the well-known fact about the Amundsen Sea being in a warm-mode.

We follow your suggestion on T_CF,mean (see lines 368ff. in the revised manuscript) and use the difference dT_max-max in this region:

> *"The highest temperatures along the calving front are found near Pine Island Glacier with a maximum temperature of T_CF, max=1.17°C. In contrast, the mean temperature of -0.23°C is influenced by much colder bottom temperatures found at Crosson Ice Shelf (-1.2°C; cf. Supplement Fig. S7). Considering that warm waters are already present within most of the Amundsen Sea's ice-shelf cavities, T_CF,mean can* ***thus be considered an unrepresentative metric*** *for deriving bathymetric constrained 'warm'-mode melt estimates in this region. For this region, we thus use the difference of T_CF,max to T_CSB,max, ∆Tmax-max = 0.4°C, to derive an upper bound melt estimate i.e. assuming an inflow of more, unmodified, CDW to all grounding line parts of the region. See Fig. 10 for the other temperature differences, ∆Tmean-mean and ∆Tmax-mean to compare."*

We have further clarified this by adjusting former Fig 05b, now Fig 10b of the revised manuscript:



In the embedding paragraph, they also refer to a study based on a change in T_CSB, which is in contrast with the author's core narrative that T_CSB is constant; hence, this comparison is again irrelevant.

We agree that mentioning the Goméz-Valdivia study that shows an increase in ocean temperatures reaching the Amundsen Sea region of 1.2°C does not tie in fully with our approach and removed it here. Since, we find it important to consider that even in already 'warm' regions i.e. where warm-mode melting dominates, temperatures can increase in the future.

Hence, we have added a slightly adjusted phrase in our discussion part l. 562ff:

> *"When considering estimates on CDW-inflow driven sub-shelf melting, one has to consider, however, that ocean temperatures are projected to become warmer in the future, for instance, by 1.2°C as found by Gómez-Valdivia et al. (2023) that employ a global climate model on a relatively coarse resolution (1° ocean model)."*

c. I do like Fig 3, as it nicely visualises the amount of sheltering of different ice shelves through bathymetric constraints. Unfortunately, the authors do not use this figure for their interpretation of their results. Similar to Fig. 4, does this mean that Fig. 3 (except for the numbers) is irrelevant for their results? Or can they use this figure to interpret their results? The sheltering of Filchner-Ronne and Amery appears much more significant than that of Ross. Is this reflected in the results as possibly expected? Or is this not reflected against expectation? If the authors do not link their core results (Fig 6) to these previous results, the reader is tempted to conclude that there is no relation and is left wondering why these previous results are included at all, and what exactly is the core result.

We have included Fig. 3 to visualize our access depth approach as we find it more insightful than a simple 2D map of the access depths, which we have included in Supplement Figure S2. Figure 3 adds information to Figure 4, as it shows how sheltered different cavities are, as you mention. In Figure 4 we show the distribution of access depth at the different region's grounding lines together with the access depth we use for deriving temperature and melt rate estimates (d_GL,0). While the melt estimates are the core result of our study (Fig. 6), the access depth analysis and discussing of oceanic gateways has an importance too. We have amended the text in several instance to tie our results both to Fig. 3 and 4:

In Results Section 3.1 (l. 217ff):

> *"At Amery, around 91% of the ice-shelf cavity is shielded by shallower bathymetry, i.e. the access depth is shallower than the topography in 91% of the cavity area. In contrast, this applies only to about a third of the cavity area for basins 7 or 17. We later see that this can be linked to the absence of any oceanic gateway structure in those regions."*

And later in the same section lines 233 to 235:

> *"Filchner–Ronne, Ross and Amery are the regions where not only the grounding lines, but, together with Fimbul (basin 3) and Totten (basin 8), also the cavities are most shielded by shallower bathymetry (Fig. 3)."*

And lines 248ff.

> *"In PICO basins 5 (Prince Harald), 19 (Eastern AP), 9 (Cook, Mertz), 15 (Abbot, Cosgrove), 16 (Bellingshausen Sea), 7 (West, Shackleton) and 17 (Western AP), we do not find a significant spike in the access depth distribution along the grounding line and hence conclude that no (seen from an Antarctic-wide scale) oceanic gateway is present. This is despite the fact that parts of the respective cavities are shielded by shallower bathymetry (in all regions at least a third of the cavity is shielded).*

In line 465ff:

> *"Generally, our analysis highlights the generally strong sensitivity of the large, 'cold' cavities (Filchner–Ronne, Ross, Amery), which stands in contrast to regions with already warm cavities (Getz – Western AP). These 'cold' cavity-regions show 'major' oceanic gateways (Fig. 4) that* ***allow access into a well-shielded cavity*** *(especially true in the case of Filchner–Ronne and Amery, cf. Fig. 3)."*

In the conclusion (l. 589ff.):

> *"All regions would experience a strong increase in sub-shelf melting, while basal melt rates would increase up to 42-fold in cavities that are currently in a 'cold' state, are* ***well-shielded by shallower bathymetry and have a 'major' oceanic gateway*** *that could channel warmer water masses to the grounding lines."*

d. Similar to the previous point, the discussion of the different regions (Sec 3.3) is largely isolated. Each region gets a description of the main gateways, a quantification of temperatures and melt rates, and a comparison to previous literature. All further interpretation is left to the reader. Fig 6c is a great tool to link the different regions, highlight the general strong sensitivity of the large, cold cavities (FRIS, Ross, Amery) and contrast this against the regions with already warm cavities (Getz – Western AP). In Sec 3.3, I would expect the authors to place the results of the different regions in the larger context and hence refer to Fig 6c (intercomparison) and ideally also Fig 3 and 4 (interpretation).

We have amended the paragraph with former Fig. 6c that now reads (l.457ff):

> *"We find the largest increase at Amery Ice Shelf, where melt rates could increase up to 42-fold, cf. Fig. 11c. With our access depth analysis in these three regions, we have found that, at the access depth used for extracting the temperatures at the CSB, d_GL,0, more than 30% of the respective grounding lines are accessed. This is why we classified these regions to have 'major' oceanic gateways see Fig. 4. This gives our results significance, as in those regions, our assumptions with PICO are*

particularly valid e.g. warm water from the CSB is channelled to the respective grounding lines."

It further reads:

"Ice-shelf regions in West Antarctica do feature oceanic gateways as well (e.g. Getz with 9.8% of its grounding line reached at d_GL,0) but since there is already warm water present at the CF, there is little potential for as drastic changes in the sub-shelf melt rates through bathymetric constrained inflow as in 'cold' cavities at present. Generally, our analysis highlights the general strong sensitivity of the large, 'cold' cavities (Filchner–Ronne, Ross, Amery), which stands in contrast to regions with already warm cavities (Getz – Western AP). These 'cold' cavity-regions show 'major' oceanic gateways (Fig. 4) that allow access into a well-shielded cavity and to the grounding line (the former is especially true in the case of Filchner–Ronne and Amery, cf. Fig. 3). "

2. Presentation of aim/motivation/methods.

The methodology is somewhat spread out into other sections. Also, the authors have done well in more clearly stating their motivation in, e.g., the title, but have not yet clearly linked their methodological choices to this motivation.

a. Lines 64-70 in the introduction read like part of the methodology (definition of ocean gateways). Consider rewriting of reorganising to clearly separate intro from methods.

We have restructured and rewritten the methods as suggested.

b. Lines 83-86 also appear in the discussion. More critically, it is unclear to me how this paragraph adds to the reader's understanding of what your study is about.

Here we wanted to refer to our follow-up study Kreuzer et al 2024 that uses our oceanic gateway concept. We see that it is not appropriate to mention it here and hence deleted it from the introduction.

c. Rather than this paragraph, I would expect the introduction to end with a concise problem statement and motivation/aim: what can the reader expect to find in the remainder of this study? Please spend a few lines on this in the place where it's expected.

Done. The revised passage in the text now reads (l. 79ff.): "In this study, we aim to estimate the impact of potential future warm water inflow on basal melting for all Antarctic regions. In order to do so, we (1) analyse the bathymetry and identify trough-like features that potentially provide access of off-shore warm waters into ice-shelf cavities, (2) calculate the increase in thermal forcing resulting from such a regime shift and (3) compute the respective increase in sub-shelf melting."

d. Also the buildup of the methodology is not directly logical because the motivation/aim has not been clearly formulated. It takes until the end of Sec 2.2 until it's clear that delta T is a main metric in this study. I strongly recommend that the authors write 1 paragraph at the start of the methodology linking their aforementioned motivation to the general methodology. Something along the lines of: *'We aim to quantify an upper bound of melting if cavities switch to a warm mode. To this extent, we will use PICO to compute melting from ocean temperatures. As a present-day estimate, we'll take T at the calving front. For the warm mode, we will take T at the shelf break. In order to account for bathymetric constraints, we*

will define gateways and derive the warm mode T at the access depth. In this section, we will first define ocean gateways and access depths (2.1), etc etc'. This way, the reader knows what's really happening and why. And the authors have a framework to easily structure the methodology.

Done. The beginning of the methodology section now reads (l . 86) :
"The goal of our approach is to quantify an upper bound to melting if cavities switch to a 'warm' mode. To this extent, we use PISM-PICO to compute ice-shelf basal melting for given ocean temperatures and salinities: As a present-day estimate, we take temperatures at the calving front. For the 'warm' mode, we use temperatures at mid-depth at the continental-shelf break (CSB). In order to constrain that latter depth and to estimate the potential impacts of this selection on the melting at the grounding line (accounting for bathymetric constraints), we define oceanic gateways based on access depths found in each region."

e. Lines 182 – 197 are a methodological description and should move to the Methods section.

Done.

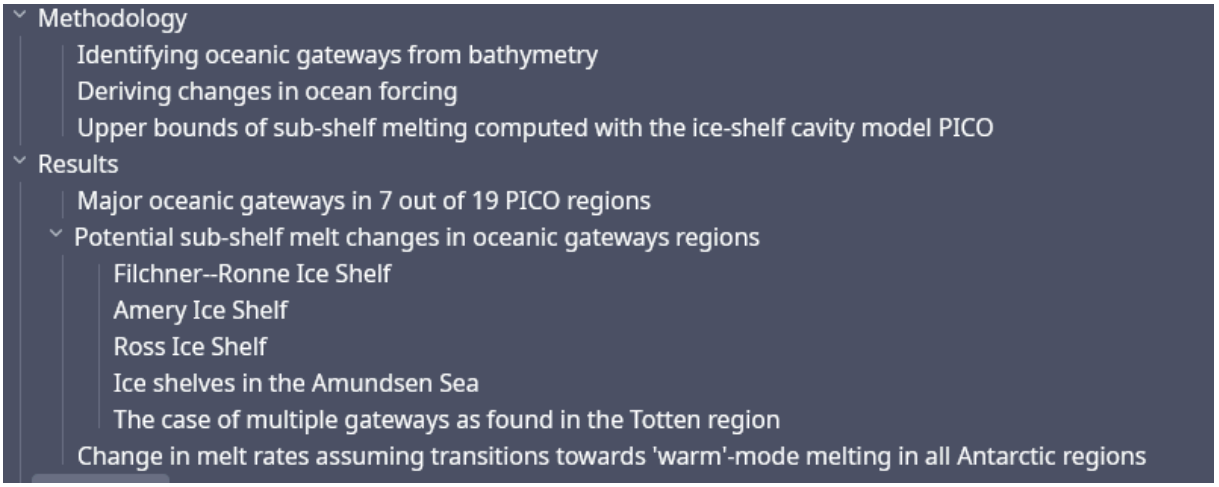
3. Presentation/organisation of results.

The order of the results isn't fully logical in comparison to the general narrative. In the methods, the order is: bathymetry -> temperatures -> melt rates. This is in line with the stated motivation as well and is reflected in each of the subsections 3.3.x. But not in the overall results section, creating confusion about what the authors consider their main results. a. I recommend trying to keep this logical order in the results section as a guideline as well. In principle, 3.1 is good as a general overview. After this, I'd put the subsections per region (now 3.3.x) keeping the current order. After that, I'd put a general section building on the current 3.2 which puts together the overall results in terms of the core objective: estimating warm-mode melt rates.

Thank you for this suggestion to restructure our text. We have reordered the sections accordingly and hopefully now provide a more coherent presentation of our analysis.

b. Also make sure your section titles properly reflect the contents. Right now, the overall majority of the results fall in 3.3 with the header 'ocean gateways'. However, in the title and overall narrative, these gateways are the constraint used to determine melt rates. Hence, the major results should be put in a section with a title containing 'melt'.

Thank you. We have changed the section titles in the results section to:



c. In the melt section, which I propose as 3.3, following the region-specific results, the bigger interpretation and intercomparison should take place such that your study forms one large narrative. In this section, you can address the points raised in 'major comment 1'.
Addressed. Please refer to the section in the revised manuscript.

4. 'oceanic gateways in 7 out of 19 regions'.

This quantitative result is based on Fig 4 comes back in several places including conclusions and abstract. So the authors deem this a major result. However, the criterium is unclear, and its relevance to the core motivation/aim is also not discussed.

a. First, the description is quasi-quantitative (l. 107: a 'large fraction'). And it's unclear why these 7 regions are qualitatively different from the other 12. In Fig 4, Larsen looks very similar. How can the reader deduct from your results that Larsen does not contain an oceanic gateway?
Thank you for making us aware of this confusing/incoherent aspect. We have revised the respective section in the manuscript including Figure 04, to clarify our analysis approach. When the largest spike in the access depth distribution along the grounding line occurs at d_GL,0 we call it a "major" gateway. Larsen does show an oceanic gateway - a major oceanic gateway is found that reaches around 11% of the region's grounding line at the deepest access depth d_GL,0. To avoid further confusion, we have revised the definition and included the respective explanation in the methodology section:

l. (108ff): "If a comparably large part of the grounding line is reached by only a small increase in vertical access level, an 'oceanic gateway' is present i.e. a deep trough connecting the (overdeepened) ice-shelf cavity to the open ocean past the continental-shelf break. We thus interpret an oceanic gateway to be the horizontal pathway from the open ocean to the grounding line of the ice sheet along the deepest possible ocean-connection between the two. For each region, we ascribe an oceanic gateway as 'major', if a global maximum (highest peak) in access depth along the grounding line is found at d_GL,0."

Figure 04 now includes additional labels indicating the found gateways and the regions (sub-panels of Fig.04) are sorted according to the accessed percentage of total grounding line i.e. the "majority" of the gateway so-to-speak for the region at d_GL,0, the depth we use later to derive melt changes.

7 out of 19 regions thus have a major gateway at d_GL,0 and we state this result throughout the text. In the abstract we revised it to "Here we identify potential oceanic gateways in at least 7 out of 19 regions subdividing the Antarctic continent" to acknowledge those regions that incorporate more than one ice shelf with a distinct peak (but not at d_GL,0) in the access depth distribution.

b. More generally: if the focus on this quantitative number is so prominent, it should **have a clear and unambiguous definition.** Either visually from Fig 4, or quantitatively from your data, a reader should be able to reproduce this number based on the information you've provided. Currently, this is not the case.
See above. We have revised the definition of a (major) 'oceanic gateway'. 7 out of 19 PICO regions have the highest peak in the access depth distribution at their deepest grounding line

access depth, d_GL,0.

c. More generally, the authors have now stated their motivation/aim more clearly: estimating an upper bound on melt rates. There is **no discussion**, however, how these melt rates are affected by whether or not a region has such a gateway. For example, the 7 regions do not stand out as a cluster in Fig. 6c. Does this mean the gateways are unimportant for the melt rate changes?

Thank you for pointing this out to us and we agree that we have not made ourselves clear enough. We have now more clearly stated that when a 'major' oceanic gateway is present, our results with PICO are more representative for the entire basin. In other regions our results represent an upper bound estimate, because only a very small amount of the grounding line is accessed at the deep access depth we use for deriving melt changes, but we assume that it is valid for the entire region. Using the temperature estimates from that depth (i.e. where warm CDW resides off the continental shelf), thus creates a very large melt increase for the entire region.

To clarify our findings further, in the conclusion section we have added in lines 550ff:
" Using the 19 PICO basins in our study, we conclude for basins 1, 6, 12, 14, 11, 18 and 13 (7 out of 19 regions; cf. Fig.4), that d_GL,0 is representative for the entire basin in the case of an oceanic-gateway driven switch to 'warm'-mode melt conditions, as d_GL,0 represents the largest grounding line share. In other regions (e.g. in basins 3, 2, 10), however, d_GL,0 and subsequent temperature offsets and melt rates based on that estimate are less representative for the entire basin but constitute even more an upper-bound estimate as d_GL,0 is lower, hence represents warmer CDW. This could be fixed by simulating each individual ice shelf separately. Finer resolutions i.e. on the individual ice shelf level would reveal more individual gateways but this analysis is out of scope of this manuscript".

Minor comments

l. 37: 'The highest thinning rates'
Corrected.

l. 44: Sign should be greater than (>)
We have cut this from the revised manuscript.

l. 76: 'provides'
Thank you. We have deleted this part as we have rewritten this section for the revised manuscript.

l. 186: 'previous studies'. Which?
Added Reese et al 2018, Reese et al 2023, Sutter et al. 2023 and Wirths et al. 2024.

The revised section of the manuscript (l. 195ff) now reads:

> *"The boundaries on land are based on ice drainage basins from Zwally et al. (2012), were consolidated to 19 regions in Reese et al. (2018a) and for the use for PICO mainly extended along meridians into the ocean. In previous studies (Reese et al., 2018a, 2023; Sutter et al., 2023; Wirths et al., 2024), those basin boundaries in the ocean were used to extract a basin average for temperature and salinity (i.e. average over the region) to feed into the box model."*

l. 213 ‘are in the mean much lower’. Odd word order, rephrase.
Changed to: “ Average temperatures along the ice-shelf fronts, T_CF, mean, which are derived at the ocean floor in the individual basins, are lower than temperatures found at the relevant access depth at the continental-shelf break, T_CSB, mean, see Figure 10.”

l. 217 ‘in the mean’. Odd word order, rephrase.
Changed to: “Mean T_CF estimates range” …

l. 258 ‘we are further analysing’ -> ‘we further analyse’
Corrected.

l. 283 ‘near Getz’ -> ‘at Getz’
Corrected.

l. 310 ‘If’ -> ‘Whether’
Corrected.

l. 352 Is the uncertainty in bathymetry in this well-sampled region significant in comparison to the uncertainty due to methodological choices made in this study? So is this the dominant reason why these results should be taken with caution?
Thank you for this comment. We agree that our methodology only provides an overall approximation to gauge warm-mode melt estimates around Antarctica. We believe, however, that reviewing our methodology i.e. providing a critical view on the connected-component analysis is important as well. We have tested whether the different routing of waters (according to our connected-component analysis) yields a difference: the access depth at the Amundsen Sea would only be around 25m shallower. For clarity, we have cut this part of the revised manuscript.

l. 374 The statement that Amundsen is already warm is very important for this whole section, but this sentence is dropped between paragraphs and then ignored. Make sure to write this section consistently around this well-known fact.
We have placed this statement now in the beginning of the subsection covering the ice shelves in the Amundsen Sea. Section 3.2.4 now begins with: “At present, ice shelves in the Amundsen Sea have ’warm’ cavities and therefore dominate the current mass loss in Antarctica (see e.g. Pritchard et al., 2012), indicating that this region is already out of balance with the current oceanic forcing. Here, comparably warm water masses have already found their way underneath the ice shelves, in contrast to the three ice-shelf regions detailed before.”

l. 446-458. I don’t think this extensive discussion on bathymetric uncertainty is very important. Again: ask yourself: is this a dominant source of uncertainty in your results, compared to uncertainties related to methodological choices? And would that be resolved by a bathymetric dataset at 100x100m resolution? I don’t think so. I suggest removing this paragraph and focusing on the dominant/significant stuff
Thank you for the suggestion. We have removed the paragraph.

l. 465 ‘It is to note’: odd sentence, rephrase
Changed to: “It should be noted that…”

l. 470: [Quote from manuscript: “With a spatially more resolved approach, one could apply the extracted temperature forcing to only those parts of the grounding lines that are connected to the open ocean at the deepest access depth found at the ice-shelf region's grounding line.”]
‘spatially more resolved’: very generic. What do this mean? Is this a limitation in the resolution you used for PICO, or a limitation of PICO itself?
Rephrased so that lines 521-523 now read: “With a spatially more explicit approach, with which one could provide temperature (and melt) locally to each grid cell, one could apply the extracted temperature offset to only those parts of the grounding lines that are connected to the open ocean at the deepest access depth found at the region's grounding line.”

l. 473 ‘less resolved’ and ‘more distributed’. Again generic wording, be specific. Note: this is not to criticise PICO or your choice to use PICO, this is valid. But be specific so that readers understand what you mean.
Rephrased to “Furthermore, the melt pattern in PICO is spatially less variable than in ocean circulation models or observations, which means that PICO does not reach the very high melting at the order of 100 m yr−1 reported close to grounding lines (Dutrieux et al., 2014; Paolo et al., 2015).”, see lines 524-527.

l. 476 Can cite Berends et al (2023) here as well: 10.1017/jog.2023.33
Done.

l. 481 ‘capture to some extent’. What does this mean? Again, be specific
Sorry for the confusion. We have changed it to: “to capture the parameter uncertainty”, because later in the sentence we already acknowledge that a full ensemble is needed to explore the uncertainty in full.

l. 490 The regions in van der Linden et al are taken from Levermann 2020. Rather cite the original: https://doi.org/10.5194/esd-11-35-2020
Changed to: ”[...] as for example in van der Linden et al. (2023), following Levermann et al. (2020), [...]” in the revised manuscript (l. 543).

l. 497 As stated before: your Amundsen Sea results clearly show that, at least in this case, the mean conditions are invalid. So yes, these give higher differences, but is that what you’re looking for? A more critical discussion would state that, simply, your method is **not designed to assess the switch from a warm to a warm cavity**, and that the sensitivity in these regions is not dominated by a qualitative circulation change but by more gradual offshore changes that would affect T_CSB (which is beyond the scope of your study).
Together with addressing your comment from 1b) we have changed this part in the discussion that it now reads (cf. l. 557ff):
*“When it comes to the effects of the potential warm water inflow, as analysed in our study, the difference in temperatures is small in some regions for physical reasons: this can be the case if the access depth of the basin is shallow and encompasses slightly colder water masses at the CSB, i.e. representing surface waters not CDW, or if the calving front temperatures are already relatively warm, as in the case of the Amundsen region. In those regions, changes in melting may be more sensitive to gradual offshore changes in continental-shelf break temperatures instead of a qualitative circulation change, i.e. a regime shift of cavity inflow leading to a switch from a ’cold’ to a ’warm’ cavity, **which our method is designed to assess**. When considering estimates on CDW-inflow driven sub-shelf melting,*

one has to consider, however, that ocean temperatures are projected to become warmer in the future, for instance, by 1.2°C as found by Gómez-Valdivia et al. (2023) that employ a global climate model on a relatively coarse resolution (1° ocean model).”

l. 514 ‘combine the latest’
Due to newer publications, e.g. Charrassin et al., 2025 (https://www.nature.com/articles/s41598-024-81599-1), we decided to cut “latest”. It now reads: “We combine available bathymetry data [...]”

l. 521 ‘two orders of magnitude’ would mean at least a 100-fold increase. This is not what you’ve found.
Thank you for spotting this typo. We have corrected it to “would increase up to 42-fold”.

l. 520 ‘parameterisation’ -> ‘parameterisations’
Corrected.