

Second review of:

Bathymetry-constrained warm-mode melt estimates derived from analysing Oceanic Gateways in Antarctica

Lena Nicola et al.

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Assessment: Minor revision

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.
 - 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 $T_{CF,max}$. 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.

- 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.
 - 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).
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.
 - 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.
 - 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.

- 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 ΔT 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.
 - e. Lines 182 – 197 are a methodological description and should move to the Methods section.
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.
 - 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'.
 - 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'.
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 deduce from your results that Larsen does not contain an oceanic gateway?
 - 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.

- 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?

Minor comments

l. 37: 'The highest thinning rates'

l. 44: Sign should be greater than (>)

l. 76: 'provides'

l. 186: 'previous studies'. Which?

l. 213 'are in the mean much lower'. Odd word order, rephrase.

l. 217 'in the mean'. Odd word order, rephrase.

l. 258 'we are further analysing' -> 'we further analyse'

l. 283 'near Getz' -> 'at Getz'

l. 310 'If' -> 'Whether'

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?

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.

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

l. 465 'It is to note': odd sentence, rephrase

l. 470 'spatially more resolved': very generic. What does this mean? Is this a limitation in the resolution you used for PICO, or a limitation of PICO itself?

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.

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

l. 481 'capture to some extent'. What does this mean? Again, be specific

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>

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).

l. 514 'combine the latest'

l. 521 'two orders of magnitude' would mean at least a 100-fold increase. This is not what you've found.

l. 520 'parameterisation' -> 'parameterisations'