Review of:

Oceanic gateways to Antarctic grounding lines – Impact of critical access depths on sub-shelf melt

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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.

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

    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.
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.
    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.
    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.
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). 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.
    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.

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?
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?
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.

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).
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.
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.
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.
l. 52. Provide a reference for these statements. A highly biased suggestion from my side would be Lambert et al. (2023).
l. 55. Is this related to near-grounding line melt? If so, mention explicitly, if not, remove this statement.
l. 80. (and other places). I don't think 'diagnose' is the correct verb. Replace it with 'parameterise' or something equivalent.

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.
l. 84. I don't understand this sentence 'The grounding lines…'. Please revise.
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.
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.
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.
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.
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.
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.
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).
l. 172. 'highest grounding line depths' -> 'shallowest/deepest grounding line depths'
l. 218. Compare 4.65 m/yr to numerical modeling studies.
l. 233. Is this other pathway deeper? How is it (or can it be) relevant to your study?
l. 318. These kinds of sentences are fine, but compare to other studies where possible.
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?
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?
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.
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.

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