The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-32

Anonymous Referee #1

Referee comment on "An assessment of basal melt parameterisations for Antarctic ice shelves" by Clara Burgard et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-32-RC1, 2022

Review of An assessment of basal melt parameterisations for Antarctic ice shelves by Burgard et al.

The manuscript by Burgard et al carries out a comprehensive analysis combining (I think) all all of the leading parameterisations of ice-shelf melt that are currently used, with a set of global ocean model simulations, in which the parameterisations are rigorously analysed in terms of their ability to replicate ocean modelled melt rates given open-ocean properties, their stability in terms of optimal parameters, and their benefits and drawbacks in terms of use.

I think this is a great study to have been carried out, as to date no other studies have collected all of the extant parameterisations together in one study, implemented them in a common framework, and tuned and evaluated them with identical data. The study also does not ignore the importance of spatial patterns of melt arising from the parameterisation, which are too often overlooked. Though it is a very long paper, it is formulaic and there is a progression in terms of the experimental setup and analysis, making it a less daunting read. The length is also owed to its comprehensive discussion of existing parameterisations, and any modifications made to them as part of this study, and it is really good to have all of this material together in one place. I think this is a worthwhile and interesting study, as it is will be important to determine how the Antarctic ice sheet will evolve in response to oceanic change, and it is clear that ocean models which can resolve under-ice shelf circulation are the rate-limiting step in such investigations. Therefore the lessons learned from this study are valuable and I recommend for publication after some minor revision. I have comments below that I hope might help in this regard.

(On a side note, the python library developed for this study will be of value as well, although its value may depend on how easily it can be implemented with C++ and Fortran ice-sheet models. However this is not directly relevant to the merits of the manuscript.)

I have only two general comments, and it is with regard to the global simulations used to force and tune the parameterisations and the assessment of parameterisation skill:

- With a resolution for the ensemble of 8 km at 70 S, this is quite coarse for a simulation that is meant to provide "truth" for ice-shelf melt in response to conditions on the shelf and the ACC. It is true the conditions on-shelf are also not necessarily realistic, but it is the continental-shelf-to-melt dependence that is important here. For instance 8km is well above the deformation scale, so I question the model's ability to represent boundary currents that bring warm water into cavities and melt-laden water out, and transport around bathymetric obstacles and through bathymetric depressions, and these could potentially impact total melt, rather than just melt patterns. I think this potential caveat, as well as those mentioned in the discussion, should be more clearly stated up front in section 2.1.

- I may have misunderstood but given the volume of data/NEMO output I found it a missed opportunity that the authors did not test any tuned models with data that was not used for tuning. There are 127 years of ocean conditions and corresponding melt; I would think it would be possible to tune with only a subset and then evaluate performance on the rest. Eq 32 and its explanation suggests this was not done. Maybe the authors could comment on this in the manuscript or, if they feel it is worth doing, carry out additional experiments.

Specific comments:

Line 28: I don’t think it is fair to say this, as the response of ice-sheet models to melt is an enormous source of uncertainty. This is really shown in the initMIP-Antarctica experiments (Seroussi et al, 2019; Fig 4c) where loss of grounded ice over 100 years in different models with melt anomaly treated in the same way across models varies by 400 mm. The papers you cite do not present any results that I can see where the melt treatment was controlled for and inter-model variance in response to melt can really be examined, so I don’t think any of these results in these papers really isolate this uncertainty… but initMIP does, so we know it is there.

Line 78: Im not sure what you mean by “physically sound in time and space”. I think you might be saying that by using a model you can perfectly match ocean conditions outside the shelf with melt rates, which you could not do with actual data.

Line 153: when I saw this, I assumed you were comparing spatial patterns of melt so was confused by eq (32). Maybe be clear for what purpose you interpolate/reproject outputs.

Figure 1, legend: HIGHGETZ, not WARMGETZ?

Line 157: 5 Delta x = 40km? not sure I follow.

Line 203: “a lot” ï “widely”

Eq 22 and others: I don’t think you say why some terms are bold.

Eq 24: for those who are not already very familiar with Lazeroms’ method, it might seem strange how you can relate a height difference to length of a plume path without actually integrating the plume equations. Can you give some intuition regarding this definition?

Line 399: Favier et al 2019 is carried out in the ISOMIP+ domain, correct? Should it be surprising that the parameters are not appropriate? Similarly, does the PIGL situation not assume that all ice shelves are flooded with CDW? Should it be any surprise these give high RMSE?

Line 425: 5x smaller isn’t an order of magnitude

Line 425: 3rd column ï 2nd column

Line 458: just wanted to point out I like this comparison.

Line 471: I would add Reese 2018 to this list.

Line 503: looks like an error within the brackets about Jacobs 1992. Also this is a really good point to bring up – and there is more recent work done regarding mode-3 melt (Silvano et al, 2016) which would be good to bring up here and in the discussion.

Line 510: can you elaborate more on your reason for using average over integrated, please. What is the risk of not doing so.

Lines 544-553: can you explain your experiment more clearly please. I do not understand what you have done here. Is this is new tuning, based on new melt and ocean conditions, or other?

Line 560: why would a sigma coordinate model fare better? Sigma coordinate models have singularities and wild errors where the column goes to zero and the surface gradient is high, i.e. near the grounding line.

Line 562-566: I think you are being too hard on yourself. Given the aims of the study, im not sure why you would need to consider evolving cavity geometry.

Line 567-571. These are really good points. You might add a discussion on why Mode 3 melt is important.

4.1.2 and 4.1.3. These are really interesting experiments but I do not understand the initial procedure at all. If I understand correctly, you are attempting to see how your parameter results would vary if you fit with a subsample of your data, or tweak your data somehow (so, perhaps this addresses my #2 general comment?) but I don't understand lines 573-576. What is meant by bootstrapping? What is the nature of each sample – because I read that each sample represents melt of each shelf in each of the 127 years.. so not a subsample. "What is meant by 36 random sub-samples, with replacement"? It is impossible to interpret the rest of the sections without knowing this.

Line 620-622: I do not follow. The way I interpret Fig 9 is that it is essentially impossible to infer the correct parameter "pair" because they so strongly covary, that depending on the specifics of the tuning data you can get e.g a low C and high gamma_T*, or vice versa, with either fitting the data reasonably well. But in e.g. a future projection with an ice-sheet model, the difference between using one or the other parameter pair could be quite large. So im not sure simply fixing one of these parameters addresses this difficulty.

Lines 700-702. I would think this of CMIP models too. Ill not attempt a list here but there is quite a lot of literature on how global ocean models have difficulty with shelf-offshore exchange.

Appendix:

Line 792: you talk about disagreement in melt with Rignot 2013 here but do not show any images.

Figure B1: add a legend

References

Seroussi, H., Nowicki, S., and others: initMIP-Antarctica: an ice sheet model initialization experiment of ISMIP6, The Cryosphere, 13, 1441–1471, https://doi.org/10.5194/tc-13-1441-2019, 2019.

Reese, R., Gudmundsson, G.H., Levermann, A. et al. 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

Silvano, A., S.R. Rintoul, and L. Herraiz-Borreguero. 2016. Ocean-ice shelf interaction in East Antarctica. Oceanography 29(4):130–143, https://doi.org/10.5670/oceanog.2016.105.