Author Response to Reviewer #1

An assessment of basal melt parameterisations for Antarctic ice shelves

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RC: Reviewer Comment, AR: Author Response, *changed manuscript text*

RC: Reviewer Summary:
The manuscript by Burgard et al carries out a comprehensive analysis combining (I think) 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 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.

AR: Thank you very much for the positive feedback and for your constructive comments on how to further improve our manuscript. We plan to address all your comments as described in the following.

RC: Side note: (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.)

AR: We agree that our package is not designed for direct implementation into an ice-sheet model code-wise. Nonetheless, it could be used as an offline python interface between a coarse ocean model and an ice-sheet model (both of them possibly written in Fortran or C++).

RC: ***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:***

RC: 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 8 km 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.

AR: We agree that a resolution of 8 km is still not perfect to reproduce the ocean circulation on the continental shelf. Nevertheless, it is a clear improvement compared to the data available typically used to force ISMIP6-type ice-sheet models, which comes from climate and ocean models that represent the topographic dynamical features even more crudely (typically at a resolution of 1°). In our study, we focus on the link between the domain in front of the ice shelf and the melt inside the cavity. Therefore, very simply said, if the "wrong" water is in front of the cavity compared to reality, it will lead to a "wrong" melt compared to reality but the physical link between the two, which we are interested in, will be consistent.
In regard to the ocean circulation inside the cavity, we agree that we do not resolve all bathymetric ridges, basal channels and eddies that potentially affect the melt locally but also on the integrated level. Also, the resolution is not as high as ice-sheet models directly next to the grounding line. We will add these points to the list of caveats intoduced by NEMO listed in Sec. 4.1.1. We will also move Sec. 4.1.1. to the end of Sec. 2.1. so that the reader has the limitations in mind when interpreting the results.

RC: 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.

AR: We agree that this approach would be statistically more robust. For the revision, we will conduct a cross-validation of the parameterisation. This means that we will conduct the tuning and evaluation several times on different periods to robustly estimate the generalisation performance of each parameterisation. To avoid autocorrelation influencing this cross-validation, we will divide the data into 10-year chunks, as the autocorrelation is typically 2 to 3 years in our input temperatures.

RC: ***Specific comments***

RC: 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.

AR: We agree that this might have been formulated too strongly. We will reformulate, saying that it is "one of the main sources of uncertainty".

RC: Line 78: I'm 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.

AR: Yes, this is what we meant. We will reformulate to clarify.

RC: 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.

AR: Thank you for pointing out that this is a source of confusion. The regridding is done for all NEMO variables at the very beginning. All pre-processing and analysis uses the regridded data. This is because it is the preferred format for ice-sheet modellers and makes computing spatial metrics easier. The integrated melt is computed based on the regridded melt patterns. We will reformulate to avoid confusion.

RC: Figure 1, legend: HIGHGETZ, not WARMGETZ?

AR: Thank you for highlighting this mistake. We will correct accordingly.

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

AR: 40 km x 40 km is 1600 km^2 and not 2500 km^2, thank you for pointing out this mistake. The effective resolution of physical ocean models is typically 5 to 10 times the grid spacing (Bricaud et al., 2020), and this is why we took a slightly higher factor than 5 to multiply with Δx but forgot to correct this in the manuscript. We will adjust the effective resolution criteria accordingly to correct this mistake.

RC: Line 203: "a lot" => "widely"

AR: Will be changed.

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

AR: Thank you for pointing out that this was not made clear in the text. The bold terms highlight the differences in the terms between the plume formulation as given in Lazeroms et al. (2019) and the new formulation of the plume suggested in this manuscript. We will clarify this further in the manuscript.

RC: 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?

AR: The plume formulation assumes that the ice shelf base rises from the grounding line up to any point of the ice-shelf base with a constant slope, which brings the formulation to a single dimension along which the plume properties are integrated. This is why height difference and length of the plume path are directly related.

RC: 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?

AR: The aim of all these basal melt parameterisations is to avoid the use of a computation-heavy cavity-resolving ocean model by representing a simplified version of the ocean physics in the cavity beneath the ice shelf. This means, in principle, that these parameterisations should have the same parameters independently of the cavity geometry (ISOMIP+

or realistic) and of the input temperature and salinity (e.g. warm or cold conditions). If one assumes that the parameters should be retuned for each specific situation, we would argue that the aim of the parameterisation is not reached. We nonetheless agree that this is not genuinely surprising.
Still, we did not expect that our results would diverge so largely from the results from Favier et al. (2019). In regard to the "PIGL" parameter, as we point out in the manuscript, the high RMSE is less of a surprise because the parameter was designed to be used with temperature corrections, which we did not apply. Besides, we would like to point out that the PIGL formulation does not assume that ice shelves are flooded with CDW, it assumes that the melt-temperature quadratic relationship is constrained by the highest melt rates found near Pine Island's grounding line.

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

AR: Thank you for pointing this out. We will reformulate.

RC: Line 425: 3rd column => 2nd column

AR: We will correct this mistake.

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

AR: Thank you! :)

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

AR: Will be added.

RC: 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.

AR: Thank you for bringing this up, we will look into it and reformulate accordingly.

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

AR: There is no "risk" per se, it is just a choice to vary our evaluation metrics and evaluate different aspects of the parameterisations. In the first part, we evaluate the integrated melt and this way focus on an ice-shelf-wide metric, which implicitly contains information about the size of the ice shelf, and its variability with time. By evaluating the average melt (over time and space) near the grounding line, we evaluate if, on average, the right melt rate is occurring near the grounding line, independently of the size of the ice shelf. We will reformulate to explain our choices more clearly.

RC: 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?

AR: We apologise that this was not clear enough. We used the tuned parameters but applied them to observational estimates (opposed to simulations in the previous sections) of one ice shelf. So there was no new tuning here. The input temperatures are from Dutrieux et al. (2014), the topography from BedMachine v2 Morlighem (2020); Morlighem et al. (2020). We then compare the resulting melt rates with observational melt rate estimates from Shean et al. (2019) to evaluate the effect of the geometry resolution on the resulting melt rates. We will reformulate to clarify.

RC: 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.

AR: We do not argue that a sigma-coordinate model would fare better, we just think that it might lead to different results. We are aware that they do not work well when there are high slopes but they tend to better resolve the ice-ocean interface in regions with non-zero columns and low gradients.

RC: 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.

AR: We thank you for this encouragement. However, we would like to keep this limitation in the manuscript. As you pointed out earlier, the parameters unfortunately seem to depend on the cavity geometry. With further climate change and increased sub-shelf melt and retreat, the ice shelf geometry and the cavity itself are prone to change. Taking this evolution into account during tuning or at least during evaluation would have been interesting for future projections.

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

AR: Thank you for pointing that out. We will reformulate, following your earlier comment.

RC: 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.

AR: The procedure is based on the standard statistical method of bootstrapping, which aims to estimate parameter uncertainty. As explained for example in Wilks (2006), bootstrapping relies on resampling different samples with the *same* size. This is why it works "with replacement". In our case, this means that the sample always has the size of 36 ice shelves over 127 years. However, the ice shelves are drawn and then replaced into the selection pool before drawing a new one. This means that, in each sample of 36 ice shelves, different ice shelves are present. For example, in one sample, we could draw Pine Island ice shelf 4 times and not draw Thwaites, Totten, Ross, and Fimbul. In another sample, we could draw twice Totten and twice Ross, but only one time Fimbul and never Pine Island. We will reformulate in the manuscript to further clarify the method for readers who are not familiar with bootstrapping.

RC: 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$_T^\star$, 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.

AR: The distributions shown in Fig 9 are the results of the bootstrapping method, which aims to give an uncertainty estimate of the parameter(s). The resulting distributions are aimed at scientists who want to sample the uncertainty in basal melt rates introduced by the parameters. Instead of varying both $\gamma_T^\star$ and C, they can now only vary one of them and, using the inferred relationships, simultaneously cover the uncertainty in both parameters. This is one of the main conclusions of this subsection. We will reformulate the goal and conclusion of this subsection to make sure this is clear.

RC: 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.

AR: Thank you for pointing this out. We will reformulate to mention that some CMIP models struggle with this exchange too.

RC: Appendix:

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

AR: We will include this additional figure showing a comparison between the basal melt rate patterns of our REALISTIC run and the patterns from Rignot et al. (2013).

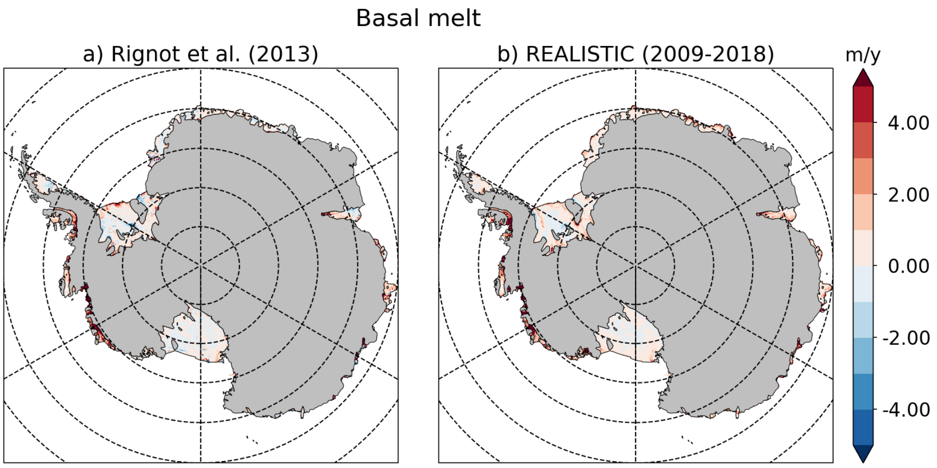

Figure 1: Mean basal melt rates from (a) Rignot et al. (2013) (2003-2008) and (b) our REALISTIC run (2009-2018) in m ice per year.

RC: Figure B1: add a legend

AR: The legend will be clarified.

References

Bricaud, C., Le Sommer, J., Madec, G., Calone, C., Deshayes, J., Ethe, C., Chanut, J., and Levy, M.: Multi-grid algorithm for passive tracer transport in the NEMO ocean circulation model: a case study with the NEMO OGCM (version 3.6), Geoscientific Model Development, 13, 5465–5483, #10.5194/tc-2022-3210.5194/gmd-13-5465-2020, 2020.

Dutrieux, P., De Rydt, J., Jenkins, A., Holland, P., Ha, H., Lee, S., Steig, E., Ding, Q., Abrahamsen, E., and Schröder, M.: Strong Sensitivity of Pine Island Ice-Shelf Melting to Climatic Variability, Science, 343, 174–178, #10.5194/tc-2022-3210.1126/science.1244341, 2014.

Favier, L., Jourdain, N., Jenkins, A., Merino, N., Durand, G., Gagliardini, O., Gillet-Chaulet, F., and Mathiot, P.: Assessment of sub-shelf melting parameterisations using the ocean–ice-sheet coupled model NEMO(v3.6)–Elmer/Ice(v8.3), Geoscientific Model Development, 12, 2255–2283, #10.5194/tc-2022-3210.5194/gmd-12-2255-2019, 2019.

Lazeroms, W., Jenkins, A., Rienstra, S., and van de Wal, R.: An Analytical Derivation of Ice-Shelf Basal Melt Based on the Dynamics of Meltwater Plumes, Journal of Physical Oceanography, 49, 917–939, #10.5194/tc-2022-3210.1175/JPO-D-18-0131.1, 2019.

Morlighem, M.: MEaSUREs BedMachine Antarctica, Version 2., #10.5194/tc-2022-3210.5067/E1QL9HFQ7A8M, boulder, Colorado USA. NASA National Snow and Ice Data Center Distributed Active Archive Center., 2020.

Morlighem, M., Rignot, E., Binder, T., Blankenship, D., Drews, R., Eagles, G., Eisen, O., Ferraccioli, F., Forsberg, R., Fretwell, P., Goel, V., Greenbaum, J., Gudmundsson, H., Guo, J., Helm, V., Hofstede, C., Howat, I., Humbert, A., Jokat, W., Karlsson, N., Lee, W., Matsuoka, K., Millan, R., Mouginot, J., Paden, J., Pattyn, F., Roberts, J., Rosier, S., Ruppel, A., Seroussi, H., Smith, E., Steinhage, D., Sun, B., van den Broeke, M., van Ommen, T., van Wessem, M., and Young, D.: Deep glacial troughs and stabilizing ridges unveiled beneath the margins of the Antarctic ice sheet, Nature Geoscience, 13, 132–137, #10.5194/tc-2022-3210.1038/s41561-019-0510-8, 2020.

Rignot, E., Jacobs, S., Mouginot, J., and Scheuchl, B.: Ice-shelf melting around Antarctica, Science, 341, 266–270, #10.5194/tc-2022-3210.1126/science.1235798, 2013.

Shean, D., Joughin, I., Dutrieux, P., Smith, B., and Berthier, E.: Ice shelf basal melt rates from a high-resolution digital elevation model (DEM) record for Pine Island Glacier, Antarctica, The Cryosphere, 13, 2633–2656, #10.5194/tc-2022-3210.5194/tc-13-2633-2019, 2019.

Wilks, D.: Statistical methods in the atmospheric sciences, 2nd ed., Elsevier, Amsterdam Paris, 2006.