Review of Lazeroms et al. “Modelling present-day basal melt rates for Antarctic ice shelves using a parametrization of buoyant meltwater plumes”

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I do not support the practice of anonymous review.

General comments:

This paper presents a new method for computing basal melt rates below Antarctic ice shelves based on a polynomial best-fit to a non-dimensionalized 1D plume model. The major innovation of this work is the methods for computing the parameters (the slope of the ice draft and the height above the grounding line) for the 1D plume fit based on 2D ice and bedrock topography data. The result appears to be a low-cost, physically based method that can capture the large range of observed mean melt rates for groups of Antarctic ice shelves. Melt rate patterns are also argued to be closer to observations than those from other melt parameterizations, though this is not shown quantitatively.

This work represents a *significant* step forward in bridging the gap between more complete representations of sub-ice-shelf dynamics (e.g. in 3D ocean models or 2D plume models) and simplified, ad hoc melt parameterizations that contained little or no physics. Given the computational expense of ocean and plume modeling and the fact that ice-sheet models are not fully coupled into earth system models, there is a need in the ice-sheet modeling community for parameterizations and simplified models like the one proposed here to improve the realism of forcing from basal melting in response to changes in ocean temperature.

The main concern I have with the paper involves the discussion around the temperature correction field ΔT applied to the observed temperatures from World Ocean Atlas (WOA). First, the claim is made that this correction is necessary because of unknown temperatures below the ice shelves, summer biases of observations and the interpolation method used to produce the base temperature field T_0 from WOA. No doubt, these factors do contribute to ΔT. But inaccuracies in the plume model itself are also being swept into ΔT. It is reassuring, as the authors state, that the ΔT is not unrealistically large (as they show it to be for an alternative parameterization), suggesting the strength of the plume-based parameterization. At the same time, the authors’ sensitivity study in Sec. 3.1 shows that melt rates can be highly sensitive to changes in temperature that are of the same order as ΔT. This suggests that the evolution of melt rates, even if they are calibrated to match present-day observations, are likely to be highly sensitive to ΔT. This is not shown or discussed in the paper. An application of this parameterization in ice-sheet simulations forced by time-evolving ocean observations or simulation results would require a method for determining ΔT. The paper would benefit from some more discussion of how the authors foresee ΔT being computed in these scenarios. Namely, what ocean state should be used to compute ΔT? Observations? The initial state of the ocean forcing? How sensitive are the melt rates likely to be to this choice?

Another comment is that this paper relies heavily on Jenkins (2014), an EGU talk that does not seem to be available online. This work is cited 9 times, often with the implication that the reader should be familiar with the equations and notation it uses. I happen to have attended this particular EGU session but, as remarkable as the talk was, I can't say I remember the notation in detail. Given how heavily this work relies on Jenkins (2014), it might be worth either providing a permanent URL to that those slides or providing their contents as an appendix here. Otherwise, I would suggest efforts be made to cut down on how often that work is cited and instead to incorporate its findings directly into the paper.

In addition to the requested discussion above, I recommend a number of minor revisions to the manuscript in the specific comments below. If these are addressed, I would recommend the manuscript for publication.

Specific comments:

In what follows, I will indicate the page number a line number as pp-ll (e.g. 1-1 for page 1, line 1) for simplicity.

2-9: "depend solely on the thickness of the water column beneath the ice shelf" I'm not aware of any parameterizations that use the thickness of the water column only, and the authors don't give a citation for this. Instead, most parameterizations I'm aware of depend only on the depth of the ice-ocean interface (the ice draft), with some parameterizations (e.g. Asay-Davis et al. 2016) *also* using the water-column thickness to taper off melting near the grounding line.

2-14: "Due to their steady nature, it is unlikely that the simple basal melt parametrizations contain enough physical details to capture this complex pattern without either significant tuning or extremely detailed ocean-shelf-cavity models." First, I have trouble following what it meant by "their steady nature". Do the authors mean their lack of dependence on external forcing (e.g. ocean temperature)? Or that they assume steady state? Or something else, perhaps? Second, "simple basal melt parameterizations" by definition will not be "extremely detailed ocean-shelf-cavity models", so I think the sentence needs to be rephrased to differentiate between parameterizations and detailed physical models.

3-10: "Special attention is given to the construction of an effective ocean temperature field from observations, which is required for providing realistic input data of the temperature within the ice-shelf cavities to the parametrization." This is part of my concern about how the ΔT field is discussed in this paper. I don't disagree that there are biases in the the WOA observations but I do not think the authors demonstrate (or can demonstrate) that the correction leads to a more realistic temperature field. Instead, it is important to acknowledge that the various biases in the WOA observations, the interpolation/extrapolation of those observation, and the plume emulator are all being compensated by tuning ΔT, and this process will not necessarily mean that the resulting effective temperature is more realistic than WOA.

4-3: “The non-linearity arose because the exchange velocity γ_T in Eq. (1a) was expressed as a linear function of the ocean current driving mixing across the boundary layer.“ This is not quite sufficient to have nonlinearity. It is also important that the strength of the ocean current is itself a function of the thermal driving. Maybe add something like, “...across the boundary layer, which is itself a function of the thermal driving”.

5-11, 5-12, 5-14: These are not the standard uses of the symbols Γ_T and Γ_S (e.g. Jenkins et al. 2010, Jenkins 2011). The exchange coefficients are typically defined to be distinct from the Stanton number, such that St = $(C_D)^{1/2}$ Γ_T (and similarly for salt). I would *strongly* recommend switching to this more standard notation or there is likely to be confusion when others try to implement the parameterization. (2c) and (2d) would therefore each need an extra factor of $(C_D)^{½}$ and this change would propagate to many other places in the manuscript.

6-12: “This simplified formulation can be used together with the prognostic equations (2) by assuming $T_b = T_f$” My understanding of the 2-equation formulation is not that one necessarily assumes that $T_b = T_f$, but rather that a new equation is adopted with the same form as (2) with T_b substituted by T_f. We never need to know what T_b is but if one were to need it (e.g. as an ice-sheet boundary condition), it would be different from T_f because of the significantly lower salinity at the interface.

6-15: “Also note the similarity between Eqs. (6) and the simple melt model described by Eqs. (1), the difference being the inclusion of heat conduction and the parametrization $\gamma_T = \Gamma_{TS}$ U.” I would say an equally (or perhaps more) important difference is the use of the plume T and S instead of the ambient fields.

6-19: “...different vertical temperature and salinity profiles of the ambient ocean (Jenkins, 2011, 2014).” My understanding is that the polynomial emulator that the authors use does not account for stratification or vertical variations in T and S. This might be worth mentioning explicitly, either here or better yet in the discussion section. Accounting for T and S profiles that vary with depth as well as time would be a potential improvement for the future that might allow the parameterization to produce Mode 3 seasonal melting (as defined in Jacobs et al. 1992) near the calving fronts of “cold” cavities. This could potentially improve the melt pattern.

7-9: “three larger length scales” If it is clear which 3 of the 4 length scales is largest, I missed it. It might be best to explicitly state either which 3 are meant or which one is excluded.

7-17: “...the slope affects the entrainment rate, but not the melt rate...” I carefully read the corresponding section of Jenkins (2011) and I think what is shown is that the term in the mass conservation equation for the melt rate doesn’t explicitly contain the slope, whereas the term for the entrainment rate does. However, when the equations are solved, the resulting melt rate will depend on the slope, since the plume speed and thermal driving (which contribute to this the melt rate, as shown in Jenkins (2011), Eq. (14)) depend on the slope. So I think the phrase should be changed to something like "...the entrainment rate explicitly depends on the slope, whereas the melt rate does not..."

9-9: "In this study, we use remapped data based on the Bedmap2 dataset for Antarctica (Fretwell et al., 2013)," Do the authors perform any kind of a firn correction to the ice thickness, given the assumption of constant ice density in the masking in Table 2? How well does the mask for grounded ice, floating ice and open ocean from Bedmap2 compare with that from the approach in Table 2? The figures suggest that the grounding line might not match well with Bedmap2 (e.g the Amery and deeper parts of the Ross and FRIS) but part of this could be due to the relatively coarse resolution. Without a firn correction, I wouldn't expect the masking from Table 2 to be a good match to the mask provided with Bedmap2.

10-8: "the algorithm searches in this direction for the nearest ice-sheet point." This may be obvious to the authors but I think the method used to search for the nearest ice-sheet point should probably be stated explicitly. This part of the algorithm seems like it could potentially be quite slow, particularly at higher resolution. There might also be approaches (e.g. working out from the grounding line, caching the distance to the G.L. in each direction) that could be used to speed up the process. Is this something the authors have considered?

11-Fig. 3: "$d_n = 1/2(H_{b,1} + H_{b,2})$" why the factor of 1/2 exactly here? Is this because the grounding line is assumed to always fall on the edge halfway between a grounded and a floating point? Also, the reasoning behind the different approaches in (b) and (c) probably deserves a bit more explanation.

11-8: Why such coarse resolution (20 km)? Is the algorithm too costly to apply on finer resolution? Have the authors explored whether it still works at, say, 1 km resolution that seems to be needed to resolve grounding line dynamics? I could imagine that issues with noise due to rapid changes in bed slope (e.g. Fig 5b) would be exacerbated by finer resolution.

12-Fig. 4: There is a strange rim of floating ice around the whole of Antarctica not present in Bedmap2. Is that an artifact of the remapping scheme that was used? Or the masking scheme in Table 2? Perhaps the calving front is being smoothed out over multiple cells, leading to apparent floating ice where none was present in Bedmap2 before remapping? Also, as mentioned above, the grounded vs. floating mask doesn't look like Bedmap2. Is this just the coarser resolution or has something gone wrong either during remapping or the masking procedure in Table 2?

12-2: "The values for the local slope are typically higher both near the grounding line and the ice front, as shown in Fig. 4c." Could the steeper slope the authors see near the ice front be an artifact of smoothing or remapping? The cross sections in Figs. 5a and 6a look quite smooth, even given the 20 km resolution, compared to plots of cross sections from Bedmap2 directly and I have not seen this tendency toward steeper slopes toward the calving front in sections I have taken from Bedmap2.

13-20: “...the discrepancies between the current parametrization and the plume model are largest when the basal slope changes rapidly, because the parameterization responds immediately to the change while the full model has an inherent lag as the plume adjusts to the new conditions.” This problem will likely get worse at higher resolution. Might it be worth looking into a certain amount of along-flow smoothing and/or lag of when computing the effective α? Perhaps something for the discussion section.

14-Fig 5, 15-Fig 6: It seems like what is potentially missing here is a comparison with the patterns from Rignot et al. (2013) or another melt rate field inferred from observations. I believe the Rignot data set is available from Jeremie Mouginot on request. The data set from Moholdt et al. (2014) is available from Gier Moholdt on request.

15-4: “This also means that the simplest basal melt parametrizations currently used in some ice-sheet models, namely constant values or monotonic functions of the water-column thickness below the ice shelf, are far from being valid.” Again, I don’t know of any models using the latter. Perhaps the authors mean “ice draft” instead of “water-column thickness below the ice shelf”?

16-3: “but we will assume that the variations in ocean salinity around Antarctica are so small that the pressure freezing point T_f is only affected by variations in depth.” What about buoyancy (via $\Delta\rho$)? Wouldn’t this also depend on S_a? Also, how has S_a been eliminated from the universal polynomial (given that it doesn’t appear anywhere in Appendix A)? By assumption? Or has it been demonstrated in Jenkins (2014) that variations in S_a in the observed range don't have an appreciable effect? Would this still be true if stratification were taken into account?

16-8: “The best possibility is an interpolated field…” First, I would rephrase “the best possibility” to something more like “We decide a more feasible approach was ...”. Second, to me it is odd to speak of interpolating the field into the ice-shelf cavities. It seems that this is what the authors did, but in my own modeling I extrapolate the field into a given cavity with no regard for temperatures in cavities on the other side of Antarctica that might figure into interpolation. Indeed, my colleagues and I have run into trouble when we were too naive in our extrapolation technique, extrapolating warm ocean temperatures from the Amundsen and Bellingshausen Seas under deep parts of FRIS. This does not appear to have occurred using the natural neighbors interpolation approach used here but it might still be worth acknowledging that interpolating temperature between ice-shelf cavities that really don’t interact with one another is not really physically realistic.

16-16: “requires minimally tuned forcing data to produce realistic output.” First, I’m not sure I agree with the assessment that the forcing in “minimally tuned”, since the tuning likely has a significant effect on melt rates and their evolution, as discussed above. Second, I’m not sure I would characterize the computation of a field with 29 degrees of freedom (to match 13 mean melt rates) as “tuning”, which in my experience refers to attempting to constrain a small number of model parameters rather than a spatially dependent field. Instead, this seems like inversion, much like the approach used to compute basal sliding factors under grounded ice in many ice sheet models. The authors have also characterized this as bias correction, but I do not necessarily agree with that characterization, as I stated above.

18-2: “interpolated using natural-neighbour interpolation (i.e. a weighted version of nearest-neighbour interpolation, giving smoother results) to obtain data in the entire domain of interest.” Again, it seems strange to interpolate *between* cavities. I guess natural-neighbor interpolation effectively extrapolate into cavities as long as the closest open ocean points is in front of *this* cavity and not some other cavity?

18-6: “this modification is necessary for eliminating biases in T_0 caused by the sparse observations and numerical interpolation, and also because the flow dynamics of the ocean are not resolved.” This may be the principle but in reality the authors are almost certainly also correcting for shortcomings in the parameterization itself.

18-9: “29 carefully chosen points” I think more explanation is needed about how these points were chosen. It appears that they are located at grounding lines near the boundaries between shelves with potentially differing properties. Assuming ΔT is held fixed during an evolving simulation, will values of ΔT in regions that are currently grounded be appropriate as the grounding line moves? What might the limitations be? Again, this may belong in the discussion.

18-11: “Note that for technical reasons explained in Appendix A, we have applied a lower limit to the effective temperature equal to the pressure freezing point at surface level.” As the authors show in the results section, this seems to be a significant limitation on the approach, particularly when applied to “cold” cavity shelves like FRIS. Perhaps some discussion is warranted on how this restriction might be relaxed in the future, as I will discuss more below.

18-18: The whole preceding paragraph for determining ΔT is the most worrisome aspect of the algorithm to me. The choice of ΔT (resulting from the details of how T_0 is computed) will potentially determine a lot about how melt rates evolve with time in response to changes in ocean temperature.

18-28: “...yield realistic present-day melt rates for all shelf groups. Therefore, we can conclude that the effective temperature shown in Fig. 8b is a realistic forcing field, at least within the current modelling framework.” I don’t think the authors can make this statement. The field ΔT was inverted to yield realistic melt rates for the 13 ice-shelf groups, so the fact that this goal was reached does not suggest that the effective temperature is realistic. A comparison with observations not used to constrain the model would be needed to make such a conclusion. All the authors can conclude here is that their inversion worked as expected (except for FRIS) and that the resulting temperature field looks plausible.

20-10: “This fact, along with the general melt pattern and the correlation with the surrounding ocean temperature, are in line with observations, e.g. Rignot et al. (2013).” This is by construction, so be careful not to attempt to used this as a validation of the model.

20-11: “However, one should note that the Rignot et al. (2013) melt pattern shows a greater spatial variability, with more patches of (stronger) refreezing occurring between patches of positive melt. The lack of such prominent patches of refreezing in the current parametrization might have different reasons, such as the coarse resolution or the fact that we disregard the details of the ocean circulation within the ice-shelf cavities, as well as effects due to stratification and the Coriolis force.” Lack of seasonal variability in T and S and also lack of vertical variability (not just in the sense of stratification, but also in the sense of having distinct water masses at different depths) likely also play a role. For example, this is likely why Mode 3 melting is missing (as mentioned above).

20-15: “All in all, the plume parametrization, together with the effective temperature field, appears to give a realistic melt pattern for Antarctica, showing both a large spatial variability and average melt rates that agree with observations.” It is definitely a strength of this parameterization compared with its predecessors that it can capture the range observed melt rates. So I definitely think this deserves emphasis. But, again, this is by construction. It is a good property to have but I think the authors should be careful to state that this is not a validation of the model, since the observed melt rates were used in the inversion for ΔT.

21-Fig 10: A comparison with the Rignot et al. (2013) melt rates seems like it would also make sense here. As I said, the data should be available on request. Having theme plotted with the same color map would make them much easier to compare.

21-13: “minimal tuning” As before, I’m not a fan of this phrasing. What was done was an inversion of a field that can be argued to be within a plausible range. To me, this is neither clearly “minimal” nor clearly “tuning”.

23-1: “parametrizations based solely on the local balance of heat at the ice-ocean interface are not able to capture the complex melt pattern...” The authors can rightly claim to have a more broadly realistic melt pattern than these previous studies, with both melting and refreezing. But the authors have not really shown that the complex melt patterns resulting from their parameterization are contributing added realism compared to a simpler pattern with a similar distribution of melting and freezing, and complex patterns are not a goal in and of themselves.

23-14: “...data from observations only need a minimal offset ΔT (between −1.4°C and 0.8°C)” Again, I would suggest a different phrasing than “minimal”. “Plausible”? Also, again I think some discussion is needed about how a time-varying T_0 field would be handled. Would ΔT be held fixed? (The authors seem to imply it would be)? How sensitive will the results likely be to ΔT? Over what kinds of time scales might it be reasonable to hold ΔT fixed? How should data from ocean models be applied? Should a ΔT be computed from ocean-model initial conditions to match observed melt rates? Or should ocean observations (e.g. WOA) be used to compute ΔT?

23-22: “All in all, the presented plume parametrization, together with the constructed effective temperature field, gives realistic results for the present-day basal melt in Antarctica, both in terms of area-averaged values (Fig. 9) and the spatial pattern

(Fig. 10a).” I don’t think this paper as written has shown that the spatial patterns are realistic, just that the mean values are (by construction) consistent with observations. A more qualitative (or better yet quantitative) comparison of the spatial patterns with Rignot et al. (2013) or with another data set derived from observations would be needed to make the latter assertion here. Alternatively, the claim could be toned down, stating that the pattern is reasonable in a broad sense -- highest melt rates are near the grounding line with refreezing closer to calving fronts.

23-29: “For such simulations, the effective temperature in Fig. 8b, even though it is a constructed field, can prove to be a valuable reference state to which temperature anomalies can be added.” As I have said earlier, I think more discussion is needed on how the effective temperature would be used in dynamic ice-sheet simulations using this parameterization. This sentence is a good start but I’d really like to see more.

23-30 “Eventually, coupled ice-ocean simulations (e.g. DeConto and Pollard 2016) can benefit from this approach by comparing ocean-model output to this reference State.” Hmm, I hope I’m misunderstanding but it seems like the authors are claiming that their reference temperature should act as a reference field, from which coupled ice sheet-ocean simulations could be validated. If this is not what was intended, please clarify what is meant here. If that *is* what is meant, that’s a very bold assertion, given the fact that plume-model biases are also “swept under the rug” during the inversion process for ΔT used to produce the effective temperature field.

24-9, 24-11 The numerical constants 3.5e-5 and 10 seem to need units of m and m/yr/°C^2, respectively. Maybe give them names and put them in a table or something, along with 0.545? Also, how were (A2) and (A3) derived, at least in broad strokes?

24-15: x_0 is unitless? How was it derived?

24-22: Reading Jenkins (2011), it seems like X > 1 means there is no momentum left in the plume, so I would expect setting m = 0 beyond this point would be more realistic than restricting T_a to not go below the surface freezing point. By the way, it might be worth discussing why, physically, X should not be allowed to exceed 1.

25: It’s not clear to me that Appendix B adds much to the text, other than to emphasize that the algorithms for finding z_{gl} and α are arbitrary. The results are visibly less realistic than for the algorithm presented in the main text and the problems with the constraint on T_a seem more severe. If you add Jenkins (2014) as an appendix, you might consider removing this one.

One final comment. Since your paper was submitted, an alternative method for parameterizing basal melt by Reese et al. (2017) has been submitted and is also on The Cryosphere Discussions (see full citation below). You might consider discussing how their approach compares with yours, including what the strengths and weaknesses of each approach might be for adoption in a full ice-sheet simulation. I don’t mean this as shameless self promotion even though I'm a coauthor. I really do think these papers are highly relevant to one another.

Typographic and grammatical corrections:

1-1: It is a very minor thing but I would suggest another word or phrase besides "decline", which implies to me that the AIS used to be better than it is now. Perhaps "...major factor in the decline in volume of the Antarctic Ice Sheet" or "...major factor in mass loss from the Antarctic Ice Sheet". Also, "Ice Sheet" should be capitalized in this case.

2-13: "...a complex spatial pattern, which depends heavily on both the geometry below the ice shelves and the ocean temperature." I do not think the observations demonstrate this, though I agree that it is the case. Perhaps rephrase something like "...a complex spatial pattern, which can be inferred to depend heavily on..."

2-17: "within a single ice-shelf cavity" Maybe consider "within individual ice-shelf cavities" instead, since we aren't talking about one specific ice-shelf cavity but rather about any one of several ice-shelf cavities in isolation.

6-30: "The first governing length scale is associated with the pressure dependence of the freezing point that imposes an external control on the relationship between plume temperature, plume salinity and the melt rate, which is determined by the temperature relative to the freezing point. " I don't follow this sentence. What is determined by the temperature relative to the freezing point? Perhaps try to reword or break this sentence into 2.

10-17: "found values" should be "values found"

23-17: "The latter behavior is also apparent in…" It's not entirely clear to me what is meant by "the latter behavior". I guess it is the low sensitivity of the melt rate to changes in temperature, though this is not explicitly the behavior described in the previous sentence.

References

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