

## ***Interactive comment on “Modelling present-day basal melt rates for Antarctic ice shelves using a parametrization of buoyant meltwater plumes” by Werner M. J. Lazeroms et al.***

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### General comments

The manuscript under review presents a new approach to parameterize the spatially resolved basal mass balance beneath floating ice shelves. The applied method uses the scaling of basal melt rates as a function of geometrical parameters and ambient ocean temperatures that has been derived in previous studies from a one-dimensional inclined plume model. Validation of this parameterization for the one-dimensional case is given through direct comparison with the underlying plume model. Aiming at generalizing the approach for two-dimensional applications, algorithms are proposed to compute local geometrical input parameters for an arbitrary ice shelf geometry. To

C1

evaluate the method, circum-Antarctic melt rates are computed by tuning the ambient ocean temperatures to reproduce realistic area averaged melt rates and subsequently comparing the associated spatially resolving melt patterns with maps of observed basal melting.

The authors convincingly show that their method provides a significant improvement compared to the two referred simplistic approaches of Beckmann and Goosse (2003) and DeConto and Pollard (2016) that scale basal melting solely based on the temperature difference between the ambient ocean and the local freezing point. While following directly from a model of the underlying physics, the method is of general nature and (presumably) contains few enough free parameters to prevent over-fitting. Based on this, the presented work is inevitably a sound and useful contribution for better understanding and modelling ice-ocean interactions and should be published.

Said this, the following three concerns need to be addressed to make the merit and scope of this work fully available to the reader.

1 - The origin of the general melt rate curve needs clarification.

In the present manuscript, it is unclear how this central element of the parameterization was obtained and to which extent its derivation has been part of the current study and how much is based on previous works. Although not being explicitly mentioned, an expression of the curve shown in Fig. 2 seems to be given in eqn. A3, but also here lacking a proper derivation or a reference thereof. Meanings of the individual terms are discussed in the text, but neither their exponents nor the fact that they should be multiplied including the factor 10. A more explicit description of what has been done (and by whom) to establish this relation is needed. To ease the line of argument, it would probably also be useful to add a summary of the nature of the basal melt parameterization found by Jenkins 2014 (i.e. the existence of a general melt rate curve) at the beginning of section 2.2., e.g. by moving the paragraph on p. 8, l. 6-14 up front, including eqn. A3, and stating that the remainder of the section will review the meaning

C2

of the respective terms.

2 – The rationale for the extension of the general melt rate curve for the two-dimensional case needs to be explained and discussed.

A basic assumption of the underlying plume model is that the geometry of the ice shelf is uniform in the direction perpendicular to the plane in which the plume is rising (p. 4, l. 15-16). This leads to a system of one dimensional equations (p. 5 l. 6-12) that yields the general melt rate curve when being evaluated for the parameter space of interest. One restriction that is imposed by this one-dimensionality and that is inherit to the general melt rate curve, is that changes in plume thickness and hence the susceptibility of relevant properties (such as plume temperature and buoyancy that in turn controls its speed) are fully predicted by the sum of the fluxes through the upper and lower interface with the ice and ambient ocean respectively. For a two-dimensional configuration, however, one would expect that the width of the plume becomes a dynamic variable of some sort, which through mass conservation affects the plume thickness as well as the width of the interface through which the plume interacts with the ice and ambient ocean. The result would be an increased degree of freedom and I am inclined to believe that this would lead to significant deviations from the general melt rate curve found for the one-dimensional case. This issue is currently lacking attention in the study.

For instance, many ice shelves exhibit asymmetric geometries, being narrower towards the grounding line and wider towards the ice shelf front, as simplistically illustrated in fig 1. Considering that every point of the ice base is covered by a multitude of plumes arising from the deepest grounding line, it is obvious that each individual plume must become wider (and hence thinner) as it ascends towards shallower depth, with direct consequences for its evolution. In fact, augmenting the original plume model of Jenkins (1991) by implementing a varying plume width in a two-dimensional configuration (Hattermann, 2012 section 3.5) it is possible to reproduce melt rates obtained from a general circulation ocean model of a realistic ice shelf geometry for a range of forcing parameters (Hattermann, et al 2014), while for the same setup, the original plume

C3

model is overestimating the melt rates along a one-dimensional flow line by an order of magnitude, primarily because the unscaled (for width) plume predicts too vigorous currents beneath the shallower part of the ice shelf. In essence, the extension into two-dimensions is likely to weaken the influence on the non-local effective grounding line depth, as a thinner and wider plume would more quickly cool and slow down on its rise along the ice base, remaining less of its properties at the source location (possibly also earlier reaching ambient buoyancy and detaching from the ice base, leading to initialization of a new plume at the detaching depth—a case that is not discussed in the manuscript at all).

Much effort has been spent in the current manuscript on reviewing the one-dimensional plume theory that is the basis of the generalized melt rate curve. However, it is currently lacking a discussion and evaluation of the validity of the transfer of that relationship and its underlying physics to higher dimensions and the possible shortcomings therein, such as the above mentioned consequences of mass conservation for an asymmetric distribution of ice shelf area with depth (which is a qualitatively different argument concerning the plume physics than the fact that there might exist multiple plume pathways). I acknowledge that this assessment can be added at various level of detail. Also, in the overall need for simplicity and recognition of other examples of parameterizations that have been used in the past, the presented approach is likely to be justified as is for the purpose of providing boundary conditions for ice sheet models. But the authors need to add some sort of assessment of physical basis for their transfer, which appears to me the major advance of this study.

3 – The evaluation of the performance of the parameterization for the circum-Antarctic case needs improvement, in particular, more information must be provided on the limitations and processes not captured by the present approach.

In the current manuscript, the performance of the method is evaluated by comparison with the simplistic approaches of Beckmann and Goosse (2003) and DeConto and Pollard (2016). In particular, the generalized plume approach is shown to be largely

C4

superior in reproducing a qualitatively realistic spatial pattern of basal melt (increased melting towards the grounding lines) and the need of fewer adjustments of the ambient ocean temperature field to obtain spatially averaged melt rates that match the observations than required by the traditional thermal driving parameterizations. From an ice dynamical modelling perspective, this is certainly an important step forward. However, today, models of a wide range of complexity are used to assess the ice-ocean system (see e.g. Asay-Davis et al., 2016 for a summary) and within the scope of these works, it is desirable to evaluate the proposed parameterization also with results from the other end of the spectrum. A couple of circum-Antarctic ocean general circulation models are readily available (even more regional models, some of already coupled with ice models), providing fields of basal melt rates by explicitly resolving the ice shelf cavity circulation. Although not necessarily yielding realistic results everywhere, all of these simulations provide a self-consistent sets of geometrical parameters and ambient ocean temperatures that can be used to scrutinize the validity of the presented plume parameterization. Applying the new parameterization in the context of a fully resolving ocean model framework of the author's choice appears to be a minor additional effort and I highly recommend that such a comparison is added to this publication, as it would substantially aid the validation of the approach (such as its extension to two dimensions) and greatly improve the understanding and integration of the new method within the context of existing works on simulating basal melting.

Another issue is that the melt rate maps shown in Fig. 10 work well to assess the improvement over the simplistic scaling of DeConto and Pollard (2016), but do not allow to compare the details of the melt rate map with the observations of Rignot et al. (2013) that is used as a reference (p. 20 l. 10-14). In particular, the truncation of the color scale to melt rates of 2 m/yr excludes a quantitative assessment of the maximum melt rates that can be an order of magnitude larger at some grounding lines with important effect on the ice dynamics. Within this scope, it is currently also not accessible to the reader, how the tuning points for the ambient ocean temperature field were chosen and by which algorithm the temperature in these points has been optimized to match the

C5

area averaged melt rates (see specific comments for details). Eventually, there are a couple of processes that are known to influence basal melting around Antarctic, but are not captured by this parameterization, with examples being the influence of regionally varying tidal current strength on the boundary layer heat exchange (Maksinson et al. 2011), as well as the enhanced heat exchange due to winds (Hattermann et al. 2014, Dinnimann et al. 2015) as well as intrusions of solar heated summer water near the ice fronts (Hattermann et al. 2012, Stern et al. 2013). Hence, their influence must either be omitted or be included in the fitting of the temperature field, a limitation of the new approach that needs to be discussed.

Also, to some extent the precision of language and figure quality should be improved.

Specific comments

p. 2, l. 11 & 15: What kind of "steady-state" is referred to and what is meant by the "steady nature" of the parameterizations? Does the new parameterization differ in a manner that it is time-varying of some sort?

p. 2, l. 14: Please clarify the ambiguous formulation "geometry below the ice shelves".

p. 2, l. 27-28: How does the referred mechanism in which upward flowing plumes induce inflow of warm water into the cavity relate to the approach presented? To my understanding, this possible feedback on the ambient ocean temperatures is not part of the plume model or the derived parametrizations, opposing the subsequent statement in line 32.

p. 3, l. 1-2 / p. 8, l. 16 ff / p. 22, l. 6-8: With the above general comment in mind, please reflect on the validity of the underlying physics, in particular the non-local dependence on grounding line depth, when extending the plume parameterization to two dimensions.

p. 3, l. 15-18: It is not always clear, which parts of these sections review the results of previous works and which parts are original contributions of the present study. It is

C6

mentioned that results are summarized from Jenkins (2011) and Jenkins (2014), while particular advances of the present study are not discriminated in detail. To some extent, the problem may arise, because a central reference of the plume theory is contained in a conference presentation, which is not available for reading. However, explicitly clearly labeling review information and original material of this paper at the beginning of the subsections, should sufficiently mediate this issue.

p. 4, l. 3-5: It would be useful to explain how the ocean current that drives mixing relate to the temperature, hence leading to the non-linearity referred to here. Does this refer to the effect of increased buoyancy by decreased salinity due to more meltwater input for higher temperatures?

p. 6, l. 8: For clarity, mention which simplification is applied, i.e. the assumption of a constant ratio between  $\Gamma_T$  and  $\Gamma_S$ .

p. 6, l. 23 ff.: The derivation of the general melt rate curve appears somewhat fragmented and I am currently not able to retrace its origin based on the information given in this section. In particular, it is not clear how the terms in eqn. 7 to 9 combine into a single expression. Specifically, it is unclear how Jenkin's extension of eqn. 7 looks like and what is described by the universal length scale mentioned on p. 7, l. 21 or how it is used. Also the discussion of the two different melt formulations (p.7, l.21-27) is confusing in the given context, as is the summarizing statement in p. 7, l. 28 (amplitude of which curves?!). Clarity would probably be added by stating in the beginning of the section that Jenkins 2014 has derived an explicit and universal expression of melt rates as function of distance from the grounding line (possibly including eqn. A3) and explaining that the remainder of the section revises the basic ingredients, to sketch how the relationship was obtained but without providing a stringent derivation.

p. 8, l. 1 & 2: The plume buoyancy is primarily controlled by salinity, while temperature has only little influence on the density for the given parameter range. Even though this is not stated explicitly, I assume that by parameterizing the plume buoyancy through

C7

the temperature difference between the plume and the ambient ocean, an assumption was made on how the temperature difference translates into a salinity difference (i.e. the freshening of the plume is obtained from transforming its respective source water along the melting-freezing mixing line/ Gade line). Does this imply that the general melt rate curve was obtained by assuming that the ambient water at any location along the plume path is the same (or lies along the same Gade line) as at the grounding line where the plume originates? In this case, this would be an important limitation of the theory, which is almost certainly not true for many ice shelves, where different source water types may dominate the ice ocean interaction in different parts of the ice shelf cavity (e.g. different sources of HSSW beneath Filchner-Ronne or the influence of more buoyant surface water near ice shelf fronts).

p. 10, l. 6-12: If my understanding of this algorithm is correct, valid plume paths will also incorporate directions for which the ice base slope reverses somewhere between the given ice shelf point and the respective grounding line since only the local slope and overall grounding line depth are evaluated. What does this imply for the nature and realism of the resulting multitude of valid plume paths?

p. 15, l. 6-9: Should be moved to discussion and supported through proper references.

p. 16, l. 5: For the given temperature range, the buoyancy of the plume is dominated by salinity differences. Please comment how the uniform salinity field affects the response of the melt rate parameterization (or its inherent ingredients).

p. 18, l. 9-11: More information on this tuning process must be provided. How were the respective temperature differences in the 29 sample points determined? Presumably, some sort of optimization algorithm has been applied, that involves iterative computation of area averaged melt rates and subsequent adjustment of the individual correction points. How well does this procedure converge towards a unique solution for the given cost function? Why were 29 points used and how have they been allocated and how sensitive is the resulting melt rate map to this particular configuration (from Fig. 10a

C8

and Fig. 11a one could get the impression that more spatial detail on the melt rate map correlates with a higher density of correction points)?

p. 18, l. 24-27: It is well known that most of the seawater beneath the FRIS is significantly colder than the surface freezing point. The reason for this is that melt water produced at greater depths is largely recirculated within the cavity and mixes with inflowing water at the surface freezing point, before this interacts with the ice base. Thus a representation of colder ambient water masses would indeed be more realistic in this case.

p. 18, l. 32-33: In fact, the continental shelf temperatures in West Antarctica in Fig. 8a appear rather low compared to observed values well above 0 degC. It would be useful know more about the spatial pattern of basal melt in this region and its comparison compares, in particular if the parameterization is capable of capturing the extremely large melt rates near the grounding lines that are observed here.

p. 21, l. 5-14: Obviously, the new plume parameterization provides significantly improved spatial basal melt patterns compared to the simplistic temperature scaling. However, to this end, it remains somewhat unclear to what extent the obtained spatial pattern of basal melt is a result of underlying dynamics of the parameterization or reflects the optimization of ambient ocean temperatures that were used for the input. Thus, a direct comparison with melt rates from a more comprehensive ocean circulation model remains a desired complement to round off the present study. This, to my mind easy achievable extension of the present work would both help to justify the ad hoc extension for the two dimensional case and scrutinize the predictive capacity of the parameterization that is required for using it in a framework of evolving ice geometry or ocean temperature sensitivity studies.

p. 23, l. 3-7: In addition to the prescription of valid plume paths provided in this study, an extension of the one-dimensional plume theory to higher dimensions needs to account for the effects of mass conservation when the dynamical equations are not

C9

constrained along a path of uniform width. This will have consequences on the validity of the general melt rate curve that need to be addressed here.

Figure 4: Use different colors for open ocean and land areas where the relevant fields are undefined.

Figure 1: Extend range of melt rates, consider using non-linear color scale.

Generally, most spatially resolving circum-Antarctic fields are difficult to assess. Consider the use of zoomed inlets to magnify relevant regions.

Technical corrections

Generally, the manuscript should be edited to improve the precision of language, including the removal of unnecessary conjectures and filling terms (examples being p. 1, l. 23: "Therefore", p. 4, l. 13: "ultimately", p. 7, l. 13: "hence", p. 8, l. 6: "thus", p. 9, l. 12: "easily", p. 9, l. 14: "Now", p. 11, l. 1: "In summary", p. 13, l. 16: "clearly", p. 18, l. 30: "Clearly", p. 20, l. 7: "obviously", p. 20, l. 20: "immediately") as well as first person narratives which is extensively used throughout the manuscript.

p. 1, l. 20: "ocean flow", better use "oceanic heat supply"

p. 2, l. 3: "In the view of these issues", imprecise, clarify: "In order to correctly predict the evolution of the ice sheet"

p. 3, l. 6: "An important part of this work is [the derivation/ the development of] an algorithm"

p. 3, l. 25 & 24: consistently refer to "sea water" when introducing  $\rho_w$  and  $c_w$ .

p. 6, l. 2: if only similar, what is the difference between eqn. 1b and 5c.?

p. 11, l. 6-9: Redundant with p. 9, l. 9-11.

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C10

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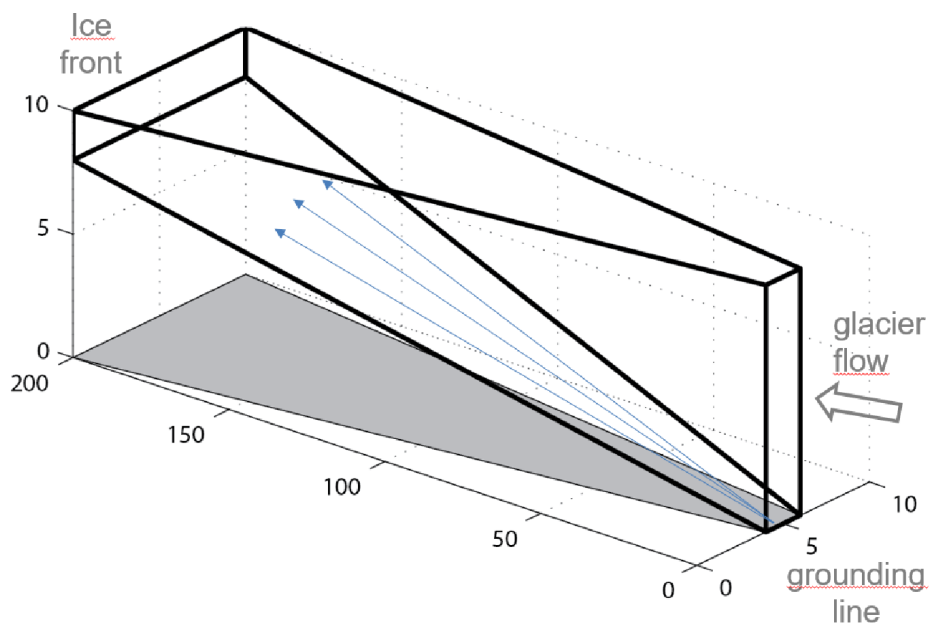
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C11



**Fig. 1.** Idealized illustration of asymmetric ice shelf geometry. Blue arrows indicate possible plume pathways that require widening of the plume at shallower depth to span the entire ice base..

C12