

Interactive comment on “Sensitivity of submarine melting on North East Greenland towards ocean forcing” by Philipp Anhaus et al.

Anonymous Referee #1

Received and published: 2 May 2019

Mass loss from the Greenland Ice Sheet is important to sea-level rise and ocean freshening. The rate of ice loss from some parts of Greenland is modulated by interactions with ocean melting. This study reports the results of a one-dimensional plume model of a layer of meltwater beneath the floating tongue of 79N glacier in north-west Greenland.

The model is first applied in a ‘standard’ case, with the goal of explaining details of the melting pattern beneath the ice tongue, and then several perturbations to the model are applied. Notably, the temperature of the ocean waters forcing the plume model is varied, and the response is taken as a study of the sensitivity of 79N to ocean warming.

This 1D plume model is a highly simplified representation of ocean flow beneath floating ice, and is not a ‘state of the art’ model, as full 3D ocean models are now com-

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monplace. Therefore, in my opinion the reader needs to be convinced that this simple model can capture the relevant physics, before they can believe the sensitivity of melting to ocean temperatures that emerges from this model. As detailed below, I am not sure that the present paper demonstrates the utility of this simple model.

Broader points

The model neglects almost all of the complexity of flow beneath floating ice, including Coriolis, tides, 3D flows in complex topography, shelf-driven circulation, flow around the island, etc. Therefore, this model needs to demonstrably reproduce observations in order to be credible. I did not find that the current manuscript demonstrates this. On several occasions the paper cites the remote sensing melt rates of Wilson et al, but I didn’t see an explicit comparison of the plume model results to those observations. In the absence of that comparison, the melt rates appear to be significantly too high. On Page 14 the authors quote a balance melt rate of 4.2 m/y. The ‘standard’ simulation has a melt rate of 15.2 m/y. Is 79N glacier thinning at a rate of 11 m/y? Such a thinning rate should be easily visible from satellites. Assuming instead that the ice shelf is in balance, I infer the plume model melting is too high by a factor of 3.5. This suggests that the plume model melting sensitivity to warming is also much too high, and this weakens the credibility of the study. The authors could address this point by explicitly validating the plume model melt rates against observations, using satellite-derived melt rates that take into account any thinning in the ice shelf. They should validate melt rates along the plume path, and also in a mean ice shelf sense.

The apparently high melt rate does not really decrease in any of the sensitivity studies in table 3, apart from the one in which the entrainment coefficient is decreased further. But even in the standard simulation the entrainment coefficient is already at a very low value, relative to the literature, and the perturbed value is a full order of magnitude lower than the value recommended by Bo Pedersen. Thus it may appear that the plume model is structurally incapable of reproducing observed melt rates, as a result of its simplified physics.

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I think the whole CTD cast is being specified as the 'ambient' water for the plume (page 10). However, this is circular, since the upper part of the CTD cast already contains the meltwater that is the 'plume', as evidenced by Figure 3c. In other words, the 'answer' is being specified in the 'forcing'. It would be a more valid experiment to specify the pure source water, i.e. the warmest densest AW only at the bottom of the CTD, and then see if the plume model can generate the observed colder meltwater in the upper part of the CTD. Since this approach would warm the ambient waters relative to those used in the experiments, I infer that it would even further exacerbate the excessive melt rates.

The authors discuss whether their melting sensitivity to ocean temperature is linear or nonlinear. I have several comments: i) The authors report a quadratic fit in Figure 5 but seemingly only based upon the 7 warmest temperatures. Why not use all of the temperatures? ii) They later claim that the fit is linear for the 7 warmest temperatures, which is fine, but that linear relation cannot be universally true since it does not pass through zero melting for zero thermal driving. So the nonlinear fit must be the more general relationship. iii) The linearity or otherwise is not rigorously tested using a statistical test. iv) On page 20 some reasons for the linearity are stated. As described above, I think the results are entirely consistent with the quadratic fits of Holland et al 2008 over the wider temperature range, and so there is no discrepancy to explain. Further, any discrepancy that is present would most obviously be explained by the lack of Coriolis force in the plume model.

The stability of the ice shelf is discussed on page 18 and Table 4 as if it is a passive ice body that simply melts away in response to a perturbed ocean melt rate. There are several problems with this: i) Ice thinning will induce ice feedbacks, such as enhanced discharge, which could stabilise the ice. ii) The ice shelf would collapse long before it melted to zero thickness. iii) Ice thinning will induce ocean feedbacks, such as decreased melting as the ice thins into colder waters.

More specific points

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General: No attention is paid to seasonality of the subglacial input?

General: are tides important?

Title: suggest changing to 'Sensitivity of submarine melting of 79N glacier to ocean forcing'?

Abstract and elsewhere: there is a claim of 5% and 12% of total Greenland freshwater flux. What does this mean? Is this claiming a fraction of the steady state ice discharge from Greenland, or a fraction of the unsteady mass imbalance of Greenland, or perhaps a fraction of the total Greenlandic ice melted by the ocean, or its unsteady component?

P2 L10: clarify the extent to which the preceding discussion was relevant to 79N glacier. I think it was mainly about the fjords to the south, which do not have ice tongues. Is 79N losing mass?

P2 L26: The papers cited are not primarily observational.

P2 L29: Distinguish between meltwater and glacial modified water?

P3 L8 and other places: Ice Shelf Water (ISW) is a recognised water mass, meaning water below the surface freezing point. There is very little ISW here, so re-name this to a meltwater plume model or similar.

P6 L11: Melt rates are high due to high slope, not pressure depression of Tb, which is small.

P7 L12 onwards: This paragraph is very confusing. I couldn't follow most of the sentences in it.

P9 L6: With the 'veering', are the authors referring to the very slight deflection of the line within the inset of figure 3c, seemingly from one meltwater mixing line to another? I couldn't follow why that is necessarily caused by runoff. It could be caused by mixing between water masses, or two sources of AW driving melting in different locations?

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P10 L5: need to define the upper limit of AW

P10: figure 8 is cited out of order.

P13 L16: what does 'in a 2D or 3D concept' mean?

P13 L34: no iceberg calving?

P17 L2: The plume did not evolve? why not?

P20 L20: the entrainment rate is not constant.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-35>, 2019.