The Cryosphere Discuss.,
doi:10.5194/tc-2016-284-RC2, 2017

The Cryosphere
Discussions
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Interactive comment on "Submarine melt parameterization for a Greenland glacial system model" *by* Johanna Beckmann et al.

Anonymous Referee #2

Received and published: 16 March 2017

The authors consider buoyant meltwater plumes rising along planar ice faces through the adjacent ocean, motivated by ice sheets melting into the ocean in Greenland fjords. Previous theoretical models are reviewed for a line plume with distributed subglacial discharge and half-conical plume with a localised subglacial discharge, before considering numerical and approximate analytical solutions, their comparison to previous detailed ocean circulation models, and comparison to cumulative melt rates in a range of field observations.

For the theoretical part of the manuscript, several of the results and key scalings from the plume modelling have been identified before in previous studies that consider individual dynamical regimes of the plume models. (i.e. limits where the buoyancy is dominated by subglacial discharge, or where the buoyant freshwater supply is

dominated by interfacial melt, and line plume vs conical plume geometries; see below for details). It appears the authors were unfortunately unaware of a selection of these studies, and so the work is not fully set in context as it stands. There was also some disagreement between one of the numerically diagnosed scaling laws and some previous analysis. These previous studies spanned the main limiting cases identified in the present work, although the authors provide a physically elegant way of producing an approximate solution for line plumes. This approximate solution patches the different limits together by considering their buoyancy flux, thus providing a single prediction for how flow velocity and melt rate varying over the full depth.

In my view the main novelty of this work comes from the attempt at a systematic comparison of both types of plume theory to a range of results available in oceanographic observations. Whilst this has been done before for individual case studies, there may be potential for new insight from some further synthesis of the present results, to evaluate the plume model across a range of conditions. I would suggest a shift of emphasis in the manuscript: cut down some of the initial analysis of the numerical model results where they overlap with previous work, and focus more on synthesising the key results and the comparison of the plume models to observational estimates of melting.

The article is written in an engaging style, with a modest selection of typos and grammatical nuances. Most of the figures are clearly presented.

Detailed comments and technical queries follow below.

Main comments:

1. Most of the plume scalings presented here recover results derived in previous work, and this is not fully acknowledged in the present manuscript:

   - Section 3 of Slater et al (2016) has previously derived an approximate analytical solution for conical plumes, seemingly with some discrepancies with the present work. It wasn't obvious how these compare to your estimate in equation (13), and in particular how Q depends on z in (13)? Similarly, figure 3 suggests there is convergence to a constant velocity near to the grounding line, but the analytical and numerical results of Slater et al (2016) suggests the constant value of U breaks down for larger z. Does your scaling hold throughout the depth of the ocean? Finally, the cumulative melt rate for the conical plume is argued here to scale as the $2/5$-power of the subglacial discharge, whereas Slater et al (2016) found a $1/3$-power dependence (their equation 11). Can you test this discrepancy more carefully?
   - The scaling inherent in the balance velocity (12) for line plumes with strong subglacial discharge was derived in equation (21) of Jenkins (2011). The corresponding convergence to solutions with an initially uniform velocity (c.f. figure 1.21 and appendix A2) was previously considered in section 3 of Dallaston et al (2015), albeit with a simplified model that captures the leading order behaviour.
   - The behaviour of the line plume model for small subglacial discharge has been considered in section 3.1 of Magorrian & Wells (2016). The result presented in (A19) recovers this as a limiting case, and it would be good to emphasise these linkages. The current method of asymptotically patching the two limits together (weak and strong subglacial discharge) by considering the total alongslope buoyancy flux is physically elegant and practically useful, so it would be good to emphasise that it recovers the key limits seen in previous work.

2. Figure 9b. Your plotted subglacial discharge exponent for line plumes disagrees

with the $1/3$-power determined by Slater et al (2016), and indeed there is some evidence of a weaker dependence for large discharge in figure 9b. Can this be investigated in more detail?

   A more convincing way to demonstrate a proposed power law scaling $\dot{m} = aQ_{sg}^{\beta} + c$ is via a compensated plot of the form

$$y = \log\left(\frac{\dot{m} - c}{a}\right)\frac{1}{\log\left(Q_{sg}\right)} \qquad \text{vs.} \qquad t = \log\left(Q_{sg}\right).$$

   On such a plot, any region of pure power-law scaling produces a constant value equal to the exponent $y = \beta$

3. Discussion of sensitivity to entrainment rate in section 3.2. The sensitivity of melt rate to entrainment for a line plume can be understood from the previous results of Jenkins (2011) and Magorrian & Wells (2016). The more novel point that you make with figures 6 and 8 is that the uncertainty in E yields quantitative changes to predicted melt rates that are comparable to non-trivial changes in forcing variables such as subglacial discharge and ambient temperature. Some of this section (and the range of figures) could be condensed by exploiting references to earlier studies, and hence highlight your new results more clearly.

4. Section 3.3. How are the scaling exponents for $\dot{m} \propto TF^{\beta}$ determined? These estimates could be compared to the previous scaling results in Slater et al (2016), Jenkins (2011) and Magorrian & Wells (2016) which analytically predict dependences on ambient temperature. Also, simulations with realistic stratifications have been considered previously by Carroll et al (2015), Carroll et al (2016), and the results here should be placed in the context of this previous work.

5. Section 4. The comparison of plume models to general circulation models has been carried out in a range of previous studies (e.g. Sciascia et al 2013, Carroll et al 2015, Kimura et al 2016). (It should also be noted that eddy diffusivities or

grid resolution can differ between models and may be tuned to best match the plume model, and thus this might not be a fully independent test of plume theory). I think the paper might read better if this section were cut down to summarise the result wherever a previous comparison is available. Also, figure 5 of Sciascia et al (2013) compared a plume model to their numerical results, and obtained a tighter fit than you obtain with the black line in figure 13. Can you explain this discrepancy? Are the same heat and salt transfer coefficients being used?

6. Section 5. Comparison of plume models to observations. In section 5.2, the observations of Fried et al (2015) were compared to a plume model by Carroll et al (2016), echoing some of your key conclusions. These should be acknowledged appropriately. More generally, the section would benefit significantly from greater synthesis and comparison between the results in different fjords. Can you provide any insight into whether the plume models are capable of predicting melt rates consistent with all the observations within error bars, with a single set of parameters (entrainment, drag, heat and salt transfer coefficients) used throughout?

   It would also be worth adding a cautionary note that you sometimes get a misleading picture from estimates of melt rate based on synoptic surveys of ocean heat and freshwater content downstream from the ice. This is due to variability in heat storage in the fjord that might not be captured in a snapshot (Jackson et al 2014).

Minor comments and clarifications:

7. p2 lines 32-35. It would be good to emphasise the different settings considered here, which have different force balances (large scale nearly geostrophic flow under a sloping ice shelf vs non-hydrostatic flow next to vertical ice faces).

8. Section 2.1.1. Cite the source of the line plume model.

9. Equation (2) omits a term of the form $-\partial/\partial x\left(D^2\Delta\rho g\cos\alpha\right)$ under sloping ice shelves (e.g. see equation 7 of Payne et al 2007, then resolve into components along slope). This might be discussed?

10. Page 6 line 15. Need to define W - presumably the width of the fjord?

11. Section 3.4. It may be worth mentioning that the applicability of your plume scalings is confined to warm fjords (if the fjord is close to the freezing temperature, the pressure-dependence of the freezing temperature becomes important as considered by Jenkins, 2011).

12. Sections 3.4 and 5.1. Discussion of the Coriolis effect. You should qualify this statement by emphasising that the Coriolis effect is significant for flows with small-to-moderate Rossby number $U/fL \lesssim 1$, where f is the Coriolis parameter, U the horizontal component of velocity and L the characteristic lengthscale. You might also note the observed channelisation of melt on Petermann noted in Rignot & Steffen (2008), and modelled by Gladish et al 2012 (for example).

13. Section 5.2. Discussion of disagreement for EQUIP. Is it possible that there is a non-trivial rotationally steered outflow here?

14. End of section 5.2, discussion of disagreement over undercutting in figure 17. The disagreement might potentially be explained by near surface calving, or local temperature/salinity differences if there is surface run off into the upper ocean very near to the glacier. This could be added to the discussion.

15. Section 5.4, discussion of Gade and Motyka methods. It would be useful to briefly explain the difference between these cases.

16. Conclusion 3 about the limited effect of entrainment on the melt rate seems to slightly contradict earlier discussion, where you argued the uncertainty corre-

sponds to a $1°C$ change in ocean temperature, or significant uncertainty in subglacial discharge.

17. Page 15, lines 15-18. Can you clarify what ranges of conditions were considered for this comparison?
18. Before equation (A13). Can you clarify in what sense this is an asymptotic solution? I.e. what are you considering to be small or large?
19. Combining figures 1 and 2 as (a) and (b) might save some journal pages.
20. Figure legends. There are inconsistent levels of precision in numerical values in the legends across many figures, and sometimes inconsistencies with the captions. Can the correct values be clarified?
21. Figure 9 belongs before figures 6-8, according to discussion in the text.
22. Figure 10. Would it be more instructive to plot these values per unit width of the fjord, so that they can be compared fairly?
23. Figures 7, 13a,15 illustrate messages from earlier work and might be omitted to cut down on length. Similarly some of figures 1.21-1.23 might be condensed/omitted where the point is clear in earlier work.
24. Figure 1.20. Values of $E = 1.6$ are unreasonably large. The plume model relies on a boundary layer approximation that the plume is thin compared to its along shelf extent $(D \ll X)$ which breaks down for large E

Typos:

25. Page 3 line 18 "organised as follows."

26. Page 4 line 18 "in the x direction"
27. Equations (5) and (6) - broken subscripts on some terms.
28. Page 6 line 20. Typo in $x = 0$.
29. Page 7 line 9/10: "which is the maximal discharge of Store Glacier along a 5 km wide glacier front in order to (Xu et al 2012)". Sentence seems garbled?
30. Page 8 line 9 "undertook"
31. Page 8 line 13 "separate"
32. Page 8 line 27: An entrainment rate of 1.6 is presumably a typo? (It would lead to solutions that invalidate the boundary layer approximation used to derive the plume equations)
33. Page 9 line 16 "explanation"
34. Figure 19 caption "sublgacial" typo.

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