Reviewer 2

We want to thank reviewer 2 very much for the constructive comments and especially the literature references pointed out. Changing our manuscript as mentioned by the reviewer and incorporating the missing literature will strengthen the scientific significance of this manuscript.

Reviewers' comments are in indented blocks and in *italic*.

Response to specific 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?*

To answer the reviewers' question we will undertake scaling analysis for the cone plume likewise we did for the line plume in the appendix. Thus we will compare our result more carefully to those of Slater et al. (2016) in order to answer reviewers' questions. Furthermore we think the manuscript will benefit from additional scaling analysis since it would allow us to make a better comparison of cone and line plume, which is one of the novelties of our paper.

> *• 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.*

We are very grateful to the reviewer for providing these references that confirm our results for scaling analysis for the line plume. We will incorporate these affirmations and acknowledge previous results in the revised manuscript.

> *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*
>
> $$y = \log\left(\frac{\dot{m} - c}{a}\right)\frac{1}{Q_{sg}} \quad \text{vs.} \qquad t = \log(Q_{sg})$$
>
> *On such a plot, any region of pure power-law scaling produces a constant value equal to the exponent* $y = \beta$

Note that Figure 9b) gives the melt rate dependence for the cone plume (CP), while the line plume (LP) is displayed in 9a). In Fig. 9a), the exponent of the line plumes (1/3) agrees with Slater's work. Additional scaling analysis for the cone plume (see response to the first part of comment 1) will give additional insight on the behavior of melt rate with respect to subglacial discharge. We thank the reviewer suggestion on improving the demonstration of the power law with a log-log plot. If it improves the explicitness of the figure we see no a priori reason against using it.

> *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.*

As the reviewer suggested, we will concentrate more on our novel findings in the discussion of the entrainment rate and will shorten this part of discussion with the comparison of earlier studies. We will also eliminate redundant figures.

> *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.*

Our determination of the power law dependence on the melt rate to thermal forcing was done numerically but we will incorporate our analytical results from our scaling analysis. We will compare our experimental with analytical solutions presented in the cited publications.

> *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?*

As we wrote above, we will follow the reviewers' suggestions and will shorten this section. As far as comparison with Sciascia et al (2013), personal communication reveals that the temperature profiles used in their work was different from ours.

> *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).*

We will follow reviewers' constructive suggestion of making a greater synthesizing of our results. We very much agree with adding the cautionary note as it appears to us as a crucial point when comparing model results to observational data. We will discuss these limitations of empirical data for testing our modeling approach.

All minor concerns will be addressed appropriately.