Response to general comments

We thank the Referees for their critical and constructive comments. Both Referees point out that the paper section in which we compare the plume model to high resolution models is partly redundant to previous work and that this section (which is already shortest in the manuscript) can be additionally shortened. We agree with the Referee 2 that the main focus of the paper should be on its novel results - sensitivity analysis and comparison of plume models to observational data. We will modify the manuscript accordingly

In what follows we respond to the major concerns individually.

Response to specific comments

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

Reviewer 1

> *The authors list the previous studies on the melt rate dependencies to the external forcing factors, such as ocean temperature and subglacial discharge. This part is written as if the previous results are inconsistent with each other, but it is not. For example, it is not fair to compare Sciascia et al. (2013) versus Holland et al. (2008b) and Little et al. (2009). Sciascia et al. (2013) considers the effects of subglacial discharge on the melting of a vertically terminating glacier, while Holland and Little (2008b) considers the effect of circulation inside an ice-shelf cavity on the melting. These papers address different problems. As a result, Sciascia et al. (2013) uses a nonhydrostatic model, whereas the other two studies use hydrostatic models. The authors states "A closer look on the CP melt rate profiles revealed differences among the 3D models: Kimura et al. (2014) showed a melt rate profile of the CP that reaches its maximum near to the water surface while Slater et al. (2015) and Xu et al. (2013) found a CP melt rate profile with the maximum located near to the bottom." This gives an impression that the numerical models are not consistent with each other.*

We agree with the reviewer that some sentences in this paragraph may make an impression that there are inconsistencies between different results performed with high-resolution 2-d and 3-d models. This was not our intention – instead we wanted to give a short overview of a number of relevant modeling works performed in recent years to study glacier-ocean interaction. We will rephrase this paragraph to avoid such misinterpretation.

> *The authors do not seem to understand that this difference originates from the difference in the model set up. The background stratification in Kimura et al. (2014) is uniform, while Slater et al. (2015) and Xu et al. (2013) employ linearly stratified profiles. The plume reaches its maximum height until it depletes the buoyancy to the surrounding environment, so the plume can reach higher in the uniform environment than the linearly stratified environment for a given amount of discharge (source of buoyancy). There are assumptions that go into setting up these numerical models and depending on the assumptions the outcomes are different. As a result, comparing the plume models*
> *and these modelling results by plotting profiles of melt rate, temperature and velocity, such as in Fig 13, 14 and 15, and coming up with a scaling factor do not provide any scientific insights.*

Of course we are aware of the different model setups and in each case we used vertical temperature and salinity profiles **identical** to the corresponding GCM experiment. We realize that this was not made clear enough in our manuscript. We will make this point very explicit in the revised manuscript. At the same time, we do not agree with the reviewer that such comparison does not provide any scientific inside. The aim of our comparison is to test our simple parameterizations against scientifically based models. This comparison shows that the simple plume parameterization produced qualitatively rather similar results to much more computationally expensive 3-d model over a large range of melt rates, but to get a quantitative agreement, a constant scaling coefficient in the order of one has to be applied. We also found that the chosen value for the entrainment coefficient has a significant impact on the simulated melt rate, and thus on the agreement with physically based models. We believe, these are important findings.

> *Temperature and salinity of the subglacial discharge are set to T0 = 0 and S0 =0, while the model uses the linearlized freezing condition, equation 7. According to the equation 7, the freezing temperature for the freshwater (S0=0) is lambda2 + lambda3*Z. This means that the prescribed subglacial discharge is below freezing at the source (x=0), 0< lambda2 + lambda3*Z, which implies freezing at the source (melt rate below 0). What is the melt rate at the source? Profiles of melt rate presented in the paper, Figure 7, 11 and 12, all seem to indicate above freezing near the source, which seems inconsistent.*

Firstly, contrary to the reviewers' assumption, freezing temperature of freshwater Ts=lambda2+lambda3*Z<0 for fjord depths larger than 109m (see Table 2.1 for numerical values), which is the case of all fjords we considered in this study. The later was not stated explicitly and, probably, caused this confusion. We will make this point clear in the revised manuscript. As a result, temperature of the plume is always above the freezing point at the source and therefore melt rate is positive (Z< -lambda2/lambda3). As far as the choice of initial temperature T_o=0 ^{o}C is concerned, we believe this is a reasonable assumption. The temperature of subglacial water is unknown, but for obvious reasons it cannot deviate significantly from 0^{o}C. Compared to other uncertainties in plume parameterization, this is probably the least important one. In particular, for conditions typical for the Greenlandic environment, we did not find any significant change in melt rate when using the pressure melting point instead of T_o=0^{o}C, since the plume temperature rapidly converges to a balance temperature close to ambient water temperature (see Appendix.)

> *The authors use the entrainment rate of 0.036 to estimate the melt rates of Greenland glaciers. The authors justify this choice by comparing the shape of plume to that from the high-resolution numerical model results of Sciascia 2013 and Xu 2012 (page 10, line 30). I do not understand this justification because Sciascia et al. 2013 calibrates the unresolved process using the entrainment rate of 0.08.*

We agree that respective sentence in our manuscript is misleading. In fact, we used the same entrainment coefficient as Sciascia et al. 2013 (as displayed in Figure. 13 a). However, we also tested other values for entrainment coefficient and found that for E0=0.036, plume model are in better agreement with result from Sciascia et al. 2013. We will rephrase this sentence to avoid confusion.

The authors conclude that the overestimation of melting by LP is due to the lack of Coriolis term in the plume model. This conclusion comes out of nowhere. There are no constructive arguments to support this point in the paper. The authors need to explain how the Coriolis term changes the plume dynamics and results in lowering the melt rate.

We thought that this fact is well-known. In particular, this limitation of 1-d plume model has been recognized already in Jenkins (1991) who wrote: “However, in this study the influence of Earth rotation is not considered, so the results are strictly only applicable to regions where the flow is constrained by topography.” and later “This is because the Coriolis force, which is not incorporated in this simple one-dimensional treatment will tend to deflect the flow across the basal slope, hence reducing the $\sin\theta$ term”. Following the reviewers’ request, we will discuss the role of Coriolis force in the manuscript.

All minor concerns will be addressed appropriately.