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

Anonymous Referee #1

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This paper describes an application of line and axisymmetric plume model to melting of glaciers in Greenland. The main part of the paper discusses a detail comparison of the plume models to results from idealized high-resolution numerical models, followed by a list of melt rates of Greenland glaciers calculated from the plume model. In a current form, the method used to compare the result is not very original, and the paper does not provide any new scientific ideas. My main concerns are below:

Concern #1

Page 11, 4.4

The first conclusion of the paper states that the plume model reveals a similar qualitative behaviour to the high resolution numerical modelling studies, but this point has already been made by previous studies, e.g. Xu 2012, Slater 2015 and Sciascia et al.

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2013.

Concern #2

Page 2, line 30-page 3, line 5

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. 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.

Concern #3

Page 6, line 14

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.

Concern #4

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.

Concern #5

Page 14, line 29, conclusion #5

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.

Minor concerns

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Page 1, line 8: computationally instead of “computational”

Page 13, line 14: missing space between “model.” and “Fried”.

Page 13, line 19: the units m and d should be non italic.

Page 20, line 20: should be “axisymmetric plume” not “axis-symmetric plume”.

Page 25, line 25: No need to cite the same paper by Sciascia twice.

Page 14, line 14. missing “.”

Figure 1, The entrainment should be perpendicular to the rising plume, so the red arrows need to be adjusted.

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