Dear Anonymous Reviewer #1,

Thank you for your review and your constructive suggestions. We have worked hard to improve the paper, and hope that you will be satisfied with our response. Below we have responded to all of your suggestions using bold font, and most of your suggestions have been applied in the resubmitted version of the paper marked in red.

Best regards,

Philipp Anhaus, Lars H. Smedsrud, Marius Årthun, and Fiammetta Straneo

Broader points

1) Model neglects complexity, Comparison between simulated melt rates using the 1D meltwater plume model and satellite-derived melt rates from Wilson et al., 2017

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.

These are important concerns and suggestions, and we agree that this should be improved in the paper. We will thus implement a better comparison by plotting the satellite-derived melt rates on top of the simulations from the 1D ISW plume model (Figure 4a). However, the satellite-derived melt rates are also estimates and have their own assumptions, and cannot be regarded as "truth". The plume model uses high resolution topography and observed ocean temperatures, so it is also based on observations. In the work from Wilson et al., 2017 a small band downstream of the grounding line is excluded, because it is hard to measure that part from space (Wilson et al., 2017 and Wilson N. (2018), personal communication) and they do not show submarine melt rates from this area, where highest melt rates are expected and simulated by the plume model. Maximum melt rates found by Wilson et al., 2017 are between 50 m/yr and 60 m/yr downstream from the neglected band. We found, that this coincides with the results from the plume model.

One assumption in Wilson et al., 2017 is that the floating ice tongue of the 79NG is in hydrostatic equilibrium with a constant ice density of 920 kg/m³. They state that hydrostasy is a good approximation over sufficiently long horizontal length scales and shallow tongue thickness gradients based on the work by Brunt et al., 2010. However, near the grounding line the tongue thickness gradients for the centreline and the south coast are large (Figure 8e in our manuscript; Mayer et al., 2000; Schaffer, 2017; Mayer et al., 2018) and the slope is steep (Figure S1). Within that area the hydrostatic assumption is less justified and, thus, Wilson et al., 2017 excluded data within a few

kilometres of the grounding line. Downstream of 5.8 km from the grounding line, simulated melt rates from the plume model are 50 m/yr and less (Figure 4a in our manuscript) and, thus, in our view, comparable to results from Wilson et al., 2017.

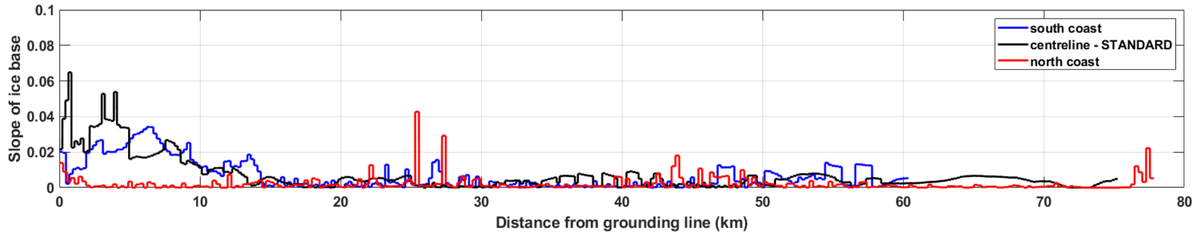

Figure S1: The slope of the ice base sinϕ of the 79NG with respect to the distance from the grounding line in km along three flow lines from RTopo2 (Schaffer et al., 2016). Centreline (black), South Coast (blue), and North Coast (red). The flow lines are marked red in Figure 2b in our manuscript.

In their Figure 2 (lower), Wilson et al., 2017 show the melt rates in a transverse profile of the 79NG. There are clearly two peaks visible with melt rates of about 75 m/yr, supporting the results of the plume model. The transverse profile is taken approx. 1 km downstream of their defined upstream flux gate.

We have additionally actually used the satellite-derived melt rates from Wilson et al., 2017 to constrain some of the model parameters. This led to the choice of the entrainment coefficient to be 0.018. The overall important coefficients (entrainment and drag) have a large range in nature, and sub-ice-shelf values are uncertain, we therefore made sure that the simulated melt rates are in reasonable agreement with the satellite-derived melt rates from Wilson et al., 2017.

The plume model is well tested (Jenkins, 1991; Smedsrud and Jenkins, 2004; Jenkins 2011; Schaffer, 2017; Mayer et al., 2018) - and captures the important physics. Mayer et al. (2018) recently used the plume model to simulate thinning rates of the Midgardsormen, a part of the 79NG ice tongue. We have also tested the sensitivity to tidal flow and found that the tides are too small to affect the melt rates (Anhaus, 2017, Master`s thesis, unpublished). The new text about tides will be found in Section 2.4.

Reeh et al., 1999 estimated the total freshwater volume from the 79NG produced by submarine melting at about 13 km³/yr. This compares well to the freshwater volume simulated by the plume model of 19.7 km³/yr (STANDARD, Table 4), and we are thus confident that the plume model does a good job.

2) Entrainment coefficient

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.

It is indeed correct that the melting depends on the entrainment coefficient. But by following the observed topography at horizontal resolution, and constantly calculating plume speed and thickness, this plume model should be better capable of simulating realistic melting than a coarse 3D model. To learn how sensitive the 79NG is to increased ocean temperatures an ocean model is clearly needed. All models are wrong – some are useful. So, we hold that this 1D plume model is

useful, and give new insight based on the simple, but sound, assumptions of a constant entrainment coefficient. The suggestion by Pedersen (1980) yielded reasonable results for ice base slopes of 0.01 and less. However, the slopes within the GLZ along the centreline of the 79NG are much steeper, with a mean of about 0.03 and a maximum of 0.06 (Figure S1).

3) CTD

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.

It is correct that the observed CTD profile contains a fresh plume, but only in the upper 100 m. Usually the plume detaches at this depth (Figure 8e), but this is also approximately the depth of the ice front. This will be explained more clearly in the text.

Additionally, to investigate this further we performed a model simulation containing only the warmest, densest AW in the CTD profile obtained in the rift of the 79NG in September 2009. This profile contains 271 entries for depth (-600 m to -60 m), temperature (0.99 °C), and salinity (34.66 psu). We found only minor changes. The mean melt rate increased from 15 m/yr in the STANDARD case to 17 m/yr. The maximum melt rate increased from 76 m/yr to 80 m/yr.

However, the plume path increased from 75 km to 80 km in response to the saltier ambient water close to the ice base. A greater buoyancy difference was thus simulated between the ambient and plume water (Figure S2), and it takes longer for the plume to get neutrally buoyant. In this case the plume is not neutrally buoyant at 80 km, this is only the length of the ice tongue. As shown in Figure S2 is the density contrast above zero.

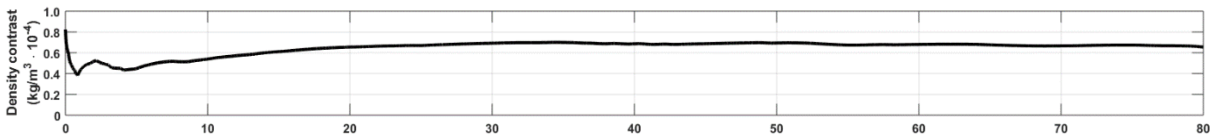

Figure S2: Density contrast between the AW and the plume along the centreline of 79NG in the case where the AW consists of the warmest densest water found in the water column at 600 m depth.

4) Melt rate dependency

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.

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?

We have implemented the suggested larger range for the temperature sensitivity in Figure 5. We did not do this earlier because we thought it was outside the interesting range.

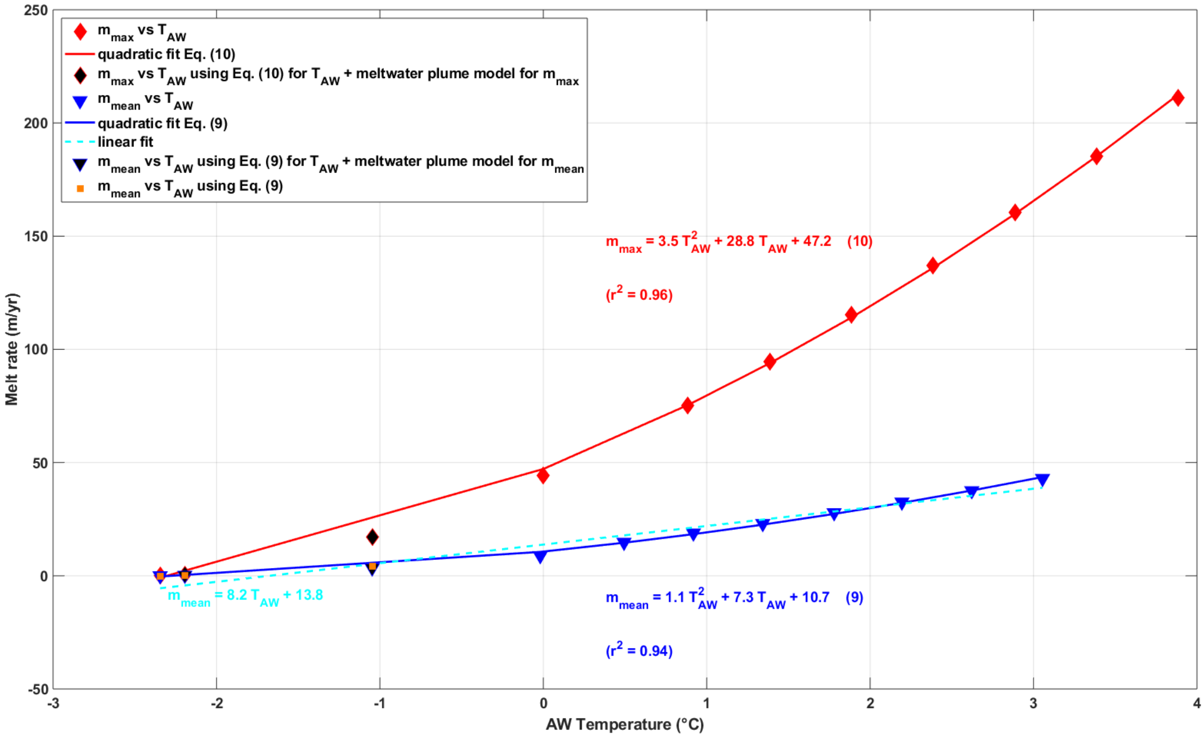

Figure 5. Mean melt rate along the centreline of the 79NG ice tongue (blue), maximum melt rate in the grounding line zone (red), and melt rate calculated using the quadratic fit function (green) depending on the AW temperature as described in the text.

ii) … 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.

This is a valid point. After performing statistical tests we conclude that the relationship is indeed nonlinear.

iii) The linearity or otherwise is not rigorously tested using a statistical test.

We agree that this is a useful addition to the sensitivity and have used several statistical tests as described below. This lead to some new related text that will be implemented.

We tested a linear model and a quadratic model using ANOVA test with a confidence interval of 95% in MATLAB (Table S1). The residual sum of squares for the linear model (93.7) is an order of magnitude larger than for the quadratic model (4.2), and thus, the quadratic model is a better fit for the melt-rate dependency. The linear part becomes larger for smaller temperatures. A quadratic dependency is also clearly revealed when plotting the raw residuals against the fitted melt rates (Figure S3, right). We conclude, that the dependency of the melt rate to the AW temperature is quadratic, though, with a linear part, also indicated in the fitting equations 9 and 10 (Figure 5). We thus agree that our results displayed in Figure 5 are entirely consistent with the quadratic fits of Holland et al., 2007. However, several other studies report a linear dependency of the melt rates to

ocean warming for different ice shelves (e.g., Williams et al., 2002; Rignot and Jacobs, 2002; Shepherd et al., 2004; Payne et al., 2007).

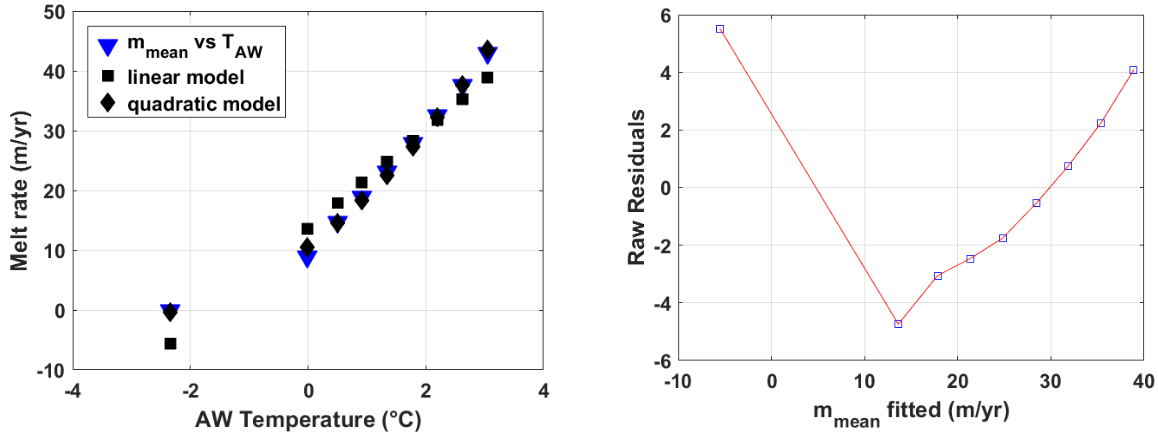

Figure S3: (left) Melt rate dependency on AW temperature based on ANOVA test. (right) Raw residuals plotted against fitted melt rates.

		t	df	p	sd	F	Residual sums of squares
Linear model (Tmean~ lmfittedMmean)	t-test	-5.5 (9.1, 10.4)	8 (7)	<0.001 (1.7e-05)	11.8		
	ANOVA		7	<0.001 (1.7e-05)		108	93.7
Quadratic model (Tmean ~ sqfittedMmean)	t-test	-5.3 (24.2, 37.08, 11.3)	8 (6)	<0.001 (2.1e-08)	12.3		
	ANOVA		6	<0.001 (2.1e-08)		1088	4.2

Table S1: Statistical test results for comparison between a linear and a quadratic relationship between mean melt rates and AW temperature. (linear model) The numbers shown within brackets for the t-values correspond to the intercept and T_AW. (quadratic model) The numbers shown within brackets for the t-values correspond to the intercept, T_AW, and T_AW^2. df means degree of freedom and is defined as the number of observations minus the number of independent variables minus 1.

We have also performed student's t-test in order to adhere to the assumption of linearity for the melt-rate dependency and due to the small data set. We used a significance level of $p < 0.05$. Test results are summarized in Table S1. We conducted all statistical analyses using the MATLAB ttest2 function. The t-tests reject the null hypothesis for both the linear and the quadratic model.

iv) … 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.

We have noticed that the reference to Holland et al., 2008 is wrong in our manuscript. We were referring to Holland et al., 2007. We will change this accordingly. We have tested the melt rate tendency on ambient temperature and find that the results are entirely consistent with the quadratic fit of Holland et al., 2007 as suggested.

5) Stability of the ice tongue

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.

These are valid points. The part on stability was not meant to be realistic estimates of the time until the 79NG "melts down", but rather a good illustration of the sensitivity of the melt rates to the AW temperatures. This section will be rewritten. Of the points above we have used the plume model to investigate changes in freshwater discharge i), and found this to be of minor importance with the present topography. We could further have investigated the effect of changes in shape of the ice shelf to address III), but have not done so as we agree that these estimates have meaning mainly to illustrate the present melting for the present topography and ocean forcing.

<u>More specific points</u>

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

Thank you for bringing this up. We found that the plume is not very sensitive to the amount of subglacial discharge, but did not show this explicitly in the manuscript. We have done simulations to address changes in subglacial discharge, and will add some text to describe the seasonality (Figure S4). We also investigated the effect of having the subglacial discharge distributed uniformly along the grounding line (GL) and as one single source (narrow opening, NO).

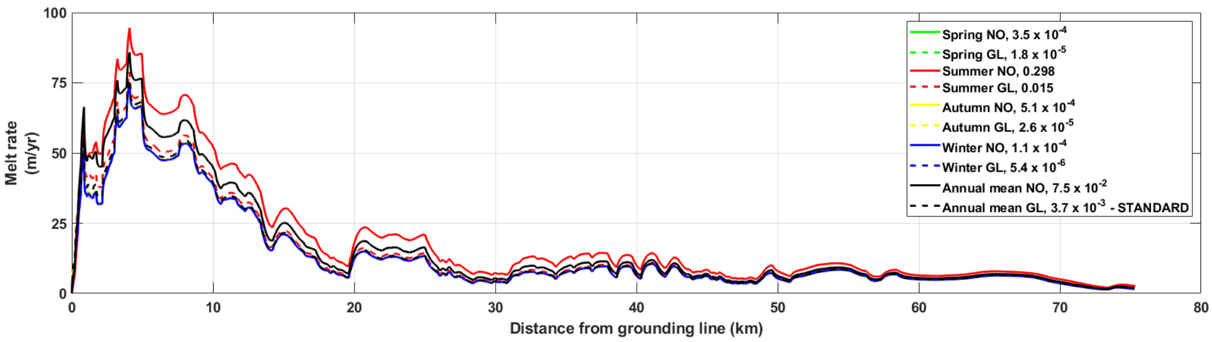

Figure S4: Sensitivity of the submarine melt rate along the centreline of the 79NG ice tongue due to the seasonality and distribution of subglacial discharge ranging from 5.4 x 10e-06 m²/s (winter, GL) to 0.298 m²/s (summer, NO).

General: are tides important?

Thank you for bringing this up. We have done quite a lot of work on the tides, but found that they overall are so small that they do not contribute much to melting (Anhaus, 2017, Master`s thesis, unpublished). In retrospect, we realize that this is an important result as well, and some new text on the tides will be included.

An Ice-Tethered Mooring (ITM) was deployed on a 1.35 m thick ice floe in a rift 15 km up-glacier from the northern terminus of the 79NG ice tongue during the ARK-XXX/2 (PS100) cruise on the R/V Polarstern (Kanzow, 2017) on August 23, 2016 at 79° 41.0 N, 20° 20.9 W. Four Aquadopp single-point current profiler from Nortek AS were attached to the mooring line at initial depths of about 165 m,

250 m, 370 m, and 500 m (https://www.whoi.edu/page.do?pid=154416). The measurements are averaged over 15 minutes. The data were collected and made available by the Ice-Tethered Profiler Program (Toole et al., 2011; Krishfield et al., 2008) based at the Woods Hole Oceanographic Institution (http://www.whoi.edu/itp).
The mean tidal velocity in the cavity was estimated to be 1.18 cm/s (Anhaus, 2017, Master thesis, unpublished) using the harmonic analysis package T_tide (Pawlowicz et al., 2002). The period October 21, 2016 to January 18, 2017 was extracted which gives a record length of 89.3 days, sufficient to detect all tidal constituents.
The tidal flow of 1.18 cm/s is too weak to contribute effectively to the melting and plume dynamics (velocity and thickness). This was concluded from applying no tidal flow as well as adding the tides in the plume model, and the results were similar (Figure S5). Tides might be low in the cavity of 79NG because of ice blocking the flow. However, this explanation is speculative at best.
Mortensen et al. (2014) performed a tidal analysis at Godthåbsfjord in West Greenland also using moored current meter measurements. Maximum tidal velocities were associated with the M2 and S2 component and 4 - 5 cm/s and 1 - 2 cm/s. The tidal flow in the cavity below the 79NG ice tongue is thus low compared to tidal velocities around Greenland. Moreover, tides are fairly barotropic (Anhaus, 2017, Master thesis, unpublished) and, thus, does not seem to influence the entrainment of AW.

In general, a stronger tidal flow would increase the shear between the plume and the ice-ocean boundary and, thus, the drag. This causes the plume to slow down and, as a result, less AW is entrained which lead to less melting. This response is supported by Smedsrud and Jenkins (2004) and investigated in Anhaus (2017, Master thesis, unpublished) for the 79NG (Figure S5).

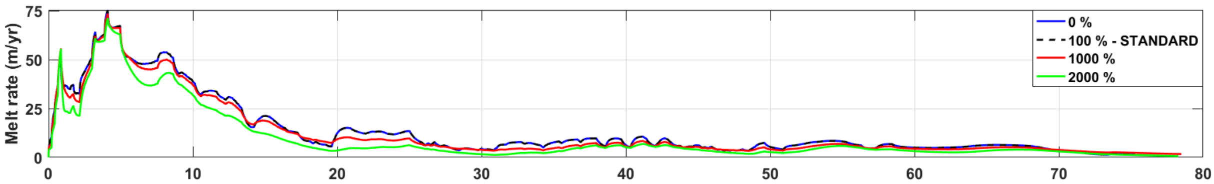

Figure S5: Sensitivity of the submarine melt rate along the centreline of the 79NG ice tongue due to the tidal flow in the cavity. The tidal velocity in the STANDARD case is 1.18 cm/s. Note that this are results from Anhaus, 2017 (Master thesis, unpublished) and here the STANDARD case has tides of 1.18 cm/s and a subglacial discharge of 1 x 10e-03 m²/s.

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

We will change the title of the manuscript as suggested.

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?

Our estimate of present 79NG melt fluxes were compared to the net mass loss for all of Greenland. We will now change to the new Bamber et al., 2018 that states an overall mass loss of 247 +/- 15 Gt/yr for the period 2012 – 2016, producing a net loss volume flux of 247 +/- 15 km³/y. Again this is to illustrate that the 79NG could be a large contributor to the overall loss, if the AW in the fjord have increased significantly over this period. The updated values gave the range 4-13% of the updated net volume loss, and this will be stated clearly.

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?

We think that some general aspects of Greenland fjords and glaciers are relevant also for 79NG, and that the longer term mass balance of 79NG is unclear. Given the observed temperature change in AW in Fram Strait, and our results here, we speculate that there has been a large mass loss, but it is hard to pin down given the available observations.

L26: The papers cited are not primarily observational.

We agree that the sentence was unclear, and it will be rewritten to: "This is suggested based on earlier observations (Jenkins et al., 2010; Straneo et al., 2010) and modelling work from other locations (Xu et al., 2012, 2013) and is also likely to happen at the 79NG."

L29: Distinguish between meltwater and glacial modified water?

Meltwater is pure freshwater, and glacial modified water is sea water that has melt water mixed in. This will be added to the text: "Both, pure meltwater and glacial modified water (sea water mixed with meltwater) were found in the cavity of the 79NG (Wilson and Straneo, 2015)."

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.

It is correct that ISW is used as an abbreviation of a water mass, and that this water mass is largely found in Antarctica, where the plume model was first applied (Jenkins, 1991). But this is also the name of the model that has been used for 28 years in the ice-shelf-ocean community. So it would just be confusing to the general reader to use a different name. An analogy could be "Greenland" – that was named by the Vikings 1000 years ago. It is clearly dominated by ice and not green, but it was named Greenland to attract Norse people to move, and we use that name in any regard.

P6

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

The depth of the 79NG is indeed modest compared to other ice-shelves, and the pressure depression is then also smaller. But the dependency of the slope is a result of the plume model, while the pressure effect is a part of the physical forcing. We will modify the sentence to: "Melt rates at 79NG are expected to be highest at the grounding line, this has a small contribution from the pressure and salinity dependence of the freezing point $T_b = \lambda_1 S_b + \lambda_2 + \lambda_3 D_e$, with $\lambda_3 D_e$ giving the ice base depth dependence."

P7

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

The paragraph will be rewritten for clarity: "The model simulates a depth-integrated steady state solution for all the variables listed in Table 1 (right), and produces results with values for these variables along the direction of the plume flow X starting at the grounding line (Figure 1). The results are obtained by solving the equations 1 - 6 for D, U, T, S, m, and e using 4th and 5th order Runge-Kutta formulas (Jenkins, 1991). The integration stops when the plume has reached its level of neutral

buoyancy or it reaches the ice front. Melting occurs when the plume temperature is above its freezing temperature."

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?

Yes, this is correct. If this was caused by melting by AW it would have remained on the meltwater mixing line. It is correct though that if there was a fresher AW source present in the cavity this could also be the cause. There is no such sign of fresher AW, and we state that this is "suggesting" this presence, so we think that this is fine as it is.

P10

L5: need to define the upper limit of AW

The AW properties were defined in the methods section (2.2 1D Ice Shelf Water Plume Model). However, as suggested by reviewer #2, a background paragraph on the hydrography of the 79NG area will be included in the introduction and here we now define the AW properties as used in our manuscript and also the PW properties.

figure 8 is cited out of order.

The sentence is obsolete and will be deleted: "The evolution of the ice base as a function of the along-tongue direction for the three line is shown in figure 8".

P13

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

The sentence will be rewritten to: "The estimate of the total final plume volume flux (the product of the final plume velocity U, the thickness D, and the 30 km width of the main front) is about 38 918 m^3/s (Table 3)."

L34: no iceberg calving?

Yes, correct. We have not found any estimates of calving, and to our knowledge, the ice tongue is protected from calving by the sea ice cover at the front (Reeh et al., 2001). Further, dominant ablation mechanisms for southern Greenland glaciers are in general due to calving (50%) whereas at Northern Greenland glaciers it is only 4% and the main mechanism for mass loss is attributed to submarine melting (Reeh et al., 1999).

P17

L2: The plume did not evolve? why not?

We have not been able to explain this properly, but suspect that there are non-linear interactions during the integration. All models have parameters that need to be within some range, and we have done extensive testing of the entrainment and drag coefficients. The sentence will be deleted, as the values are clearly shown in the table, and the text was not able to add any understanding here.

P20

L20: the entrainment rate is not constant.

That is a valid point. Figure S6 shows the entrainment rates for ECCOv4 and the STANDARD cases. We claim that the variability of the entrainment rate is insignificantly.

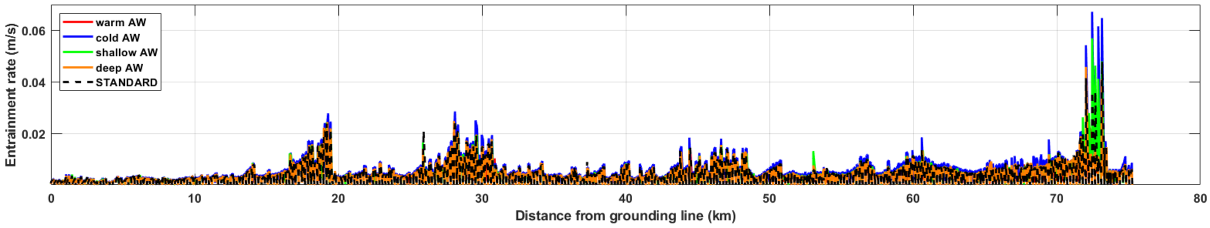

Figure S6: Entrainment rate simulated by the 1D ISW plume model using variations in AW temperature and presence in the water column in the Norske Trough from the ECCOv4 for the period 1992–2015. The STANDARD CTD profile is the rift profile (black dashed). Warm AW (red), cold AW (blue), shallow AW (green), and deep AW (orange).

We have tested a number of different entrainment coefficients, but the one we suggest is most correct for 79NG is the one in the STANDARD simulation. For the AW cases (Table 3) the entrainment rate has this value, and is constant at 1.8 10e-02. (Table 2).

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