Dear Anonymous Reviewer #2,

Thanks for your invested time in reading our manuscript and for your many and detailed constructive suggestions. We hope that you will find the new and improved version of the paper satisfactory. Our response is written in bold font directly below your original comments in the text below. We have responded to all suggestions, followed most of them, and clarified the issue if required.

We will made an attempt to shorten the paper, but were also told by reviewer #1 to add some text on the tides and seasonal changes in subglacial discharge and AW temperature. These additions hopefully contribute to the novelty of our work.

Best regards,

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

Major points

1) Ice-tongue melt-time projection

The ice-tongue melt-time projection neglects all feedbacks in this glacier fjord, including ice dynamics (evolving ice flux across the grounding line, and shape of the cavity), iceberg calving, evolving ocean circulation and wind patterns (and sea ice, if relevant) outside the fjord. The projection also does not take into account, rotational effects, dimensionality of the problem, and temporal changes inside the fjord - I would expect at least seasonal AW temperature/thickness (and presence), and seasonally distributed subglacial discharge to play some role here. As a result, I am not sure the provided ice-tongue melt-time projection has much meaning. If not a coupled glacier-ocean model, I think at least a glacier model should be involved, to produce a somewhat more robust statement about the 79NG stability at present and in the future.

We agree that the melt-time estimates neglects a number of the processes listed above, and we only calculated this to illustrate the sensitivity to ocean temperature in an easy-to-understand way. The paragraph will be rewritten to make this more clear. We will include new runs that includes seasonality in subglacial discharge and AW temperature/ layer thickness. These are both highly uncertain. The subglacial discharge seasonal variation has an overall small contribution, and this is also the case with the seasonality in the AW temperature/ layer thickness as observed with an Ice Tethered Mooring (ITM, Figure S6).

The 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 (http://www.nortekusa.com/usa/products/acoustic-doppler-current-meters/aquadopp-current-meter-brochure/view) 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 including temperature were 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).

2) New insights

The main focus of the manuscript is on assessing sensitivity of the plume model to its parameters - but I am not sure it provides any new insights or conclusions. I am also not sure it provides any new insights on the processes driving submarine melting - but is it possible that I missed it - it would be helpful if the authors clarified what the contributions are and how they differ from previous studies. Here are a few studies (not referenced here) that have done this before and more exhaustively: Carroll et al 2015, and 2016, Sciascia 2013, Beckmann et al 2018. As it stands, I think the presented plume model application to 79NG is within the parameter range studied previously. If not, it would be good to clarify that. The main result (linear scaling of melt rate with AW temperature) is consistent with other studies in Greenland glaciers, and as mentioned even in the discussion here it maybe more of a property of the plume model itself, than anything else.

The main goal of the paper is to estimate the sensitivity of ocean driven melting of 79NG. But to find this sensitivity we had to find a realistic set of parameters. We agree that the main finding appears to be consistent with earlier studies, but this is a good thing, because there are so few observations available of 79NG. So, despite the limited observations, results appear sound. We have further found that melting is in-sensitive to a large range in subglacial discharge (from RACMO2.3p2), and that tides are likely too slow to contribute much to the melting. Overall the sensitivity to AW temperature (from ECCOv4) seems large, and the present AW presence already melts the 79NG quite effectively.

We are thankful for the suggested new references that we have investigated and some will be included in the new version of the manuscript. They largely describe comparable results, but for other areas, or in a more general way using parameters not specific for the 79NG configuration.

3) References

The manuscript is not very carefully referenced. Although there are a lot of references, the choices are sometimes quite arbitrary. Given this is primarily a sensitivity study of the plume model - it should be clear how the findings here differ from other (often more complete and insightful) sensitivity studies of the plume model. Modelling studies are at times used as references where one would expect a reference for observations.

We are sorry to learn that you find other studies more complete and full of insight, but hold that we have also found some interesting results that will be useful. We will update the citations in many places, and include the results on tides and seasonality in subglacial discharge and AW temperature/ layer thickness – so the novelty should be improved.

4) Structure

I feel the manuscript is written quite confusingly and could use a bit of reorganization. Description and interpretation are often mixed without a clear distinction. Although I have quite a few in-line comments and clarification suggestions below, I feel that for what the manuscript does, it could be half its length, and more to the point. A lot of the confusion arises because of the poor organization. Here are some suggestions for restructuring:

*Include a background section (could be part of introduction) - where all relevant information about the region and glacier is summarized and refer to it whenever necessary, instead of giving background throughout the manuscript, sometimes repetitively, sometimes not at all.

Thanks for the constructive suggestions on improving the structure of our paper. We will try to follow your suggestions by moving the first paragraph of the results chapter which addresses the hydrography at the 79NG (Wilson and Straneo, 2015) to the background in the introduction.
Otherwise the background in the introduction is fairly short and contains the relevant information. The rest is methods, new results, and discussion of our results. We will attempt to delete some details about the model parameters, and focus more on the results in the present version.

*Clearly describe the experiment setup in the methods section, motivate and justify these experiment choices and clarify what you are trying to achieve. State all the assumptions in this section. At present, while there is discussion of sensitivity to some parameters in the end, there are many model choices for which there is no reasoning/explanation provided

These are indeed general and good rules to follow, and we will follow most of the specific suggestions. We will merge the content of Table 2 in Table 1 in the methods section in order to describe all model parameters in this section instead of having this separated. The "problem" arises here because some model "choices" are indeed results, and needed some discussion. What we have provided in the methods section is the model equations and the STANDARD values for the model parameters. These are our best choice values, and were used for the sensitivity of melt rates towards AW temperature – the main focus. Our best shot at the mean situation is presented in section 3.2 (STANDARD case), and the sensitivity to the ocean forcing in section 3.3 (ocean forcing sensitivity). The detailed testing of the parameters will be shifted to section 3.4 (parametrization). It is probably this section that is thought of as "model choices" – but these are truly unknown, and we had to do extensive testing to find values that we think are representative.

*Describe the relevant part of the results (I think currently the result description is quite long, given it doesn't provide that much new insight)

Hopefully people have different interests, and some will find the results regarding plume speed, plume thickness, entrainment rates, drag coefficients, temperature evolution and topographic control for 79NG interesting. No such results are available for 79NG, until now. We agree that the melt rates are probably the most interesting though and some were already presented in Schaffer (2017) and Mayer et al. (2018) but not the extensive testing of the coefficients, the subglacial discharge range we will update now, and the tides we will now describe. The paper is quite compact, and only has 8 figures. Four of them present the melting results. We also have only 3 tables now.

*Explain why the results (resulting melt rates in this case) are believable for the base case (present), and only then move on to the results for the future case.

We will add a direct comparison with satellite-based melt rates in the new version. However, these were cited in the beginning of the discussion section (4.1). In our view the results section should only present our results, and then these should be compared to other work in the discussion part. Perhaps the suggestion here is to merge results and discussion – but we think the normal and best way is to have them separated. We will now include direct comparison between our melt rates from the plume model and the satellite derived melt rates from Wilson et al. (2017) in Figure (4a). This was also suggested by reviewer #1. We do not do 'future' cases, we only present a sensitivity towards AW temperature.

*To discuss the future warming scenarios, I think some sort of a model would be needed (see points 1)

We have used the ECCO simulations to extract variability in AW properties, and we have used the plume model to estimate the melt rates given a range in these, so we have used two models. Perhaps a glacial, or ice-shelf model is meant here, which we have not used. We will rewrite the "melt-time" part – to clarify that we are indeed not presenting realistic scenarios for future changes in shape of the 79NG.

<u>In-line comments</u>

Abstract:

L7 - decay of what? 20 km from the grounding line?

Will be changed to: "Melt rates drop rapidly to 6 m/yr within the first 10-20 km from the grounding line."

L10 - why is the melt rate sensitivity reported along a centreline when just a few lines above the melt rate is divided into three sections? Also, is this range of melt rate or melt rate increase?

We think that both the maximum and mean melt rates are worth mentioning in the abstract. This will be changed to: "The melt rates increase quadratic with rising AW temperature, and overall mean melting changes from 10 to 21 m/yr with the changes in ocean forcing."

L13 – In which way does the manuscript improve the understanding of processes driving submarine melting of marine-terminating glaciers around Greenland? This is the place to be specific.

Agreed, will be changed to: "Our results show that submarine melting of the marine-terminating 79NG is sensitive to changes in ocean temperature."

Intro:

P2:

L5 - general statement - doesn't need a reference.

We will delete the reference here.

L6 - Holland 2007 seems a more appropriate reference here, than this review paper.

We will change the citation accordingly.

L7 - Do you mean net mass loss increase? Also, this sentence seems to contradict a previous sentence (P1 L20-22: The enhanced mass loss is caused by increased surface melt, and retreat and speed up of marine-terminating glaciers (Enderlin et al., 2014))

Yes, will be changed to: "The net mass loss increase of marine terminating glaciers is believed to be caused by increased melting at grounding line depth, leading to inward migration of the grounding line and accelerated glacial flow (Thomas et al., 2009).

L8 - The increase of submarine melting.... not the submarine melting itself - ...leads to an inward migration..... A stable glacier can still have submarine melt.

Yes, will be corrected as stated above.

L9 - "It is important to study submarine melting since it is a likely trigger of change of ice loss from the ice sheet." Again, the presence of submarine melting is not a trigger of change, it is the change of submarine melt that may act as a trigger of change.

Yes, correct. This will be changed to: "It is important to study submarine melting, because an increase could trigger increased loss of the ice sheet."

L14 - buttressing is defined usually at the grounding line, not at the terminus, could you specify what you mean by buttressing at the terminus here?

Suggestion will be followed: "at the terminus" will be deleted.

L26 - Are these references supposed to refer to observations of melt/discharge driven plumes? Two of these are models, not observations

Sentence will be rewritten, also suggested by reviewer #1.

L27 – which of the two scenarios are likely to happen at NG? and why is it likely? The majority of the subglacial discharge is most likely released at depth —- are you referring to 79NG specifically here? again, why is it likely? is there any support for this, or is that an assumption (which is completely fine as long as it is clarified) based on observations elsewhere?

There is in general very few observations available for 79NG, and then we have used the word "likely" to state that things are "probably" similar here to other Greenland areas. Subglacial discharge is hard to observe in general, so this is also primarily unknown in general.

L29 - This is another awkward choice of reference. There have been plenty of earlier studies characterizing channelized network under ice tongues/ ice shelves.
Since Dallaston does not relate to 79NG or even Greenland specifically, I don't understand the choice for this particular reference here as opposed to earlier ones.

We found this to be a general good description. We will also cite Rignot and Steffen (2008) and Millgate et al. (2013) which both investigate channelized bottom melting at the floating ice tongue of Petermann Glacier in North Greenland based on observations (Rignot and Steffen, 2008) and the model MITgcm (Millgate et al., 2013).

L33 - Isn't it 50% calving and 50% submarine melt? The wording here suggests that 50% is from these two together and the remainder from something else.

This addresses Greenland, so the remaining part is the surface melt.

L34 - This manuscript really overuses the word likely. It would help to clarify what is known (reference), what is speculated (reference), and what is assumed for the purposes of this study.

As noted above is there often no specific observation available for the 79NG. We agree that it is good not to use the same phrases, and we will use a different term where this is just as correct.

P3:

L5 - In my view this is more of a sensitivity study, not a process study

Yes, we agree. This will be changed to: "The main goal of this study is to address the sensitivity of submarine melting of 79NG to changes in ocean forcing. However, to do this we also needed an improved understanding of the ocean processes below the ice tongue".

Data:

P3:

L20 - What is a high-res digital bathymetry model and how does it compensate poor data coverage?

Will be changed to: "The poor bathymetric data coverage ..." So what is meant here is that the additional CTD profiles, echo-soundings and model make the bathymetry better.

L18-23 - Bathymetry - did you do this data merging here for the purpose of this manuscript? If so this needs to be described in much more detail. If not, it would be good to first write what product is used in which part of the MS, followed by a brief description what this product consists of.

The plume model uses only the ice base data, but we also plot bedrock and ice elevation in the figures, so we actually use all of it. We will add this: "Bedrock, ice elevation, and ice base along the centreline of the 79NG ice tongue are shown in Figure 1 and spatially in Figure 2b, but it is only the ice base that is directly used by the plume model."

L24 - This third paragraph logically follows from the first one, not from the second one as it refers back to the ice, so perhaps rearrange. Also please explain the choice of the spatial filter, and how it guarantees an increase of the ice base, or if this was then enforced by some other procedure.

The lines 24-27 will be moved up and merged with the first paragraph of the section.

L25 - How were the plume paths chosen? If it is discussed later perhaps reference the section.

Will be changed to: "Data points were manually extracted and smoothed by a moving average (boxcar filter) every 5th data point along the three selected plume paths along the centerline, northern and southern sides of the fjord."

P4:

L1 - Which tidal effects? Do you include those here or you use the basic version?

Will be changed to: "Frazil ice growth and precipitation, as well as increased vertical shear from tidal currents was included by Smedsrud and Jenkins (2004). This was tested for the 79NG as well, but no super cooling is produced, and there is no frazil ice dynamics."

Tidal effects were investigated in Anhaus (2017, Master's thesis, unpublished). The mean tidal velocity in the cavity was estimated to be 1.18 cm/s using the current data collect by the ITM and 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 S1). Tides might be low in the cavity of 79NG because of ice blocking the flow. However, this explanation is speculative at best.

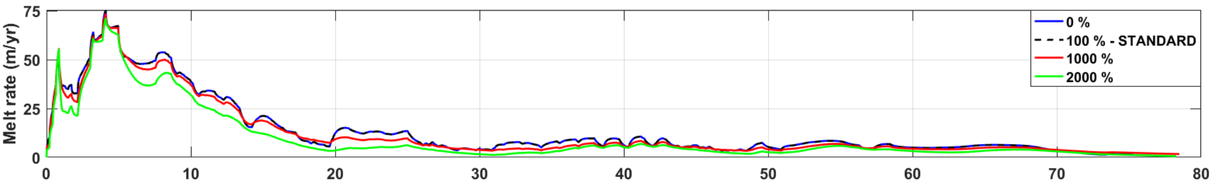

Figure S1: Sensitivity of the submarine melt rate along the centreline of the 79NG ice tongue due to the tidal flow in the cavity. The tidal velocity in the STANDARD case is 1.18 cm/s. Note that this are results from Anhaus (2017, Master's thesis, unpublished) and here the STANDARD case has tides of 1.18 cm/s and a subglacial discharge of 1 x 10e-03 m²/s.

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's 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's thesis, unpublished) for the 79NG (Figure S1).

L5 - Why is STANDARD capitalized? At least at this point of the manuscript this is not at all clear.

We chose to capitalize STANDARD to make it clear that this is a model simulation and not the regular meaning of the word. We will add this: "We use the term STANDARD to name the set of values used in our regular model simulation, so the STANDARD value for subglacial discharge used is 4.0 x 10e-03 m²/s (Table 1).

L5-7 - This part is a bit rushed, could you be a bit clearer on how you derive the subglacial discharge, and what assumptions go into the derivation. Further, have you considered separating the summer and winter case? Presumably the subglacial discharge is very seasonal and unless the plume model depends linearly on this parameter, using a long term annual mean might over- or under-estimate the melt.

In an effort to produce a compact and easy to follow paper we postponed the details to section 3.4. We have done simulations to address changes in subglacial discharge, and will add some text to describe the seasonality (Figure S2). 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). As it turns out the melting is quite independent of a large range of this forcing.

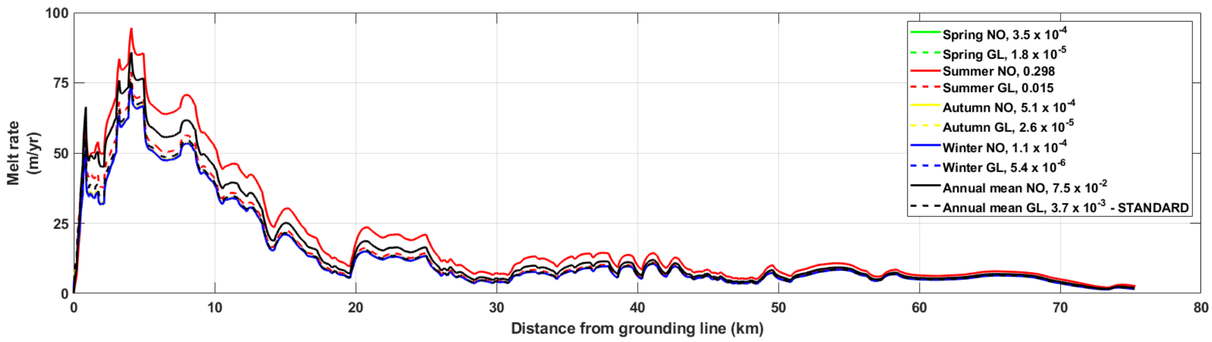

Figure S2: Sensitivity of the submarine melt rate along the centreline of the 79NG ice tongue due to the seasonality and distribution of subglacial discharge ranging from 5.4 x 10e-06 m²/s (winter, GL) to 0.298 m²/s (summer, NO). The STANDARD value for subglacial discharge used is the long-term annual mean 4.0 x 10e-03 m²/s from RACMO2.3p2.

L9 – I am not sure I am familiar with the terminology "line source equation" could you clarify what this term means?

We will delete "line source" in the text, it is not required.

L9-10 - it is the quantities, not the fluxes of the quantities that are conserved.

Indeed correct, we will delete "fluxes of" here.

P5: How is the "re-circulation" and "the southern and northern recirculation in Fram Strait" shown in the figure?

The re-circulation is the flow in Fram Strait that turns southwards, one branch in the north, and one in the south.

P6:

L7 - What value do you use for the ref. density? I don't see it in the table.

The reference density of 1028 kg/m³ will be added to Table 1.

L8 - Clarify that initial doesn't refer to initial condition as there is no time dependency in the equations.

Will be changed to: "The initial start temperature T0 and salinity S0 of the plume …"

L9 - Why is T0 set to freezing point? Does the result depend on different values of T0? What is S0 set to?

This is meltwater flowing alongside the glacier, so it must be at the freezing point. Will be changed to: "Here, T_0 is set to the freezing point, and S0 is zero as this is pure meltwater".

L11 - Melt rates at 79NG are .... This statement is not specific to NG, or is it?

Will be changed to: "Melt rates at 79NG as most other ice shelves are expected to be highest close to the grounding line".

L24 - Do you have any reference that -15C is reasonable, or how sensitive is the result to that?

This is a small part of the heat budget, so we have chosen not to do specific tests on this forcing. It would be easy to do though, but it represents a mean surface temperature during snow accumulation.

General - I think it would be clearer if the plume model was first presented in general, and only after all the concepts are introduced, you can introduce specific choices for 79NG and justify how appropriate they are. Constantly switching between these two makes it very confusing.

This is indeed what we aimed to do; present the model and the normal parameters in this section. The values that needed specific testing and were unknown for 79NG are tested in section 3.4 and discussed in section 4.3 and 4.4.

P7:

Table 1 - the values used for the constant, where are they taken from? – reference

Most values are the same as used by Jenkins (2011) unless otherwise stated. This will be added to the table caption now: "Physical constants (left) and output variables (right) of the 1D ISW plume model. Unless stated otherwise constants are the same as in Jenkins (2011)."

L6 - Having determined that rotation is important in this fjord, how is it taken into account? As far as I am aware applying 1D plume model to 3 different paths, does not deal with rotation effects - but that is what line P7 L9-11 seem to suggest.

The results are quite similar for the southern and central path, indicating that the buoyant outflow leans up against the coast to the south over a large area. In the north melt rates are much lower, but

this is caused by the very flat profile (Figure 8d, e). We describe these differences at the end of section 3.3 and discuss them in section 4.4.

L8 - is 2-layer an assumption, or is it an approximation based on observations?

The two layer assumption is here just required for calculating the Rossby radius, this will be made clear now: "To calculate the first baroclinic radius of deformation R … the stratification is assumed to be a two-layer system consisting of PW overlying AW with densities …".

L9 - define f when it is introduced not several lines later

Agreed. The first line after the equation will be changed to: "f is the Coriolis parameter, and the required water column thicknesses are the upper H1 (90 m), lower H2 (470 m), and total H (560 m) values from the CTD profile in the rift."

L11 - The differences between ... it seems like this belongs more to discussion/results than here.

This result justifies the use of mainly using one plume path for the sensitivity.

L13 - Since there is no dependency, aren't all variables diagnostic?

The paragraph will be rewritten in response also to reviewer #1.

P8:

L4 - I don't understand this what ....with S instead of Sb refers to

This refers to the equation in the text without a number for T_b. The whole paragraph will be rewritten also in response to reviewer #1.

L5 - can you show that ECCOv4 does a reasonable job in this region? are there any data to constrain it here, if so, how well does it match them, if not, why do you think this coarsely resolved model represents well the water masses relevant to your computation?

Yes – all available observations have been used to constrain the ECCOv4. ECCOv4 assimilates observations and thus simulate hydrography better than a "normal" model, and model drift might be avoided (Forget et al., 2015). However, data coverage is sparse, especially on the continental shelf of Northeast Greenland and at 79NG (Figure S3). The data constraints are Argo floats (IFREMER), CTD profiles (NODC, WOA09), and moorings.

In Anhaus (2017, Master's thesis, unpublished), ECCOv4 was evaluated with CTD profiles collected across Fram Strait (FS2016) and on the continental shelf of Northeast Greenland (Figure S4) and investigated annual and monthly (August and September) variations in AW temperature, salinity, and layer thickness. The CTD profiles were collected during the FS2016 cruise with R/V Lance organized by the Norwegian Polar Institute in August/September 2016 which the first-author participated.

ECCOv4 reproduces the location of the maximum temperatures in the surface layer offshore from Spitsbergen and the cooling of AW across Fram Strait is reflected (not shown). The modelled East Greenland Current (EGC) in the upper 100 m to 200 m is similar to the one observed during FS2016 (not shown). The large scale eddy observed during FS2016 offshore the Continental Shelf (not shown) is not found in ECCOv4. It is possible that eddies are not resolved in ECCOv4 due to the coarse

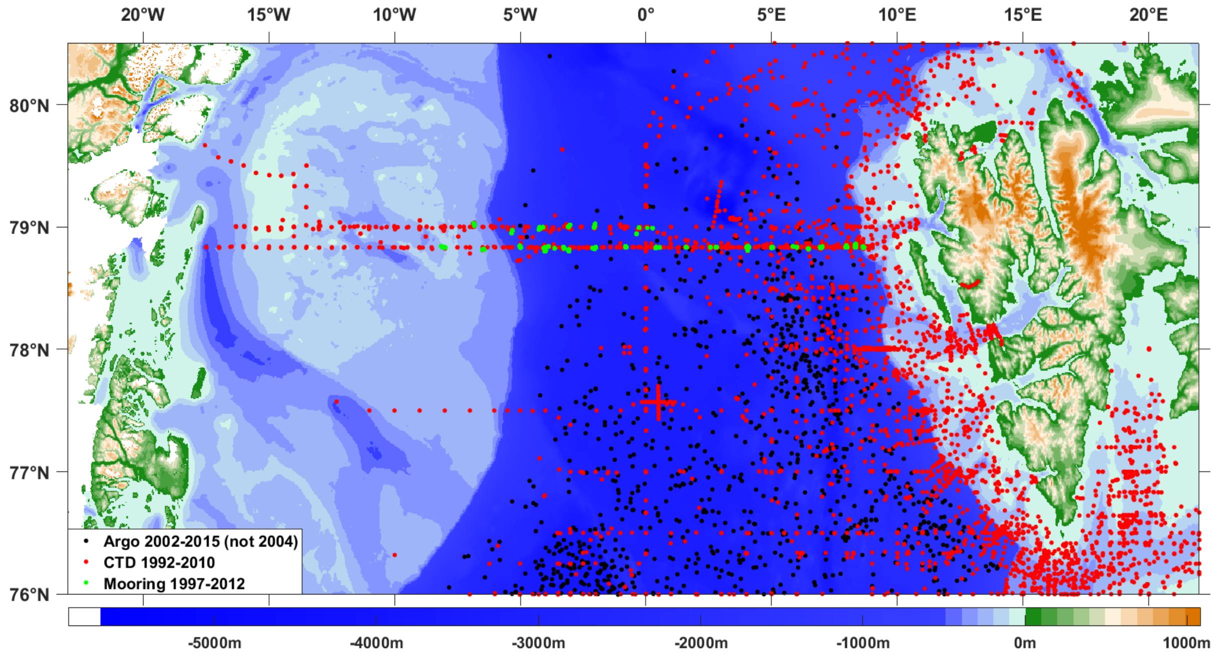

Figure S3: Location of hydrographic observations which constrain ECCOv4. Argo floats are indicated in black, CTDs in red, and moorings in green. Bedrock and surface mask are the RTopo2 (Schaffer et al., 2016).

grid. The horizontal spacing of the CTD profiles is about 40 km (Figure S4 and Forget et al., 2015). The first baroclinic radius of deformation is about 2.6 km in the study area (Nurser and Bacon, 2014). Thus, eddies are not resolved but parameterized. However, Hattermann et al. (2016) found the eddy kinetic energy exceeding the mean kinetic energy in winter and, thus, the westward mixing of AW properties due to eddies being strongest. The ECCOv4 section shows the mean situation for 24 years. Hence, eddies occuring during one season might be eroded by the mean flow. The situation during FS2016 might have been extraordinary and, therefore, eddies might have survived influences from the EGC and winters. In contrast, profiles during FS2016 where the eddy is found are denser (Figure S4) and, thus, eddies are resolved.

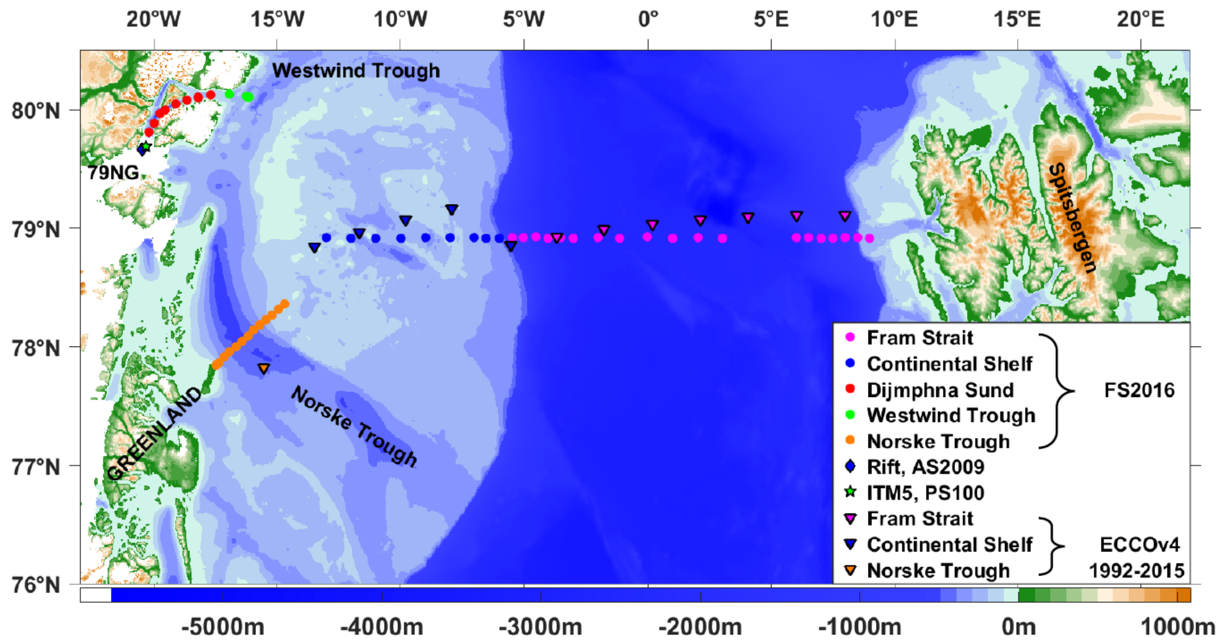

Figure S4: Overview map of the location of all hydrographic data collected during FS2016 and previous surveys. FS2016: Fram Strait (magenta), Continental Shelf (blue), Norske Trough (orange), Dijmphna Sund (red), Westwind Trough (green). Wilson and Straneo, 2009: Rift (blue). Straneo et

al., 2016: Ice-Tethered Mooring (ITM5, blue). Estimating the Circulation and Climate of the Ocean version 4 state estimate and ocean model (ECCOv4) September 1992-2015: Fram Strait (magenta), Continental Shelf (blue), Norske Trough (orange). Bedrock and surface mask are the RTopo2 (Schaffer et al., 2016).

The ECCOv4 yields a range in AW temperature of 0.1 °C to 1.3 °C at 300 m depth in the Norske Trough between 1992-2015 for both August and September (not shown). This gives an upper temperature limit of 1.3 °C for AW that can potentially flow into the cavity beneath the 79NG ice tongue. This limit is comparable with the observations during FS2016 in the Norske Trough (not shown). The cold halocline between 50 m and 100 m in ECCOv4 is very similar to the observations taken during the FS2016 (not shown). Moreover, it is present every year making it to a constant feature in the Norske Trough (not shown). This has major consequences for the heat loss of AW which is assumed to be rather small toward 79NG.

We decided not to include these comparisons as it would require additional text and figures in the manuscript, and it does not seem essential. We still think this is the best solution, especially as it was suggested to further compress the manuscript.

L7 - Forget et al., 2015 - Another incorrect reference. Also, this reference is not even listed under the "References" section.

Indeed our mistake, the citation will be added now. We will also include Forget (2010).

L13 - Again, I don't follow why it is reasonable to consider a long-term mean for the value of subglacial discharge. At a Greenland glacier like NG79, subglacial discharge will vary seasonally, if it has a significant surface runoff component. I think that would only make sense if the basal melt scales linearly over the range from 0 subglacial discharge (winter) to max subglacial discharge (summer) - is that the case at NG79?

This was also brought up by reviewer #1, and we will now describe the seasonal change in subglacial discharge in a little more detail. Overall it does not affect the melt rates to a large extent (Figure S2), because this influence is dominated by entrainment of AW downstream from the grounding line.

L14 - Could you please give more detail on the surface run-off calculation.

Some text will be included in the methods section to describe the surface run-off calculation, but this is kept very short and the reader is referred to the references for more detail. Further, some details are discussed in section 4.4.

L15 - Could you please provide reasoning for the assumption of equally distributing the flux?

This is the simplest possible assumption when it is basically unknown. This is also discussed in more detail in section 4.4.

Results:

P8:

The first part of results is more of an introduction - the data and water masses discussed here are presented elsewhere, so these aren't quite "results" of this paper.

We agree, and this was also suggested by reviewer #1. We will move this to a paragraph in the introduction section.

P9:

Did you use just one horizontal grid cell profile from ECCO or some spatial average (which would be more robust)? Why did you choose this particular grid cell? It does not seem to be so well justified, because water there is saltier than in the observed CTD profile (as shown in Fig. 3c). Why is the chosen ECCO grid cell so far from the observations, how coarse is the model resolution here (km) - can you show ECCO bathymetry for comparison?

We have examined the spatial variability in ECCOv4 quite substantially. This was done in Anhaus (2017, Master's thesis, unpublished), and showed reasonable spatial variability as described above (Figure S3 shows assimilated observations). Based on this evaluation we decided to include seasonal means that should be robust because the average over several months (Figure 3a and b). The values vary quite a lot spatially because of the nearby EGC and the topographic flow control, so we used the most representative grid cell for 79NG in our view.

P10:

L5 - what do you mean by "not significant in mean"?

The seasonal variations are within one standard deviation, this will be added now: "Seasonal variations in AW temperature are small and within one standard deviation (Figure 3a).

L7 and elsewhere - clarify when you are talking about observed and when about modeled AW.

All of this paragraph regards simulated AW. We will change this to: "The annual mean simulated AW temperature at 300 m depth varies between 0.1˚C and 1.4˚C (not shown), and the upper extent varies by 65 m; from 195 m to 260 m."

L8 - Is the AW protected from heat loss in the winter as well? Does sea ice form in this area, and is it possible that it transforms AW seasonally, in the winter - something that the model with this resolution might not resolve well?

Yes – good thinking. Variations in both temperature and salinity in the upper 50 m of the Norske Trough are very likely driven by sea ice formation and melting. As a result, the surface layer salinity increases during winter from brine release during ice formation, and the mixed layer deepens. On the other hand, during melting freshwater is added to the surface layer, leading to a more stratified surface layer that acts as a barrier towards the warmer underlying AW. The ECCOv4 model does a good job in simulating these processes as shown in Figure 3a and b, but it only affects the upper 50 m. AW is only present at 195 m depth. Between 50 m and 100 m there are still seasonal variations in salinity but it is not AW salinity.

L10 - why three flow lines?

The fjord is 20-30 km wide, and the baroclinic radius of deformation is 9 km, so this seemed as an appropriate number to resolve the cross-fjord spatially.

L15 - introduce figures in their numbered order

Will be changed, also required by reviewer #1.

L22 - do you mean retreat?

No – we mean disappearance. But we realized that this needs to be rewritten in order to fully describe that this is a way to illustrate present ongoing melt – and not a realistic estimate of total melt. This part will be rewritten (section 3.5 and 4.5).

General - How does the thickness of AW change through time? Is there any evidence of AW in the cavity all year round? Is it possible that AW is present in the cavity only in the summer (e.g. due to heaving isopycnals?), what controls the renewal/circulation on this fjord.

Good questions – the answers are not straight forward. Some of these might be answered over the next year or so when the second author (Lars H. Smedsrud) will work at Scripps and dive properly into the AW variability along the East Greenland coast. There is evidence of AW in the cavity though, as shown in Figure S6 below. We suspect that most of the AW variability, including depth, is wind driven. In any regard is this the focus of another paper.

L18 - Optimal in which sense? how did you determine that?

These were found through extensive testing, and the essential description is found in section 3.4. This was done to make the main findings and story more clear. Will be changed to: "First, results obtained using our best estimates of model parameters for the 79NG are described ..."

P11:

L2 - Motivation/justification of model parameters should be in the methods, or early on, not in the end after the results have been described.

We have given the values and shortly described those values in the methods. What we include towards the end of the results section is a sensitivity study of the two parameters that cannot really be based on observations or earlier simulations. We did it this way to increase readability of the paper.

L2 - result not results

Yes, will be changed.

L5 - 10 km....looks even within first 2 km to me
L6 - what is the definition of GLZ here?

Will be changed to: "The plume quickly entrains AW causing the temperature and salinity to rise to a maximum within the first few km – the Grounding Line Zone (GLZ, 0-10km) "

Fig 4, are the jumps in velocity and thickness simply a function of the ice base profile or its derivative - can you add the relevant quantity to the plot here? I see now it is plotted in Fig. 8, but here it would be appropriate as well since this is the first case discussed.

The ice base profile of the STANDARD simulation has been shown already in Figure 1. Many of the plume properties are controlled by the slope, but it develops also by non-trivial processes as melt, entrainment and the ambient stratification. We have described this in section 3.2 as well as we can.

P12:

L1 - gradually approaching -2.0 C (Fig 4d shows the limiting T to be more like -1.7 or -1.8 C)

This is a very similar statement. Will be changed to -1.8˚C.

L2 - In contrast is the density contrast (reword)

Will be changed to: "On the other hand is the density contrast between the plume and surrounding AW quite constant"

L7 - what is the reason for the sudden increase at _72 km?

We have not investigated this in special, because it is at the end of the plume path and the melt rates are low. At 72 km the density contrast increases, velocity decreases, and thickness increases.

L8 - reference an equation that implies more AW entrainment with higher velocity, since the amount of entrained AW is not plotted. Same for next sentence.

The entrainment depends linearly on the velocity as given in the equation in section 2.2 (no number). Figure S5 shows the entrainment rates for ECCOv4 and the STANDARD cases. We claim that the variability of the entrainment rate is small and insignificant.

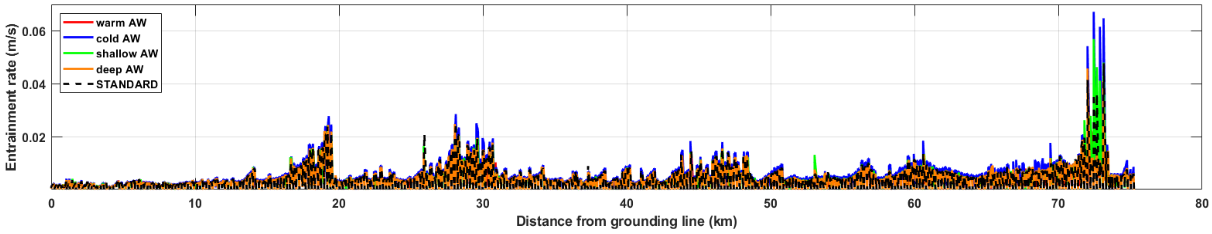

Figure S5: Entrainment rate simulated by the 1D ISW plume model using variations in AW temperature and presence in the water column in the Norske Trough from the ECCOv4 for the period 1992–2015. The STANDARD CTD profile is the rift profile (black dashed). Warm AW (red), cold AW (blue), shallow AW (green), and deep AW (orange).

L12 - ....the density contrast continuously decreases as a result of less melting - Fig. 4a doesn't quite show a decrease of melt part 30 km, in fact the melt rate is more less constant past this distance.

Agreed – just as well to compress here. Will be changed to: "Down-glacier of 30 km, the density contrast decreases (Figure 4b)."

P13:

L8 - accelerates linearly do you mean that the acceleration is constant over the first 5 km? There are still jumps in the velocity

Will be changed to: "The plume speed increases close to linearly within the first 5 km due to a …"

L9 - Besides three local minima – which ones? I see lots of (>3) local minima on the quite rugged velocity plot

Will be changed to: "Besides three local minima (20, 30 and 48 km) where the plume velocity decreases quite substantially the plume maintains …."

L11 – What is the reason for the velocity decrease there? L11 - "Small scale variability is due to the ice base." Clarify that point earlier, when discussing melt rate already - since the small scale features are already visible there.

The velocity variations (increase and decrease) are due to the rugged ice base. The variations are larger in speed, but we have indeed described this earlier in the section: "The overall result is that the ice base changes laterally and longitudinally and determines a spatially varying distribution of the melt rate and plume dynamics."

L17 - a 2D or 3D concept .... the units suggest that you are extrapolating the result to 3D

The sentence will be rewritten, also suggested by reviewer #1: "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 38 804 m³/s (Table 2)."

L18-21 - first introduce/justify the experiments, then describe them

We agree that this short description of the entrainment rate, drag coefficient, and subglacial discharge belongs in the methods section. It will be moved there.

L25 - At the end melting variability - at the end of what?

Will be changed to: "At the end of this section melting variability across the ice tongue is assessed …"

L31 - why 0.5C?

This is based on the ECCOv4 data and the observed warming of 0.5°C in the Norske Trough from the period 1979-1999 to 2000-2016 (Schaffer et al., 2017), will be adde: "the AW temperature in the cavity (rift profile) was increased by 0.5˚C based on the ECCO profiles (Figure 3) and the observed warming in the Norske Trough from the period 1979-1999 to 2000-2016 (Schaffer et al., 2017)."

L34 - ... and outflowing flux down-glacier? Or do you just mean flux across the grounding line?

Yes, nice and short. Will be changed to: "… ice_{flux} the flux across the GL".

P14:

L1 - the ice flux is probably quite spatially variable (3D) and possibly also varies seasonally. Using a bulk value is not very well justified. Alternatively, a use of glacier model would be more appropriate to assess the ice tongue stability.

We are indeed aware that there are spatially varying ice fluxes, and we make no attempt to address details in glacial flow. We only attempt to present our melt rates based on ocean forcing in comparison to the bulk values of the glacial mass balance. Section 3.5 and 4.5 will be rewritten to incorporate this better now.

L2 - in a submarine melting of about 4.2 m/yr - 4.3 no? This whole section is very confusing and involves a lot of hypothesizing, and if anywhere, it probably belongs to discussion, not to results (that is the description of the outcome of the experiments) I missed where equations 9 and 10 come from, what they mean, and what they assume

Eq. (9) and (10) are our best fit for melt rates based on the results of the plume model. The other "hypothesis" are just made to illustrate the melt rates, and find the steady-state melt rates for the 79NG to compare our results against. Comparison to other melt rates are indeed found in discussion.

P15:

L5 - because ECCOv4 does not have an ice tongue - this is not a justification for your assumptions.

Will be changed to: "ECCOv4 does not have a realistic 79NG ice tongue, so we apply hydrographic properties from the nearby Norske Trough which compare well to recent observations (not shown)".

As mentioned above - is there any evidence that AW is in the cavity year-round? Section 3.4: Can you comment on if distributed plumes would be a better model here or not?

To address seasonality we have investigated further observations from ITM that are available. The observations reveal some changes, but not really any systematic seasonal variation (Figure S6). AW (T > 0 °C, S > 34.3 psu) is present at all depths with maxima of 1.5 °C at 500 m depth (bottom) and 0.5 °C at 170 m (top).

To our knowledge, there is no further observational evidence that AW is present in the cavity year-round. CTD profiles are limited to the summer season as the 79NG is hard to access in winter, partly due to a permanent fast-ice cover present just off the main front (Reeh et al., 2001; Straneo et al., 2012; Wilson and Straneo, 2015) which is more prominent and extensive during winter. All available temperate and salinity profiles collected so far are nicely and detailed described in Mayer et al., 2000; Wilson and Straneo, 2015; Schaffer, 2017; Schaffer et al., 2017; and Mayer et al, 2018.

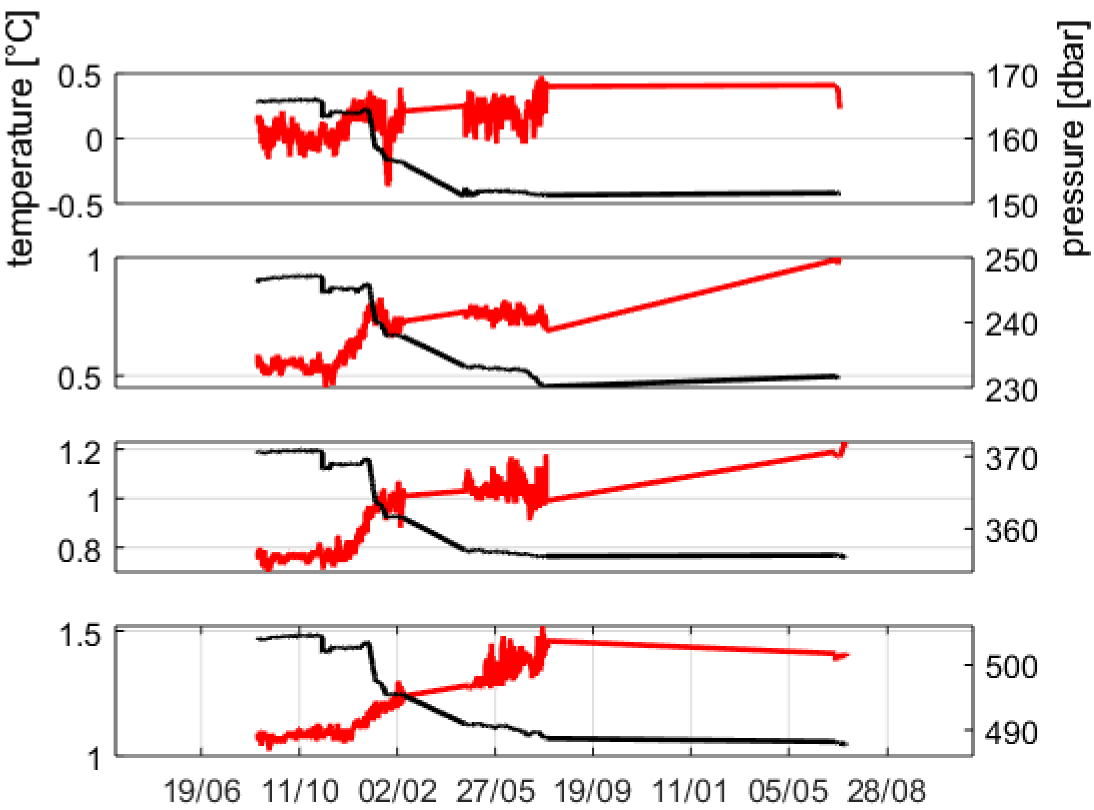

Figure S6: Temperature (red) and pressure/depths (black) recordings from an Ice Tethered Mooring (ITM) in the cavity below the 79NG ice tongue. The ITM was deployed in a rift close to the northern terminus. Initials depths are (from top to bottom) 165 m, 250 m, 370 m, and 500 m. Measurement period is from 23 August 2016 to 11 July 2018.

P17-18: I think the sensitivity of the plume model to various parameters have been addressed more exhaustively in the past (see general comments). How do the selected experiments here extend/complement/contradict the sensitivity findings of the previous studies?

This is correct. Much testing has been done for other ice-shelves, and the main unknowns that vary regionally is the drag coefficient and the entrainment rate. These could vary due to different ice shelf roughness for example. This is why we tested a large range in these quite extensively. There are also differences in forcing, like the subglacial discharge, and ocean ambient temperature, but this is more clearly affecting results.

P18: L24 - No calving? Ice dynamics?

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).

L32 - Is the evolving thickness of the ice tongue taken into account in the mean melt time calculation? (it should be) I am unconvinced that assessing future glacier stability simply based on a far-field temperature and a fixed glacier flow-line geometry is of much relevance/value. If this number should have any meaning, there should be some evidence/justification, that the more complicated processes are well captured by the simple plume model.

This part will be rewritten, and was mainly added to illustrate the magnitude of the changes in ocean driven melting. We do not attempt to calculate a realistic total melt-time of the 79NG.

P19:

L14 - what do you mean by "qualitatively"?

By qualitative we mean that the melt rates have roughly the same maximum, and that the shape along the plume path is similar. We will now include the satellite-derived melt rates from Wilson et al. (2017) in Figure 4a as asked for by reviewer #1. We think this comparison will be more clear then.

L15 - why do you think there are such large differences in the GL melt rates? Table 4 - do you use mean melt

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 S7). 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).

Yes, in table 4 we show the mean melt rates.

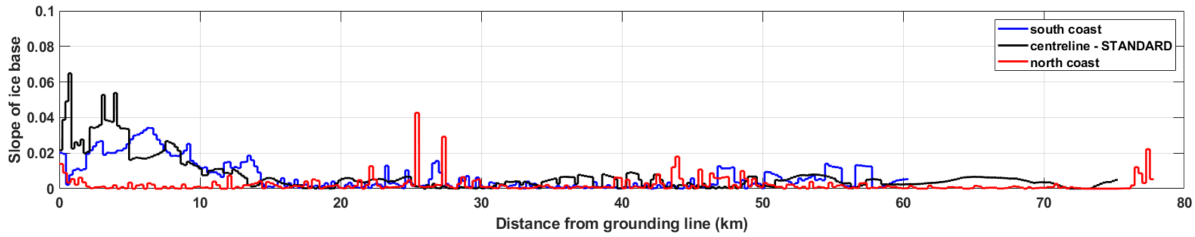

Figure S7: The slope of the ice base sinϕ of the 79NG with respect to the distance from the grounding line in km along three flow lines from RTopo2 (Schaffer et al., 2016). Centreline (black), South Coast (blue), and North Coast (red). The flow lines are marked red in Figure 2b in our manuscript.

P20:

L1 - "approximate linear relationship for mean melt rates near the GL region." - melt rate linear with what other quantity?

The melt rates are linear with respect to distance (X) along the plume. Will be changed to: "Jenkins, 2011 performed sensitivity experiments and found an approximate linear relationship for melt rates near the GL region with respect to distance, consistent with the melt rate evolution identified here".

L25 - typo, ECCOv4

Will be corrected.

L30 - I am not sure the use of ECCO is really adding much to the manuscript - since it is basically only used to get the past temperature variability, and the only justification that it does well, is that the range of variability is similar to the range of variability in the Norwegian Sea and Fram Strait. Why don't you just use the temperature range the observations show? If you use ECCO, you have to go on to justify it does well in this region.

There are not many observations in this area, and the few that exist are from summer. ECCOv4 uses the existing observations, and provide a physically consistent ocean reanalysis. If there were more observations available we would indeed have used them. ECCOv4 is a well established reanalysis product that is used in a number of studies. We are confident that using ECCOv4 as a constraint for the AW temperature range is the best available option, and it adds to the novelty of this manuscript.

L32 - why are they unrealistic? Could you please explain?

A larger warming than this is outside the range in ECCOv4 (Figure 3), and the general warming in Fram Strait over the last 30 years is below 1˚C (Onarheim et al. 2014). Will be changed to: "Simulated mean and maximum melt rates (Table 3) corresponding to higher ocean warming than 0.5˚C appear unrealistic as it is outside the ECCO range (Figure 3), and 30 year long AW observations in Fram Strait show warming of about 1˚C (Onarheim et al. 2014)".

P21:

L27 - spatial variability - do you mean across-flow or along-flow?

By spatial variability, we mean the across-flow variations.

L31 – how accurate is the satellite-derived melt rate near steep ice basal topography when the ice is not expected to be in hydrostatic equilibrium?

In the work from Wilson et al. (2017) a small band downstream of the grounding line is excluded (see answer above for P19 L15).

P22:

L21 - why not? where else would the meltwater go?

The increased surface melt could refreeze at depth in the ice sheet and/or flow along the surface of the glacier tongue into the ocean.

L31 - "assuming a constant ice flux" not a justified assumption. Is any of this thinning that is apparently already underway observed in the rate you/Wilson et al suggest?

The calculation is mainly included to illustrate the effect of ocean forcing. We assume a constant ice flux just to illustrate the level of submarine melting.

P23:

L6 - reference for the other 3 glaciers? Where is Kanquersal Glacier?

Helheim Glacier, Jakobshaven Isbræ, and Kanquersal Glacier will be discarded here, not really needed as they do not have a floating ice tongue as 79NG and Petermann Glacier and are not located in North Greenland.

L8 – existing observations, not new - as far as I understand there were no new observations/data presented in this paper

Valid point – we mostly use new ocean simulations (ECCOv4), and the CTD profile has been used to estimate melting before (Schaffer, 2017; Mayer et al., 2018). Will be changed to: "In this study submarine melt rates of the 79NG have been examined based on observations and simulations of hydrography and a 1D Ice Shelf Water plume model. These indicate that warm Atlantic Water (AW) from the continental shelf with temperatures up to 1.0˚C has access to the ice shelf at grounding line depth".

L11 - there would be an AW/melt mixture even if the melting was driven by subglacial discharge, would it not?

In theory yes – but the T/S properties are close to the melt-water mixing line (Figure 3c). If the fresh discharge dominated they would be along the "Runoff Mixing Line".

L11-12 awkward sentence

Will be rewritten to: "To establish a realistic sensitivity of the 79NG melt rates to ocean forcing we also needed to test different model parameters over a large range".

L13 - where are these differences and what are they attributed to?

The differences are now more clearly shown as those of Wilson et al. (2017) will now be included in Figure 4a. The cause of the variations in melt rate in our case has been established, so this is at least a step forward.

L18 - what is since 1995? I though you are talking about annual freshwater flux.

The sentence will be rewritten for clarity – also suggested by reviewer #1.

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