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Interactive comment on "Sensitivity of submarine melting on North East Greenland towards ocean forcing" *by* Philipp Anhaus et al.

Anonymous Referee #2

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Summary:

In my view this manuscript presents an application of an existing 1D plume model to the 79NG geometry. The model is constrained by a single CTD profile collected during one summer (described by Wilson et al). Elsewhere the model uses standard values of unknown variables. This CTD profile is then modified in four different ways to simulate AW temperature and/or thickness change, and the melt rate is recomputed from the plume model for these cases. The resulting melt rate for each case is then used in combination with an ice flux value estimate (satellite derived, Wilson et al) to give a time over which the ice tongue will melt under the prescribed AW, assuming nothing but the AW vertical profile changes.

Major comments:

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1) 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.

2) 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.

3) 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. Modeling studies are at times used as references where one would expect a reference for observations.

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4) 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, in stead of giving background throughout the manuscript, sometimes repetitively, sometimes not at all.

*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

*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)

*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.

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

In-line comments:

Abstract:

L7 - decay of what? 20 km from the grounding line? L10 - why is the melt rate sensitivity reported along a centerline 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? 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.

Intro:

P2: L5 - general statement - doesn't need a reference. L6 - Holland 2007 seems a more appropriate reference here, than this review paper. L7 - Do you mean net mass loss increase? Also, this sentence seems to contradict a previous sentence (P1L20-22: The enhanced mass loss is caused by increased surface melt, and retreat and speed up of marine-terminating glaciers (Enderlin et al., 2014)) L8 - The increase of submarine melting.... not the submarine melting itself - ...leads to an inward migration..... A stable glacier can still have submarine melt. 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 the change of submarine melt that may act as a trigger of change. 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? L26 - Are these references supposed to refer to observations of melt/discharge driven plumes? Two of these are models, not observations 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? 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. 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. L34 - This manuscript really overuses the word likely. It would help to clarify what is known (reference), what

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is speculated (reference), and what is assumed for the purposes of this study.

P3: L5 - In my view this is more of a sensitivity study, not a process study

Data: P3: L20 - What is a high-res digital bathymetry model and how does it compensate poor data coverage? 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.

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 a increase of the ice base, or if this was then enforced by some other procedure. L25 - How were the plume paths chosen? If it is discussed later perhaps reference the section.

P4: L1 - Which tidal effects? Do you include those here or you use the basic version? L5 - Why is STANDARD capitalized? At least at this point of the manuscript this is not at all clear. 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. L9 - I am not sure I am familiar with the terminology "line source equation" could you clarify what this term means? L9-10 - it is the quantities, not the fluxes of the quantities that are conserved.

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

P6: L7 - What value do you use for the ref. density? I don't see it in the table. L8 - Clarify that initial doesn't refer to initial condition as there is no time dependency in the

equations. L9 - Why is T0 set to freezing point? Does the result depend on different values of T0? What is S0 set to? L11 - Melt rates at 79NG are .... This statement is not specific to NG, or is it? L24 - Do you have any reference that -15C is reasonable, or how sensitive is the result to that?

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 tow makes it very confusing.

P7: Table 1 - the values used for the constant, where are they taken from? - reference 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 P7L9-11 seem to suggest. L8 - is 2-layer an assumption, or is it an approximation based on observations? L9 - define f when it is introduced not several lines later L11 - The differences between.... it seems like this belongs more to discussion/results than here. L13 - Since there is no dependency, aren't all variables diagnostic?

P8: L4 - I don't understand this what ....with S instead of Sb refers to 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? L7 - Forget et al., 2015 - Another incorrect reference. Also, this reference is not even listed under the "References" section 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? L14 - Could you please give more detail on the surface run-off calculation. L15 - Could you please provide reasoning for the assumption of

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equally distributing the flux?

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. 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? P10: L5 - what do you mean by "not significant in mean"? L7 and elsewhere - clarify when you are talking about observed and when about modeled AW. 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? L10 - why three flow lines? L15 - introduce figures in their numbered order L22 - do you mean retreat? 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 L18 - Optimal in which sense? how did you determine that?

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. L2 - result not results L5 - 10 km....looks even within first 2 km to me L6 - what is the definition of GLZ here? 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.

P12: L1 - gradually approaching -2.0 C (Fig 4d shows the limiting T to be more like -1.7 or -1.8 C) L2 - In contrast is the density contrast (reword) L7 - what is the reason for the sudden increase at $\sim$72 km? 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. 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.

P13: L8 - accelerates linearly do you mean that the acceleration is constant over the first 5 km? There are still jumps in the velocity L9 - Besides three local minima - which ones? I see lots of (>3) local minima on the quite rugged velocity plot 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. L17 - a 2D or 3D concept .... the units suggest that you are extrapolating the result to 3D L18-21 - first introduce/justify the experiments, then describe them L25 - At the end melting variability - at the end of what? L31 - why 0.5C? L34 - ... and outflowing flux down-glacier? Or do you just mean flux across the grounding line?

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. L2 - in a submarine melting of about 4.2myr−1 - 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

P15: L5 - because ECCOv4 does not have an ice tongue - this is not a justification for your assumptions. 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?

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?

P18: L24 - No calving? Ice dynamics? 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.

P19: L14 - what do you mean by "qualitatively"? L15 - why do you think there are such large differences in the GL melt rates? Table 4 - do you use mean melt

P20: L1 - "approximate linear relationship for mean melt rates near the GL region." - melt rate linear with what other quantity? L25 - typo, ECCOv4 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. L32 - why are they unrealistic? Could you please explain?

P21: L27 - spatial variability - do you mean across-flow or along-flow? 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?

P22: L21 - why not? where else would the meltwater go? 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?

P23: L6 - reference for the other 3 glaciers? Where is Kanquersal Glacier? L8 - existing observations, not new - as far as I understand there were no new observations/data presented in this paper L11 - there would be an AW/melt mixture even if the melting

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was driven by subglacial discharge, would it not? L11-12 awkward sentence L13 - where are these differences and what are they attributed to? L18 - what is since 1995? I though you are talking about annual freshwater flux.

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