Impact of icebergs on the seasonal submarine melt of Sermeq Kujalleq
Karita Kajanto et al.
RC1: 'Comment on tc-2022-136', Anonymous Referee 1, 25 Jul 2022

Summary

This paper presents novel simulations of an idealised representation of Ilulissat Icefjord, West Greenland, quantifying the impact of iceberg submarine melting on the circulation and water properties in the fjord and on the submarine melt rate of the Jakobshavn Isbrae icefront, from winter through to peak summer. The simulations show that the effect of icebergs is significant, causing substantial cooling and freshening over their draft as well as more modest but still significant changes in the deep basin, plus changes to the strength and pattern of fjord circulation. The authors go beyond this, and also show that these changes to the fjord water properties lead to a reduction in the neutral buoyancy depth of the subglacial discharge-driven plume, which consequent impacts on glacier submarine melt rates, fjord circulation and fjord water properties. These are novel findings, which go beyond previously published research on iceberg-ocean interactions in Greenland's fjords, providing insights into the interactions between iceberg melt, plume dynamics and fjord circulation that were not previously available. However, I believe the paper is hampered by three main issues, which I outline below.

- Thank you so much for your time and effort and the thoughtful comments that have greatly improved the manuscript.

Firstly, the introduction as written does not provide sufficient context to motivate the work. The importance of icebergs in glacial fjords is a relatively new field of research, and many who have not been following that research closely will not necessarily appreciate the need to conduct the experiments presented here intuitively, and will likely not grasp the potential importance of the potential links between iceberg-induced changes to water column properties, the vertical distribution of glacier submarine melt rates and calving rates. I suggest that the introduction be lengthened (it is currently only three paragraphs) so that it can provide a more thorough narrative to introduce the work, ensuring that the potential importance of vertical variations in water properties (and how icebergs might affect those) for iceberg calving are explained. Introduction too short and does not provide sufficient context or background to motivate the work. This is not too onerous a task, but it is an important one if readers are to understand and appreciate the importance of the results.

- Thank you for the comment, we have now revised the Introduction to better narrate the significance of dynamics in glacial fjords and the potential role icebergs have in them.

Secondly, many of the key results in the manuscript focus on mixing and recirculation within the fjord. They will therefore depend somewhat on mixing and diffusion within the ocean model. I was therefore a little surprised that no analysis quantifying the sensitivity of the results to choices of model viscosity and diffusivity parameter values was presented. To remedy this, I think these sensitivity analyses need to be conducted; at the very least, a few simulations testing higher and lower values of vertical eddy viscosity, and horizontal and vertical diffusion coefficients of temperature and salinity in the IBP setup. It may be that they make little difference to the behaviour of IMAW and GMW, but this needs to be demonstrated with simulations in order to ensure the results are robust to these choices.

- Thank you for the comment. We have now included sensitivity experiments with vertical eddy viscosity and horizontal diffusion coefficients of temperature and salinity in IBP, as well as the OBCS restoration time scale. These are now presented in Supplementary Figure S4. Horizontal diffusivity has a slight cumulative impact on the peak summer fjord properties, as the efficiency of heat/salt transport over the sill is either increased or decreased, and thus the relative contributions of GMW and IMAW vary slightly. This does not, however, change the results or conclusions. We have included this sensitivity now in the discussion, see lines 466–469.

Thirdly, a considerable portion of the results and discussion is dedicated to (1) entrainment of GMW into the deep basin and (2) cooling of inflowing water by icebergs, which subsequently enters the deep basin, and the behaviour of these water masses form the basis for some of the key results in the manuscript. However, no definition or means of identifying GMW and its fate is provided, nor is the inflowing water tracked, which somewhat undermines the related results and discussion. To remedy this, you need to describe your method for identifying GMW, assuming one is used. In addition, I think it would greatly strengthen the manuscript if you were to perform a small number of additional experiments in which tracers are included at the ocean boundary (and perhaps also in the subglacial discharge) as these would unambiguously demonstrate the changes in GMW outflow dynamics and allow you to calculate precisely the relative contributions of IMAW and GMW to the deep basin.

- Thank you for the suggestion. We have now included a set of tracer experiments in the supplementary, where we model the exit of a passive tracer pulse in the subglacial discharge for different seasonal conditions. The results from these experiments in Fig. S7 effectively visualize the GMW outflow and early and late-season refluxing at the sill. Since passive tracers increase computation time, which is already 4–5 weeks for a full runoff season in *IBP*, we do not consider long runs with tracers. It is not clear to us how to technically trace the IMAW, since tracing only inflow does not provide additional information to Fig. 4. Therefore, we did not run a tracer experiment for IMAW. However, since the tracer experiments for GMW demonstrate the exit of GMW during peak season, it conversely means that the modification of inflowing water in Fig. 7 must be from IMAW. We have improved the description of our interpretation of both model results and the comparison to observations in Section 4. We consider our results together with the tracer experiments now clearly demonstrate the dynamics of the GMW.

I believe this paper presents important and novel findings of iceberg-ocean interactions in Greenland's glacial fjords, particularly with regard to glacier submarine melt and mélange dynamics; however, more detail, clearer methods and some additional context is required to make this paper robust enough to be published.

1 Below, I provide more specific comments going through the paper line by line:

Abstract

Line 1: "marine terminating" should be "marine-terminating"

- Thank you for the comment, text edited.

Line 3: consider changing "at depth" to "vertically" or "along the iceberg draft" or similar.

- Thank you for the comment, text edited.

Line 3: "contributing to fjord stratification, thus impacting melt and dynamics at the front". There's a lot to unpack here. Firstly, "contributing to fjord stratification" is somewhat ambiguous. Secondly, regarding "melt", this statement is written as if there is already substantial evidence showing that icebergs affect glacier submarine melt rates and that it is widely known, which I don't think is the case (though this manuscript is of course a start). Thirdly, regarding "dynamics", it's not clear if this refers to the dynamics of the circulation at the ice-ocean interface or the dynamics of the ice front itself (i.e. calving). The former is a logical progression from a change in fjord stratification, but the latter is quite a jump. Consider rewording this sentence to focus on the known (albeit simulated) affects of iceberg melt on fjord water properties and circulation, but state that the impacts of those on glacier submarine melt rates are poorly known.

- Thank you for the comment, text edited.

Line 3-4: "We model the high-silled..." -> suggest changing to "We model an idealised representation of the high-silled...".

- Thank you for the comment, text edited.

Line 4 and elsewhere: I suggest being more specific than just "the effect of icebergs", and clarify (at least on the first use in the main text) that this means the effect of submarine iceberg melting.

- Thank you for the comment, text edited.

Line 5: change "fjord properties" to "fjord water properties from winter through August" (or similar)

- Thank you for the comment, text edited.

Line 6: "seasonality" doesn't seem quite right here, given the simulations covered a winter period as an initialisation then the rising limb of the summer hydrograph. I suggest changing this to "primary driver of fjord circulation, glacier melt and iceberg melt during the melt season"

- Thank you for the comment. We have now included the decline of the runoff in our results by suggestion from Reviewer 2, and also extend the experiments to the following winter in the supplement.

Line 6-7: Suggest changing "Icebergs are necessary to include to correctly..." to "Including submarine iceberg melting in the simulations is required to reproduce the observed water properties in..." (or similar).

- Thank you for the comment, text edited.

Line 8: Consider providing representative values for the amount of freshening and amount of neutral buoyancy depth depression (though I appreciate these are sensitive to various model parameters)

- Thank you for the comment. Since the representative values for cooling and freshening are depth-dependent, and the depression of the neutral buoyancy depth is seasonal, we do not see it meaningful to give single values in the abstract.

Line 10: "increased entrainment of glacially modified water into the fjord" – should "entrainment" instead be "recirculation" or "reflux"? Also, consider linking this point to the depression of the depth of glacially modified outflow.

- Thank you for the comment, text edited.

Line 10: Consider changing "ambient water" to "shelf water" or similar.

- Thank you for the comment, we stick to the terminology in (Straneo and Cenedese, 2015).

Line 11: Consider changing "limits melt to the deep section of the front…" to "limits the vertical extent of plume-enhanced glacier melting…" or similar, because melt will of course occur across the entire front.

- Thank you for the comment, text edited.

Line 13: again "impact of icebergs" should really be "impact of submarine iceberg melting"

- Thank you for the comment, text edited.

Line 14 and elsewhere: "melange" should be "mélange"

- Thanks, text edited.

Introduction

Line 18: "controlled by the fjord geometry and fjord stratification" is somewhat ambiguous (what aspect of the glacier is controlled?) and excludes all the other important factors that affect glacier behaviour, particularly external forcing. If this point is necessary to introduce the manuscript, then I suggest a longer explanation is required.

- Thank you for the comment, this was indeed not necessary, and is no longer included in the new version of the Introduction

Line 20: Clarify that this refers to "future sea-level contribution estimates"

- Thanks, text edited.

Line 20: "Greenland ice sheet" should be capitalised as has been done on line 16.

- Thanks, text edited.

Line 22: I suggest that (1) submarine melt rate and/or our ability to model submarine melt rates, and; (2) our knowledge of water properties at the ice-ocean interface, should be included in this list. Consider also being more specific for each of the items currently listed. For example, with "calving", does this mean overall calving rates, or some particular aspect of calving? Similarly, should it be "subglacial discharge volume"?

- Thank you for the comment, this formulation no longer exists in the revised Introduction.

Line 22: "shape of the plume" – I assume this refers to the geometry of the source; however, plumes have not been introduced yet, so it's not clear what is meant here (see first major comment).

- Thank you for the comment, this formulation no longer exists in the new version of the Introduction.

Line 23: I suggest that "ocean driving retreat" is perhaps overly simplistic and not a fair representation of the current state of knowledge. There is for example quite considerable evidence that increasing runoff, driven by rising atmospheric temperatures, would increase submarine melt rates and potentially affect glacier terminus position. Consider changing this to something like "evidence that changes submerged glacier geometry due to changes in the magnitude and spatial distribution of glacier submarine melting are a key control on marine terminating glacier retreat rates"

- Thank you for the comment, this formulation no longer exists in the new version of the Introduction.

Line 26: Again "melange" should be "mélange"

- Thanks, text edited.

Line 27: Please provide at least one appropriate reference for this statement (the connection between dense mélange and glacier calving). It would also be clearer if the direction of this connection (that mélange presence suppresses calving) is stated in this sentence, rather than in the subsequent one.

- Thanks, text edited, lines 40–45.

Line 27-29: Consider also providing the counter example, that anomalously long periods of mélange absence are associated with anomalous glacier retreat, with appropriate references.

- Thank you for the comment, we have chosen not to expand the description of rigid-mélange glacier-front interactions here in the Introduction, since we want to keep the focus on the impact of icebergs on the hydrography. However, these interactions are further discussed in the Discussion, lines 418–447.

Line 31: Moon et al. (2018) didn't present observations showing icebergs modify fjord water properties in fjords, so I don't think it is an appropriate reference here.

- Thanks, text edited.

Line 32: specify that the comparison between iceberg freshwater flux and subglacial discharge is on annual timescales (which I think has to be for this statement to be true)

- Thanks, text edited.

Lines 34-35: whilst I agree with this statement, I do not think that it follows logically from the preceding sentences. I strongly recommend revising the preceding sentences to provide a more detailed summary of how icebergs could impact the glacier front – i.e. how do they affect fjord stratification and oceanic heat delivery to calving fronts? What is the vertical pattern of that? And, based on our understanding of undercutting-driven calving, how might that affect the glacier front? (see my first major comment)

- Thank you for the comment, this statement and the accompanying paragraph is now revised, lines 46–54.

Line 36: Change "the fjord" to "Ilulissat Icefjord", and change "to the glacier front" to "for submarine melting and undercutting of Sermeq Kujalleq". Or, if you wish to keep this sentence non-specific, then just say "a large west Greenland fjord" or similar.

- Thank you for the comment, text edited.

Line 37: Change "construct a model of" to "construct an idealized model of" – I appreciate that it's not that idealized, but the geometry is idealized.

- Thank you for the comment, we have changed the formulation.

Line 38: MITgcm has not been defined yet. Marshall et al. (1997) should also be referenced

- Thanks, reference added.

Line 39: change "impact of icebergs" to "impact of submarine iceberg melting" or similar.

- Thank you for the comment, text edited.

Line 40: "each phase of the discharge season" would imply that you also consider the descending limb of the seasonal runoff hydrograph, which you don't, so I suggest changing this to "during winter, spring and peak summer" or just "throughout that time".

- Thank you for the comment, as stated earlier, we have now included the whole runoff season.

Line 43: "stability" can mean lots of things, but instability usually implies continued retreat even once a forcing is removed, which I don't think you mean here. Consider changing this to "calving rates" or similar.

- Thanks, text edited.

Line 43: change "the glacier" to "Sermeq Kujalleq"

- Thanks, text edited.

Ilulissat Icefjord

Line 50: "70 Gt/a" – I thought it was more like 55 Gt/a at most, so this seems a little high (see Mankoff et al. 2020). I also don't think that Bondzio et al (2017) is an appropriate reference given it is a modelling study.

- Thanks, text edited.

Line 50: Suggest starting a new sentence and changing "leaving the fjord clogged with icebergs" to "The rapid supply of icebergs and the barrier presented by sill, often leave the fjord iceberg-congested", and move this sentence to line 53 after the info about icebergs grounding on the sill.

- Thanks, text edited.

Line 54: CTD has not been defined.

- Thanks, text edited.

Line 58: Consider providing representative temperature and salinity values for the layers

- Thank you for the suggestion, we have included the range of the deep basin temperature variability, line 82.

Line 60: provide a definition for glacially-modified water i.e. a mixture of runoff, fjord water and meltwater

- Thank you for the comment, definition is added, line 79.

Line 63: Specify that you mean seasonality of water properties (and circulation?)

- Thanks, text edited.

Line 73: It is not clear what "in front of the sill" means. Seaward of the sill? Please revise the wording so that your meaning is clear

- Thanks, text edited.

Methods

Line 76: I'm surprised you used MITgcm in hydrostatic mode given the resolution of your domain and the inclusion of a steep sill. In high-resolution simulations with steep topography, in which vertical momentum may be important, it is common and recommended to use the non-hydrostatic mode. Perhaps it makes little difference here, but I suggest that it is at least worth running at least one simulation in non-hydrostatic mode to check.

- Thank you for the comment. We have tested with *NoIBP* that there is no difference between the modes, and have included this in the supplement, please see Fig. S5. We stick to the non-hydrostatic mode due to computational efficiency, since experiments with the IceBerg package take 4-5 weeks to run, depending on the experiment.

Line 80: Specify the direction of each resolution value.

- Thank you, text edited.

Line 81: Change "third dimension" to "vertical dimension"

- Thank you for the comment, text edited.

Line 81: Can you clarify what is meant by "icebergs and plume width considerations". I can appreciate how a relatively fine vertical resolution is required to faithfully represent the vertical distribution of iceberg freshwater flux, but it is not clear how it affected plume width in the horizontal direction.

- Thank you for pointing out the grid description. We do not consider lateral variations in this study, which is why we have excluded rotation, even though the model is technically 3D. We have chosen a 3D setup, since the y-dimension is needed to include icebergs and to study the effect of the subglacial discharge outlet width. The description of the grid is now revised, lines 100–102.

Line 85: Why not just call it "source width" or "outlet width". Calling it "plume width" is confusing, because this does not refer to a plume itself and because the width of a plume changes as it entrains ambient water.

- Thank you for the suggestion. We choose to keep "plume width", as we consider "source" and "outlet" as potentially ambiguous. Since we use the sheet plume parameterization, the plume expands only in x-direction (Jenkins, 2011). We now state this on line 106.

Line 89: "much larger contributor" to what?

- Thank you for the comment, text edited.

Line 91: Please note that the observations from Jackson et al (2017) were from a different fjord

- Thank you for the comment, text edited.

Lines 94-101: please specify somewhere here what outlet height you use (or how you define it), what the velocity is, and which of those variables you modify in order to increase subglacial discharge throughout a simulation. Also, Cowton et al. (2015) use a fixed water velocity at the outlet and therefore accommodate all changes in runoff by changing the radius of the outlet. They demonstrated that their results were not sensitive to the choice of water velocity. However, I do not recall seeing a similar analysis for a sheet plume. Therefore, please also indicate with evidence (whether through simulations or otherwise) whether your results are sensitive to your choice of water velocity and outlet height.

- Thank you for the comment. We follow the approach described in the documentation of the IcePlume package (obtained from Tom Cowton with the rest of the package), where changes in runoff are accommodated with varying the thickness of the outlet. We keep the water velocity constant at the default 1 m/s, since the results are not sensitive to this value, as stated in the documentation of the package. We have added this to the supplement, along with a sensitivity test of the plume to the water velocity (Fig. S6).

Lines 94-101: please specify how you conserved mass in the domain so that the runoff did not just slowly fill up the basin (which would modify sea surface slopes and drive erroneous currents). It is typical to impose a small outward velocity at the boundary, equivalent to the subglacial discharge volume flux, but it is not clear whether that was done here.

- Thank you for the comment, we do indeed have an outward velocity at the western boundary, matching in volume with the subglacial discharge flux. This is now stated in lines 139–140.

Line 101: Please define what is meant by "bell-shaped". Is it a Gaussian? I suggest it is also worth noting here that you do not simulate the falling limb of the runoff hydrograph as your simulations stop at the end of August.

- Thank you for the comment, we have revised the text in line 128, and have now included the late season, please see our reply above.

Lines 116-119: I think this description needs some more detail and clarity with regard to where you are specifically describing boundary conditions imposed on the model boundary and where you are describing initial conditions throughout the domain. I think it would be useful to state where and when the observations in the fjord were acquired, which you use to set the conditions at and below sill depth. Assuming you initialise the model at and below sill depth with observations acquired in the fjord during summer, then that initialisation will have an imprint of glacially modified water and other summertime processes. Therefore, please provide evidence that the model reached a quasi-steady state in terms of water properties (not just circulation) during the winter portion of your simulations. Your Sup. Fig. 2 does a fairly good job at this and I recommend explicitly referring to that here.

- Thank you for the comment, we have revised the description of the boundary conditions with reference to comments from both Reviewers. Lines 141–150.

Line 124: Change "Melt and negative salinity flux" to "Heat and salt fluxes" as there is no real freshwater flux in the IceBerg model of Davison et al. (2020)

- Thank you for the comment, text edited on line 154.

Line 129: It is also worth mentioning that the only deterioration mechanism is submarine melt (i.e. no wave action, no mechanical breakup and no plumes – though they are crudely parameterized with the background velocity.

- Thank you for the comment, text edited, lines 160–162.

Results

Line 146: again "seasonality" doesn't seem like the right word for this, as it's only the winter/spring and up to peak summer.

- Thank you for the comment, text edited, we refer to our earlier reply regarding seasonality.

Line 152-154: My understanding of IcePlume is that the plume water is input to the model grid cell at the neutral buoyancy depth, which immediately becomes a horizontal outflow (and any subsequent dynamics are dictated by your choice of diffusion and mixing parameter values in MITgcm), and that there is no calculation of the plume vertical momentum above the neutral buoyancy depth, so I can't work out how you calculated the continued vertical transport of the plume due to momentum. Did you modify the code? Perhaps I'm wrong and I just missed that option. Or is this sentence more a musing on what happens in reality but not in the model? If so, please clarify in the text.

- Thank you for the comment, this is indeed correct. We had a mistake in our diagnostics calculation, which has now been corrected and we now use the outflow from the plume to the model grid as an indicator to the neutral buoyancy depth. We have edited the text, lines 185–187.

Line 154: related to the above, is the 230 m outflow of GMW just the plume neutral buoyancy depth, or have you extended the plume model?

- Thank you for the comment, please see our reply to the comment above.

Line 154: Also related to the above, how is GMW defined here in such a way that you can specify a depth and even arrows on some figures? Did you use ptracers? I can't see a definition in terms of water properties or currents anywhere in the manuscript, so it's no clear how these arrows were generated if not from some tracer.

- Thank you for the comment. The arrows are streamlines starting from the cell where output from IcePlume enters the model grid. We state in the caption of both Figure 3 and 4: "Black arrows indicate the centerline of the outflow of glacially modified water from the plume". We have added tracer experiments to the supplement in order to help to visualize the GMW outflow.

Line 160: As written, this implies that there was no inflow from Disko bay until August, which I don't think is the case based on Figure 4.

- Thank you for the comment, we have edited the text and first reference to the inflow over the sill at line 184.

Section 4.1: there is no mention of up-fjord currents above the GMW outflow, and indeed they don't seem to exist on Figure 4, but shouldn't they exist in this situation?

- Thank you for the comment. There is a small up-fjord current in *NoIBP* above the GMW outflow, but speeds are insignificant compared to GMW outflow and the inflow over the sill, hence the color scale does not show these.

Line 161: the switch to melt rates in this paragraph is somewhat confusing, given that the first half the paragraph is about fjord circulation. I suggest you start a new paragraph for submarine melt rates.

- Thank you for the comment, we have restructured the Results-section.

Line 166: Define "early season" (May-July?) and clarify what is meant by "entrainment" – does this really mean reflux? Or eddy-driven mixing between deep basin water and GMW outflow? As written, it's not clear what is being entrained nor what is doing the entraining.

- Thank you for the comment, we have added the definitions, and this sentence no longer appears as such in the revised Results-section.

Line 148: "properties remain constant since there is no circulation" but presumably there are some small changes in temperature and salinity because of diffusion (Sup. Fig. 2 suggests there are some changes in model avg properties), and Fig. 4 suggests there is circulation down to 500 m.

- Thank you for the comment, we have revised the wording on line 183.

Line 171: "Seasonality" in Section 4.2 subheading. See comments above re seasonality

- Thank you for the comment, as described earlier, we have revised the seasonality.

Line 201: I'm not sure this reasoning holds. With greater subglacial discharge, the GMW will almost certainly be warmer because the more vigorous plume will entrain more deep basin water. This will drive more rapid iceberg melting, thereby cooling the GMW locally, such that the MITgcm diagnostic temperature remains constant. The fact that temperature remains constant in the diagnostics could therefore just be a reflection of the balance between iceberg-driven cooling and plume-driven warming.

- Thank you for the comment. It is correct that this described effect also exists. However, the increase in the plume temperature is only 0.2 degC from July to August, since July is already a month of relatively high discharge, and the deep basin cools slightly. We have removed this statement from the manuscript due to ambiguity.

Line 204: are these values just for the inflow? I assume they are, based on the context, but the preceding use of "volumetric flow rate" is somewhat confusing. I suggest you modify this to "increasing the up-fjord volume flux over the sill..."

- Thank you for the comment, text edited, lines 235–238.

Line 213: "entrainment of GMW into the inflow into the deep basin" is a bit confusing. Perhaps just "enhancing the reflux/recirculation of GMW into the deep basin" or similar would be clearer?

- Thank you for the comment, text edited, lines 282–284.

Line 213: "the melt rate" specify that this is now the glacier submarine melt rate. Actually, I suggest just changing this sentence to something like "These changes to the water column properties reduce average glacier submarine melt rates by X%/Y m/d and reduce the vertical extent of plume-enhanced melt rates by X m".

- Thank you for the comment, text edited, lines 284–286.

Line 215: again, this is submarine melt, and specify that you are now referring to total submarine melt flux. Regarding "melt will be limited to the deep part of the glacier", please specify that you are referring to plume-driven melting, because the rest of the submerged face will still be melting.

- Thank you for the comment. As shown by the horizontally averaged melt rate in Fig. 5, also the melt outside of the plume is reduced, and vertically limited, since there is little heat available for melting within the surface and intermediate layers. Please see the new formulation in lines 284–286.

Line 219: It's not clear how "GMW outflow" is defined in the observations, given that no current observations are presented.

- Thank you for the comment, we have now reformulated the description of the observations, lines 261–274.

Line 222: "the observed deep basin is dominated by melt" – do you mean "vertical changes in temperature and salinity and dominated by ice melt.." ?

- Thank you for the comment, we have now reformulated the description of the observations, lines 268–287.

Line 224: "we interpret this to indicate a large contribution of IMAW rather than GMW" – I agree with this interpretation but can you explain your reasoning? For example, based on the simulated circulation and/or simulated volume fluxes of GMW and IMAW.

- Thank you for the comment, we have now reformulated the description of the observations, lines 268–287.

Line 226: This is a neat suggestion that would be useful for those working in the field. This is something you can very easily test using your model output and range of experiments, by determining the isopycnal separating up and down-fjord currents. Given the potential utility of this information, I suggest you do this analysis and present it at least as text at this point in the manuscript, but possibly also as a supplementary figure.

- Thank you for the comment, we have now reformulated the description of the observations, including this suggestion, lines 268–287.

Line 228: "we run three different plumes and three different iceberg distributions" should be "we run simulations using three plume/outlet widths and three iceberg distributions (Table 1)". The reference to Table 1 would make more sense mid-sentence, otherwise readers may look to table one for the outcome of the sensitivity analysis.

- Thank you for the comment, text edited, lines 289–290.

Lines 232-234: I suggest removing the sentence beginning "Changes in the depth of the GMW outflow..." and instead changing the previous sentence to "Plume width is the primary controlling factor on the vertical extent of the plume, which impacts fjord circulation and water properties. I also suggest re-wording the next sentence to: "Decreasing plume width reduces the volume of deep basin water entrained into the plume. The plume thus rises higher in the water column and exports GMW closer to the fjord surface. The shallower and more concentrated outflow of GMW leads to reduced entrainment/reflux/recirculation of inflowing water at the sill..." As currently written, these connected and important processes appear bitty and almost unrelated.

- Thank you for the comment, we have reformulated this paragraph, please see lines 289–300.

Line 237: “entrainment into the plume is more efficient” I think you should be more specific here by stating that total entrainment of deep basin water per m3 s-1 discharge is likely greater than with a narrow plume outlet, because of the greater surface area available for entrainment. Also “causing the plume to remain deep” should be “causing the plume to reach neutral buoyancy lower in the water column”

- Thank you for the comment, we have reformulated this paragraph, please see lines 289–300.

Line 237: “entrainment of GMW” see comments above re the use of entrainment in this context, which is particularly confusing in this instance because of the preceding discussion of an entirely different kind of entrainment.

- Thank you for the comment, we have reformulated this paragraph, please see lines 289–300.

Line 239: please quantify what is meant by “significantly higher” (double? An order of magnitude?) The reader shouldn’t have to go to the figure to check just how significant this difference is.

- Thank you for the comment, text edited, line 300.

Line 241: This wording suggests that there are no changes in the amount of cooling and freshening in the upper layers that contain some icebergs in all experiments i.e. that there are only changes in the vertical extent over which cooling and freshening occurs. However, Fig. 9 suggests that are changes in water properties at these depths?

- Thank you for the comment. We have revised the description of the impact of icebergs to better include the dynamic impact of increased iceberg depth has on the water column. Lines 301–306.

Lines 241-242: it’s not clear which of these contributions relate to the intermediate layer or the deep basin, or both? Please clarify your meaning

- Thank you for the comment. We have revised the description of the impact of icebergs to better include the dynamic impact of increased iceberg depth has on the water column. Lines 301–306.

Discussion

Line 300: “more realistic” – perhaps “more appropriate in Ilulissat Icefjord” would be more precise?

- Thank you for the comment, text edited, line 331.

Line 304: no direct contact with the plume? Or the plume outflow/GMW?

- Sorry, we were not able to find where this comment refers to.

Line 324: “seasonal surface warming in Disko Bay has little impact in the fjord when icebergs are included” – I think this correct, but it should be substantiated with reference to a figure and the key observation from that figure. For example, “when icebergs are included, as shown by the absence of a simulated summer surface warming in the fjord (Fig 3f-j)” or similar. Substantiating this statement is also necessary to substantiate the following statement regarding the driver of seasonal iceberg freshwater flux.

- Thank you for the comment, text edited, line 399.

Line 328: “wintertime freshwater flux is estimates” – freshwater flux from what? Icebergs? Subglacial melt? Glacier submarine melt? Also consider changing m3 s-1 to “cumecs” when using it in a sentence rather than as a unit.

- Thank you for the comment, text edited, lines 400–401.

Line 330: perhaps I’m wrong, but I thought that Moon et al (2018) kept their iceberg distributions static for each of their integrations, and that the seasonal differences in freshwater flux were largely due to changes in water temperature?

- Thank you for the comment, this is indeed true, we have edited the text accordingly, lines 404–405.

Line 336: “either the calving-multiplier effect or a destabilizing influence” There’s quite a lot to unpack here.. My understanding is that the calving-multiplier is defined as some ratio of calving flux and submarine melt flux (or rates if considering 2D), but that there could be a suite of processes that lead to the calving multiplier value at a given glacier and at a given time. In other words, the calving multiplier is just a useful term to describe the aggregated effect of lots of processes that we either can’t measure or don’t understand. So, to say melt and calving are connected through the calving multiplier is not very helpful, because they are by definition, even if there is no multiplier. Therefore, it also doesn't make sense to distinguish a separate "destabilizing influence", because that would be roped into any calving multiplier value. If you want to emphasise the point that changes in the rate and vertical distribution of melting can affect calving rates, potentially in a non-linear manner, I suggest you focus on describing mechanistically how you might expect that to occur (the following sentences to that to a degree), and then afterwards perhaps summarise those in terms of the calving multiplier.

- Thank you for the comment, we have reformulated this paragraph, lines 409–417.

Line 340-342: "melt is the main driver of calving". As written, this implies that icebergs increase calving compared to periods where there are no icebergs. However, based on the next paragraph, I'm not sure if that's what you mean because melt-driven undercutting would act as a stabilizing influence and reduce bottom-out rotation driven calving events, not increase them. This is never explicitly mentioned in the manuscript, but I think it would really help to clarify the argument that is being made in this and the following paragraph. However, it's worth pointing out in the manuscript that bottom-out calving events do still occur even when the fjord hosts many icebergs. So perhaps the conclusion is something like "icebergs encourage a melt pattern that should hinder bottom-out calving, so maybe if there weren't icebergs, we would see even more bottom-out calving?".

- Thank you for the comment. Our aim with this section discussing possible implications to calving is to first discuss the seasonality of melt rate driven by subglacial discharge and its possible implications to calving, then the possible connection of seasonality if the plume to the rigidity of the melange, and finally the impact of the presence of icebergs to the glacier front when the melange is not rigid. We have revised the formulation of these paragraphs, lines 409–433.

Section 5.1. Somewhere here I think it would be worth mentioning the impact of icebergs on the total glacier submarine melt flux. Melt rates may not change that much at a given depth, but presumably the change in neutral buoyancy depth really affects the total melt flux?

- Thank you for the comment, this is now included in line 256.

Line 344: "strong control of the melt rate, both through discharge volume". I suggest being more specific here and relating discharge volume to plume properties (i.e. vertical velocity)

- Thank you for the comment, this sentence no longer appears in the revised formulation of the Discussion.

Line 355: "we see the seasonal growth of rigid mélange as a consequence of decreased melting and calving" This is arguably one of the key outcomes of the paper, and yet it's not clear exactly what is being suggested here? Decreased glacier melting or iceberg melting? Decreased calving allows time for the mélange to become rigid, even though less ice is being supplied to it? Also, how does this hypothesis explain years with very late or even no mélange formation? (assuming it can be applied to other fjords as well?) Given the importance of the proposition here, which is a completely different perspective on mélange-glacier interactions than has previously been supposed, I think this hypothesis needs to be explained much more clearly.

- Thank you for the comment, we have reformulated and added further clarifications to this hypothesis. Unfortunately our study is not sufficient to fully resolve the dynamics of the melange in a general sense. We point out that we assumed a constant and dense iceberg coverage, and further study is needed regarding sparse and variable iceberg coverage. Please see the revised formulation on lines 409–433.

Line 355: Please also clarify whether you are suggesting that periods of mélange reduce calving in a binary manner, or whether you are suggesting that periods of more concentrated/thicker mélange will suppress calving more than times of less concentrated/thinner mélange. I would argue that the former is justified by your results, but that the manuscript presents much less evidence for the latter (if nothing else, the melt curves in Figure 10 are very similar for each different iceberg configuration, even though these iceberg configurations represent quite a wide spread of iceberg conditions).

- Thank you for the comment. While our iceberg distributions cover a large range in terms of keel depth, the iceberg concentration is (>=60%), which is relatively high compared to the range Davison et al. (2022) considers. We have added this consideration to lines 429–434.

Line 367: "increased entrainment and iceberg modification as the plume weakens" – entrainment and modification of what? (see similar comments throughout this response)

- Thank you for the comment, this formulation no longer exists.

Section 5.3: given that some of the key results in the manuscript relate to mixing between water masses, I was surprised that there wasn't a discussion here of the sensitivity of the results to choices of diffusivity and viscosity parameter values (see major comment above)

- Thank you for the comment, we have now included this in the discussion, lines 466–469, and into the supplement, please see Fig. S4.

Line 402: I'm not sure that comparison to Fitzmaurice et al. (2018) is robust here. Fitzmaurice et al (2018) consider a melt parameterization for entire icebergs, so need to distinguish between periods in which the plume is attached to or detached from the iceberg. However, the IceBerg package of Davison et al. (2020) uses the 3 equation formulation for submarine melting of a portion of an ice wall, so melt rate is a function of the temperature and velocity at the ice-ocean interface, regardless of whether those currents are apparently caused by a plume or the ambient water motion. Of course, there are no plumes as such in the IceBerg package, and their parameterisation is crude, so that certainly is an area of improvement.

- Thank you for the comment.

Conclusions

Line 410: "glacier's response to external forcing" this statement is too general to be supported by the results. I suggest being much more specific and instead focus on the suggestion made earlier in the manuscript regarding mélange rigidity.

- Thank you for the commentt, this formulation no longer appears in the revised Conclusions.

Line 416: "early in the season" – specify this means melt season.

- Thank you for the comment, this formulation no longer appears in the revised Conclusions.

Line 418" "undercutting and thus calving" – see my comment above regarding this and being specific about the direction of the relationship

- Thank you for the comment, text edited, lines 510–512.

Typos and very minor edits

- Thank you for the very thorough review, we have edited the text accordingly where it has not been otherwise revised.

Line 37: remove "rapidly calving" (as the fjord is the subject)
Line 38: change "ocean model" to "ocean circulation model"
Line 46: change "Western" to "West"
Line 47: "fastest"
Line 54: "year, however expendable" should be "year; however, expendable"
Line 78: switching between km's and km. I don't really mind which is used, "km" is probably more common, but be consistent.
Line 79: The sill is 5 km from the western boundary?
Line 79: change "in front" to "west"
Line 82: "monthly conditions" à "monthly condition"
Line 87: check the formatting for reference to Motyka et al. (2011)
Line 103: "Mid-July" à "mid-July"
Line 110: Change "accounted for" to "partially accounted for" or similar. Also change "boundary forcing" to "idealized boundary forcing"
Line 111: Change "of Disko Bay" to "provided at the open boundary, which in our simulations represent the seasonal changes observed in Disko Bay"
Line 113: Please specify the width in km of the sponge layer.
Line 115: please state what month this profile was obtained (it is stated in Fig 1, but it would be clearer to state it here too)
Line 131-133: Specify whether the iceberg concentration is uniform along the fjord
Line 164: smaller than what?
Line 172: Please provide a value for the freshwater flux
Line 170: remove "out"
Line 174: suggest changing "slight" to "weak"
Line 176: should be "in NoIBP"

Line 178: suggest "increases iceberg melt rates"
Line 179: "increases the inflow" – can you quantify this?
Line 190: as above, the use of "entrainment" here is not very clear. There are some other instances of entrainment in a similar manner later in the manuscript – please address them all.
Line 190: see comment above re ambient water vs shelf water
Line 196: clarify that this melt rate decrease is relative to the NoIBP simulation
Line 197: space in "watercolumn" needed
Line 253: "GWM" should be "GMW"
Line 258: semi-colon required before "however" (here and elsewhere)
Line 283: "Illustrated" should be all lower-case
Line 290: "iceberg induced" à "iceberg-induced"
Line 316: "high silled" à "high-silled"
Line 394: "impacta" à "impacts"
Line 415: "glacially modified" à "glacially-modified" (here and throughout)

Figures

The results figure locations all seemed a bit late to me, usually many pages later than their associated bit of text.

- Thank you for the comment, this will be fixed once the paper will be formatted.

Figure 1: The black box in the inset is hard to see The colour bar blends in too much with the background – consider moving to one side of the figure, and make the text larger The green diamond is not very clear – consider yellow or magenta? BedMachine version 4 is now available – consider replotting the figure with this How are the sill and calving front defined? Is there a date for the calving front position? (present day will obviously not age well)

- Thank you for the comments, we have made adjustments to the figure. We checked with Mathieu Morlighem that there are no significant updates to the bed in this location, and keep BedMachine version 3.

Figure 2: The text, especially tick labels and axis labels, are a little hard to read at 100

- Thank you for the comment, label sizes edited.

Figure 3 and other along-fjord transects: please state in the figure caption whether these are centreline profiles or across-fjord averages (or something else?).

- Thank you for the comment, we have now included this in the captions.

Figure 3: panel (k) contains observations from a fairly small number of casts (assuming Figure 1 shows all relevant casts). Please explain somewhere, in the figure caption or methods, how these observations are interpolated to produce the data plotted in this panel?

- Thank you for the comment, we have now included this in the caption.

Figure 3, 4 and 9: a continuous colorbar is shown for each of these figure; however, the colours in the panels appear to be contoured, or at least represented with many fewer colours than shown in the bar. Please either provide a colorbar that is representative of the data as plotted, or plot the data using continuous colours as shown in the current colorbar of each figure.

- Thank you for the comment, we have added the contrours to the color bars.

Figure 8: I found the red star a bit confusing, as initially it looks like it could be a data point.

- Thank you for the comment, we have removed the star.

Figure 9: depth/y-axis not shown, despite being referred to in-text.

- Thank you for the comment, we have added the y-axis.

References

Davison, B. J., Cowton, T., Sole, A., Cottier, F., and Nienow, P.: Modelling the effect of submarine iceberg melting on glacier-adjacent water properties, The Cryosphere, 16, 1181–1196, https://doi.org/10.5194/TC-16-1181-2022, 2022.

Jenkins, A.: Convection-driven melting near the grounding lines of ice shelves and tidewater glaciers, Journal of Physical Oceanography, 41, 2279–2294, https://doi.org/10.1175/JPO-D-11-03.1, 2011.

Straneo, F. and Cenedese, C.: The Dynamics of Greenland's Glacial Fjords and Their Role in Climate, http://dx.doi.org/10.1146/annurev-marine-010213-135133, 7, 89–112, https://doi.org/10.1146/ANNUREV-MARINE-010213-135133, 2015.