# Reply to Reviewers

Impact of icebergs on the seasonal submarine melt of Sermeq Kujalleq
Karita Kajanto et al. 2022

# 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.

## 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.

## RC2: 'Comment on tc-2022-136', Anonymous Referee 2

### Summary

This manuscript presents a study into the effects of icebergs on the circulation and water properties within Ilulissat Isfjord, a major ice-choked fjord in western Greenland. The study utilises the recent 'IceBerg' package developed for MITgcm (Davison et al 2020, 2021) to simulate the evolution of the fjord with and without icebergs, with the results then compared to (sparse) available observations. It builds upon the two earlier Davison papers in its application of the model to a new fjord system, and one of particular significance to this subject due to its exceptionally high ice concentration and proximity to Greenland's largest outlet glacier.

The manuscript makes a useful contribution to the growing literature on Greenland's fjords, clearly demonstrating the potential for icebergs to strongly modify fjord processes, and elucidating some of the mechanisms through which this can occur. I have one major comment which related to the experimental design which needs to be addressed to allow clear and confident interpretation of the presented results. Beyond this, I have some specific questions on aspects of the model set up and quite a long list of further comments, questions and points of clarity. Finally, I have attached a separate PDF with typological issues highlighted.

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

### Major comments

#### Experimental design

The experiments are run from initiation in March through to August, at which point modelled fjord conditions are compared with observations. While this shows the transition from winter through to summer conditions, terminating the experiments in August seems premature and makes it difficult to assess the modelled evolution of the fjord. In particular, it is not clear how fjord water properties would continue to evolve beyond high summer, and if and how they return to something like the initial conditions in time to undertake this evolution again.

The fjord undergoes freshening and cooling over the duration of the model run, reaching something approximately resembling the observations by the end of the run in August. It's not clear though whether the August temperatures are the end of the journey (with the fjord existing in a new quasi equilibrium state similar to the experiments by Davison et al 2022), or whether the modelled fjord would actually continue to cool and freshen into the autumn and winter if the model was allowed to run on. If it's the latter, this implies the fjord is periodically returned to something resembling the original conditions by some other mechanism, only for the icebergs to resume the cooling process.

- Originally, we did not include the latter part of the runoff season, due to a lack of observations to compare the model with, and the general uncertainty relating to the autumn/winter season. We do see your point in running the model longer as a modelling exercise, and now include the latter part of the runoff season. The results demonstrate how density-driven inflow over the sill becomes dominant as subglacial discharge decreases. This behaviour is similar to the observational findings from LeConte glacier in Alaska (Hager et al., 2022), providing credibility to the model results. The results from the late season are included throughout Sections 4 and 5 and are presented in Figs. 3–6.
- We did run the model forward through a second winter season (Fig. S2), and we do see that the model starts to recover towards the original density during the winter, not quite reaching it, and temperature not recovering. While we feel that the model captures the general features of wintertime forcing and provides general understanding of the circulation regimes of the system, the model is not designed to include processes that might become relevant during winter, such as storm surges, sea ice formation and iceberg volume decrease. Therefore, we do not think that the validity of our results for the runoff season is impacted by a discrepancy between the intial state and the following winter.
- We performed an additional wintertime test (presented in Fig. 1 at the end of this document), where the iceberg distribution switches to *IB200* mid-winter, to simulate melting of deepest icebergs (Enderlin et al. (2016) show the decrease in iceberg draft during winter). The switch triggers temperature increase, since in the absence of deep icebergs no more IMAW is formed, and the basin gets filled with umodified Disko Bay water at sill depth. This test highlights the need to include the decrease of iceberg volume during winter.

I think this is important for at least two reasons. Firstly, it is has implications for the validity of the comparison of model results with August observations. If it is necessary for the fjord to start off warmer

/ saltier at the start of the melt season in order to approximately match observations by August, then the difference between the applied initial conditions and the summer observations becomes critical in determining whether the model matches the observations. As the winter profile (initial conditions) comes from 2018, it's not clear whether this does represent an appropriate starting point relative to the summer observations (which come from 2014). There is also no evidence provided to show that the assumption that winter conditions inside the fjord simply match shelf water properties at sill depth is appropriate. If water properties inside the fjord were already cooler/fresher than those at sill depth at the start of the melt season, would they end up even cooler/fresher by August (in which case the mismatch with observations will increase)?

- We base our summer boundary condition to a single observed profile from August 2014, which is not necessarily a good representation of the densest inflowing water. Since we compare to observed profiles within the fjord, we need to make sure that it is theoretically possible for the model to reach the observed properties. This is why we adjust the properties at the sill. The winter observations from 2018 extend only to 150 m depth, below which we join the simulated winter profile with the summer one. Thus, at sill depth the properties are the same, as are the adjustments we make, leading to no difference between winter and summer at sill depth. We have revised the description of the boundary condition on lines 141–150.
- The general understanding originating from Gladish et al. (2015) is that the fjord basin is filled with Disko Bay water at sill depth, with no significant renewal of water otherwise. We acknowledge that this study, as all other studies of this fjord, is from summer, but we have used this assumption as a starting point. This is explained in lines 82–85. We are not aware of any winter observations extending down to the deep basin from this fjord.
- As for running a spinup for over one year, unfortunately this is not practically possible with IceBerg package due to the very long computation time. In addition, a spinup extending over winter would need to address the variability of the iceberg distribution, as discussed in our earlier reply regarding the following winter. However, we see a dedicated winter model including relevant processes as an important future study.

Secondly, it raises interesting questions about the key processes and the seasonality of fjord water properties (which is a key aspect of the paper and the focus of the title). In the way the experiments are set up, you are implicitly assuming that there is an annual cycle of winter warming and summer cooling within the fjord. It's not clear though from the evidence presented whether this is correct or whether given sufficient time a relatively consistent offset between shelf and fjord temperature/salinity would be established.

- We are not assuming any seasonality below sill depth, as properties at sill depth (grid cell where the densest water is able to enter the basin) are kept constant throughout the run. The seasonality of the boundary condition only applies above sill depth, and due to smaller density, these waters do not enter the deep basin. Thus, all changes in the deep basin properties presented in the results are due to other processes than changes in the boundary condition. We have revised description of the boundary conditions to make this more clear in lines 141–150.

Without further investigation / discussion of these points, it's difficult to assess the validity of the seasonal evolution of water properties, which is a key aspect of the study as it is currently presented. This could be partially addressed by running the experiments at least until the end of the melt season (which is I think fairly conventional for studies of seasonal processes) so that we can at least assess whether cooling and freshening continues beyond the August observations. I think though that at least one scenario should be run for $> 1$ year to allow for a proper spin up, reduce the dependency on the initial conditions and allow you to look into the questions of seasonal cyclicity raised above. If results diverge from those presented for the first melt season, then this raises an interesting discussion over whether other processes are serving to balance the impact of icebergs over this timescale. Finally, you could also run the model with a few steady discharge values to see what equilibrium conditions are eventually reached (similar to Davison et al, 2022). This would help to reduce the temporal dependency in the results, and allow you to assess what the impact of a given discharge and iceberg concentration would be on the fjord in the longer term. This would help to assess how far down this path the fjord has progressed on the timescale of the experiments presented here.

- We have revised the Results section, now including also the late season, included in Figs. 3–6, and also present the results from following winter in the Supplementary Figures S2 and S3.

**Model details**

There are a few points in the paper where the description of model function differs from that in the original paper describing the IceBerg package by Davison et al. (2022). This raises questions over whether the model is being accurately described (or whether it has been modified), and if the results accurately interpreted.

- Thank you for the comment. We have not changed the IceBerg package, nor has it been our intention to describe it differently. In the revised manuscript we have improved our description of the model, please see our replies to the specific comments below.

L125-126. This sentence describes how ice adjacent currents are calculated, but it doesn't explain the representation of iceberg drift in the IceBerg package. According to Davison et al (2020, p10) 'Melt rates derived using the velocity-dependent three-equation formulation are sensitive to the current velocity at the ice-ocean interface. For icebergs, this is the difference between an iceberg's drift velocity and the ambient water velocity at any given point on the iceberg. In ice mélange—a dense matrix of icebergs and sea ice often found adjacent to large tidewater glaciers—iceberg motion is typically slow relative to the surrounding currents; therefore, in this region of our domain, we assume the icebergs are fixed in place. Elsewhere in the domain, we calculate iceberg drift velocity as the average water velocity from the fjord surface to the iceberg keel depth (but we do not use this to update the location of each iceberg) ... We calculate the submarine melt rate of every face on each iceberg individually at each model vertical level using ice-parallel current speeds (relative to the calculated drift of the iceberg).' This raises the question of whether or not drift is included in the present simulations? For a fjord like Ilulissat Isfjord, where dense melange is widespread, it may be more appropriate not to include drift?

- Thank you for pointing this out, we have now corrected the sentence, please see lines 156–159. We have not changed the code, and are including the effect of drift. As pointed out in this comment, there are arguments to both including and excluding drift. Since in 2014 there was relatively short and scattered episodes of rigid melange and none during the melt season (Joughin et al., 2020), we assume that icebergs were relatively mobile this season, and thus we see it more appropriate to include drift that to exclude it. Furthermore, Fig. S3 demonstrates that water velocity is slow within the extent of most icebergs due to the deep GMW, causing the drift speed to be small (0.02–0.1 $ms^{-1}$), which is comparable to the assumed background velocity of 0.06 $ms^{-1}$.

L152-154. Unless it has been edited, I think the plume in IcePlume is programmed to terminate upon reaching neutral buoyancy rather than zero momentum (see Cowton et al, 2015).

- Thank you for pointing this out. There was a mistake in our diagnostics calculation, which has now been corrected and we use the outflow from the plume to the model grid as an indicator for the neutral buoyancy depth. This does not change the results nor the interpretation.

L398-399. This line states that icebergs do not present an obstacle to flow in the model, but according to Davison et al (2020, p10), 'As well as drifting with ocean currents, icebergs also act as a barrier to water flow. We represent this effect using partial cells within MITgcm—essentially forcing a portion of some of the cells to be 'dry'. The fraction of the cell that is dry is equivalent to the proportion of the cell volume occupied by icebergs. In this way, the blocking effect of all of the icebergs in a cell is represented using a single value, rather than representing individual icebergs as solid bodies within grid cells.' So unless the package has been modified, it seems the icebergs should be exerting a physical obstacle to flow in these simulations (albeit a simplified one based on a cell averaged approach)? This seems to be noted on L127-128.

- Thank you for pointing this out. We have not modified the code of the IceBerg package. As described in the attached citation, the IceBerg package does accoung for the volume that icebergs uptake. However, we wish to point out to the reader that this approach does not fully encompass changes to flow due to an iceberg distribution, since it does not force the flow to change direction, as in Hughes et.al., 2021. While we do believe that the approach of the IceBerg package is sufficient for our type of study and the results from Hughes et.al., 2021 indicate that a single obstacle could be a reasonable representation, we wish to point out to the reader the complexity of flow within an iceberg network. We have revised the formulation on line 487–488.

## Other specific comments

### Introduction

L18. In what sense are the glaciers 'controlled' by the geometry and stratification?

- Thank you for the comment, this point is now removed from the revised introduction, since it is not necessary to our study.

L19-22. I feel this would benefit from a little elaboration. How does the ice-ocean interface create uncertainty in sea level contribution predictions?

- Thank you for the comment, we have revised the text, lines 20–22.

L26-7. Be more specific - do you mean that calving is reduced when dense ice melange is present?

- Thank you for the comment, the Introduction is revised with reference to comments from Reviewer 1, and now includes a better description. Please see lines 40–44.

**Methods**

L80. Why not include Coriolis force? This is discussed later, but should be justified here.

- Thank you for the comment, text edited, line 101.

L81-82. Why not vary the runoff smoothly by interpolating between monthly values? I think this is the default set up for MITgcm, and would prevent unrealistic step changes.

- Thank you for noticing, that is indeed what we do, we have revised the text.

L92-93. I don't quite follow: if water is draining through the shear margins, wouldn't this suggest drainage close to the lateral margins rather than across the full width of the glacier?

- Thank you for the comments regarding plume width. We have rearranged the text to better convey our justification, see lines 104–125. The choice of a 1.2 km plume is a middle-ground assumption for the entire runoff season. We expect the degree of channeling to vary within the season (Cook et al., 2021), and potentially outlets to form also close to the shear margins (Cavanagh et al., 2017). We assume that over the season, discharge is somewhat distributed on average, and our choice of a wider plume width represents this.

L94. Why 1.2 km? I appreciate plume width is hard to constrain but why this value in particular? It seems very wide compared to the sort of values that are normally used (e.g. a recommendation of 200 m by Jackson et al 2017). Without further justification, it gives the impression it was chosen to give the best fit to observations – if this is the case, it should be stated.

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

L99. As above - a 'narrow' plume of 400 m wide is still wide by conventional standards (e.g. Jackson et al 2017, Slater et al 2022).

- Thank you for the comment. Since the grid resolution in our model is 400m in the y-direction, we are restricted by this resolution regarding the narrowest plume.

L102-103. It's unlikely that subglacial discharge at a glacier like Jakobshavn is  0 in the winter months - it will be a lot smaller than summer but with such a large catchment and such high sliding velocities there will likely be a non-neglible winter discharge of subglacially-derived meltwater. This would affect the result that in the NoIBP scenario there is no circulation in the deep basin before May. It's very hard to quantify subglacial melt rates, but it might be worth trying using a discharge of a few m3/s in the winter to see if this has a noticeable effect on results.

- Thank you for the comment. It is indeed true that there is very likely some small amount discharge during winter in this glacier. We have now noted this on lines 129–130, and also in the Discussion on lines 475–477. As you point out, the difference would in this case be similar to the difference from April to May in our results, which is a relatively small difference and has little implications to our results.

L102-103. What is the justification for such a large lag time? In a pressurised drainage system, there should be almost no lag between input to the system (i.e. surface runoff) and output from the system (i.e. subglacial discharge). There will be some subglacial storage which will serve to smooth the peaks, but this wouldn't cause the peak to be displaced by several weeks. For example, Mankoff et al. (2020) assume instantaneous routing between runoff and outlet discharge, and find good agreement in the timing of discharge peaks with observations (with a 7 day smoothing applied).

- Thank you for the comment. We point out that our subglacial discharge forcing is synthetic, and we chose to keep the forcing as simple as possible. Since we assume a gaussian temporal distribution, and assume May to November to be the runoff season, it follows that the peak will be in August. We have revised the text to point out that the forcing is synthetic, lines 128–129.

L103. What is the peak discharge of 1200 m3/s based on?

- Thank you for the comment. The peak is and estimate from (Enderlin et al., 2016), as cited in the text.

L114-116. Give the years and dates of these data here – presently this is only stated in the SI.

- Thank you for the comment. We have revised the description of the boundary condition based on comments from both reviewers, now including also the years, lines 141–150.

L117-118. I don't follow - why modify the forcing in this way (and at what point in the seasonal cycle were these values obtained?)?

- Thank you for the comment. We base our boundary condition to a single observed profile, that is not necessarily a good representation of the densest inflowing water. Since we inted to compare to observed profiles within the fjord, we need to make sure that it is theoretically possible for the model to reach the observed properties. The observed profiles both in and outside of the fjord are from 15th to the 16th of August, 2014 (Beaird et al., 2017). We have revised the description of the boundary condition on lines 141–150.

L124. The description that 'Melt and negative salinity flux are computed' seems odd. What about heat? Should it state that melt rates, and thus salinity and heat fluxes, are calculated?

- Thank you for the comment, we have revised the text, lines 154–155.

**Results**

L148. See earlier comment regarding winter discharge.

- Thank you for the comment, please see our earlier reply regarding winter discharge.

L163-4. This is the case in the model (melt in areas of low current velocity is poorly constrained in iceplume), but there needs to be a bit more comment on whether or not this is deemed realistic – recent research suggests there should be much less discrepancy between melt rates within and outside of the plume area (Jackson et al. 2020; Sutherland et al. 2019).

- Thank you for the comment, we point this out in the Discussion, lines 498-500.

L165-7. I don't follow. The deep basin starts off colder than the shelf (due to the initial conditions) but seems to be steadily warming through the summer due to inflow over the sill (shown in Figure 3/4)?

- Thank you for the comment. The deep basin is initialized with ambient water properties at sill depth since that is the densest water accessing the basin (see Gladish et al. (2015)). Early-season refluxing of GMW causes the basin water to cool by up to 0.2 degC during the summer, compared to the initial basin properties, as best seen in Fig. 8a.

L167-8. Ambiguous - do you mean it is 2 C cooler than the equivalent Disko Bay temperature?

- Thank you for the comment, we have reformulated the sentence, line 199.

L177-8. Keep in mind this is likely underestimated due to poorly resolved boundary processes (see earlier comment)

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

L176-6. This doesn't sound very likely - even the plume melt rate in figure 5 only reaches 4.5 m/d in August (Figure 5). Is this a mistake, and if not where is it shown?

- Thank you for the comment, sorry this was freshwater flux, not melt rate, lines 219–220.

L201. See earlier question on 'drift'. And if drift is turned on, how do you distinguish between 'drift induced' melt and melt due to the flow of water past the berg? (Given that the net flow velocity past the berg is the difference between the drift velocity and the current speeds at any given depth).

- Thank you for the comment, we mean velocity-induced. However, this statement no longer appears due to a comment from Reviewer1.

L210. It's also notable that they result in a much larger export of freshwater and GMW from the fjord.

- Thank you for the comment, this is true, however, not relevant to our main conclusions.

L218-9. How is the GMW outflow identified in the observations? Is this based purely on the T-S properties?

- Thank you for the comment, yes the interpretation is based on the CTD-data only, see Beaird et al. (2017) for further analysis. We have revised the paragraph describing comparison to observations, lines 277–287.

L220-2. Again, how is this determined? This section would benefit from a little elaboration.

- Thank you for the comment, we have revised the comparison to observations, lines 277–287.

L237. Does ‘entrainment of GMW’ here just mean entrainment of outflowing GMW into shelf water flowing inwards over the sill, or does it also include recirculation of GMW where the plume termination depth is deeper than sill depth? (The latter being perhaps a slightly different thing to ’entrainment’).

- Thank you for the comment, we have revised this paragraph based on comments from Reviewer1, lines 289–300.

**Discussion**

L268-9. The fjord in the IBP scenarios gets steadily cooler and fresher below 200m over the course of the summer (Figure 8). Does this trend continue if the simulation is allowed to run on for longer (into autumn and winter), such that model results and observations continue to diverge, or is a new equilibrium reached? See earlier major comment.

- Thank you for the comment, please see our reply to the major comment and the new late-season results.

L271-2. How is this quantified?

- Thank you for the comment. This is achieved by comparing the mid-fjord profiles of both *IBP* and *NoIBP* to the summer Disko Bay boundary condition.

L275-8. Muilwijk et al (2022) show that subglacial discharge represents a small fraction of GMW, but that upwelling of AW by the plume is very important in the formation of GMW. The role of plumes doesn’t seem to be properly captured by this sentence.

- Thank you for the comment, we are discussing here the small contribution of suglacial discharge to the cooling, while we do not expect upwelled AW to contribute to cooling.

L278-9. Could this be tested based on whether the difference between the modelled and observed properties sits on a melt or runoff mixing line?

- Thank you for the comment. Outside the range of Fig. 7 the observed profiles do turn to align with runoff eventually, but they do also have a significant contribution of melt close to the surface. So, there is for sure a runoff-contribution, but some contribution from small icebergs is also likely, as stated in the text, lines 344–347.

L294-5. It would be valuable to compare other aspects of the results to Davison et al (2022) as well, given the similarity in these studies. Davison et al used an idealised domain to investigate the impact of icebergs across a parameter space representing the diversity of Greenland’s fjords. As Ilulissat Isfjord represents one end member of this range, it would be valuable to examine how closely it aligns with the predictions of Davison et al.

- Thank you for the comment. See comparison on lines 372–388.

L299-300. Jackson et al. (2017) propose that a line plume of 200 m width gives best agreement with their observations – this is much narrower than the tested range of 400-4000 m, so it doesn’t really justify the choice of plume widths used in this study (see earlier comment).

- Thank you for the comment. Please see our reply to the plume width earlier. We have reformulated the discussion so that this reference no longer appears here.

L331-2. Should qualify that this is true over the parameter space considered

- Thank you for the comment, we have included the fluxes, line 407.

L340-2. I don’t follow - wouldn’t the change in geometry due to undercutting make icebergs more likely to rotate top-first into the fjord?

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

L353-5. I feel this needs some substantiation. The currents in question are a summer phenomenon, whereas rigid sea ice format occurs towards the end of winter. Would a weaker plume during one summer really affect sea ice formation the following winter / spring? I’m not saying it’s impossible, but it seems highly speculative and would benefit from stronger justification.

- Thank you for the comment, we have reformulated this part of the discussion, also in reference to comments from Reviewer1, see lines 418–433.

L357. Need to be more specific with terminology, to make clear you are talking about recirculation of deep waters rather than some other form of entrainment. Same for 'iceberg modification'.

- Thank you for the comment, we have reformulated the discussion so that this statement no longer appears here.

L361-3. This is broad statement which doesn't really do justice to the rich literature on calving, including on the impact of undercutting on calving (e.g. O'Leary and Christoffersen 2013; Ma and Bassis 2019; Benn et al. 2017; Slater et al. 2021).

- Thank you for the comment, we have reformulated this paragraph, please see lines 409–419.

L377-382. The comparison of model results and observations hinges on implications of the experimental design, and may need to be reconsidered (see earlier major comment).

- Thank you for the comment, we refer to our earlier reply regarding the experimental design.

L381. It is hard to compare these two plots. Could additional curves (or even additional plots) be added to Figure 8 to allow comparison? (Also, there is no Figure 3m).

- Thank you for the comment, we have considered adding isopycnals to the figure, however, this led to more confusion than excluding them.

L385. Why not vary this smoothly (see earlier comment)?

- Thank you for the comment, we refer to our earlier reply.

L392. As earlier, is this 'drift induced', or is it water flowing past stationary grounded icebergs?

- Thank you for the comment, this paragraph is reformulated and this statement no longer appears.

L392-4. I'm not sure I follow this. I would assume that freshening causes upwelling and outflow of GMW, and that it is entrainment into this flow that drives subsurface inflow to the fjord (as well as entrainment into the main plume)? In which case it's not obvious to me why the rate of entrainment/inflow should be greater due to the negative salinity approach. I can see that if you used a real freshwater flux from the melting icebergs this would increase the outflow, such that there was a net outflow from the fjord (equal to the meltwater flux), but it's less obvious to me why this would reduce the inflow to the fjord in absolute terms. If you used a real freshwater flux, there would also be a question over whether it is appropriate to add a physical volume of meltwater whilst not simultaneously decreasing the volume of the fjord occupied by icebergs, as the two should approximately balance each other out.

- Thank you for the comment, this is indeed what we intend to discuss. We want to point out that in reality there should be a net outflow of iceberg meltwater over the sill, and in this context the validity of the negative salinity approach reqires further study. We have revised the formulation, lines 480–482.

L406-7. Given that the results presented already over-estimate cooling and freshening, this raises further questions over why the impact of icebergs in the model seems to overestimate the impacts of icebergs, and should probably be referenced in this context.

- Thank you for the comment, we have expanded the discussion on deep basin cooling and freshening, lines 457–469.

L410-1. Have 'misleading interpretations' been presented in the paper? If not, perhaps better to simply state that it's important to take icebergs into account when studying and simulating these systems.

- Thank you for the comment. We have presented the diverging interpretations of deep basin water modification by Gladish et al. (2015) and Beaird et al. (2017) on lines 366–371, and explained that this difference is due to the lack of icebergs in (Gladish et al., 2015), and also due to the missing concept of IMAW as separate from GMW.

L416. 'Entrainment' is unspecific – clarify the process in question.

- Thank you for the comment, this sentence no longer appears in the conclusions.

L416-8. This sentence is hard to follow. Also the influence of discharge on the plume and frontal melt rates has been demonstrated in many other places (more so that here, where it isn't really the focus of the paper).

- Thank you for the comment, we have reformulated the sentence, lines 508–509.

L419. Need to be specific about the mechanism here - is it purely due to changes in the stratification?

- Thank you for the comment, we have reformulated the sentence, lines 509–513.

L421. This seems to be overstating things given the speculative nature of this connection. A 'potential link' would seem more appropriate.

- Thank you for the comment, we have reformulated the sentence, lines 511–513.

See annotated PDF for further minor corrections.

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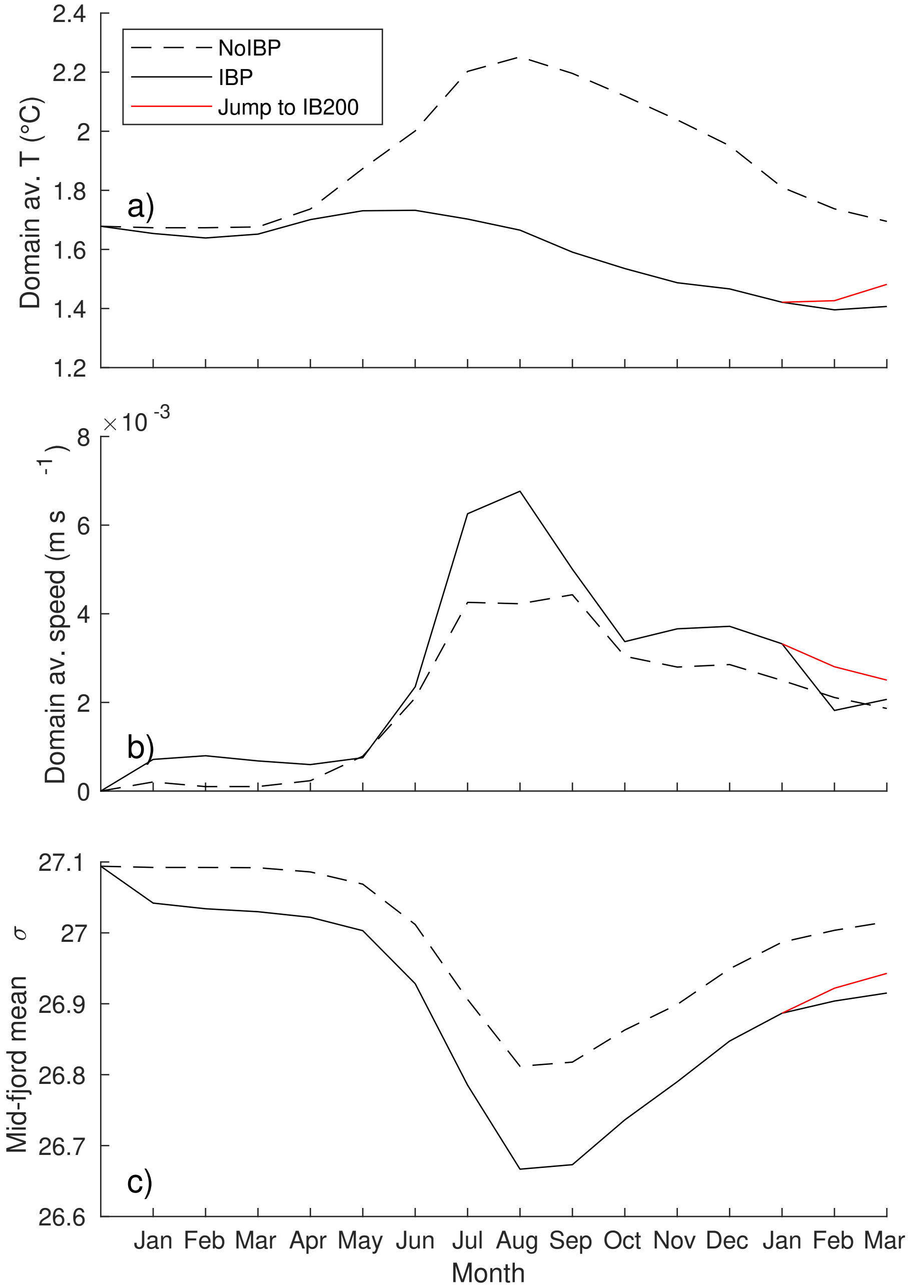


Figure 1: Time evolution throughout the model run of a) domain averaged temperature, b) domain averaged speed and c) vertically averaged potential density at mid-fjord location for experiments with and without icebergs (*IBP* and *NoIBP* respectively). Red line indicates the response to mid-winter change of iceberg distribution of *IB200*, to test the response of the model to shallowing of icebergs due to submarine melt during winter.