Authors' Response to Reviews of

Impact of the Nares Strait sea ice arches on the long-term stability of the Petermann Glacier Ice Shelf

Abhay Prakash, Qin Zhou, Tore Hattermann, and Nina Kirchner
egusphere-2023-73, doi.org/10.5194/egusphere-2023-73

RC: ***Reviewers' Comment***, AR: Authors' Response, ☐ Manuscript Text

1. Response to Reviewer #1

AR: Our response is structured as follows:

- Reviewer's Comment (RC) in bold italics.
- Authors' Response (AC) in regular font.
- Changes to be made to the manuscript following RC are enclosed in a text box. The manuscript line number and the line to which the new text will be appended is highlighted in blue. The appended text follows in black (regular font).

1.1. Overview

RC: ***This paper presents results from a set of experiments examining the impact of changing sea ice cover in Nares Strait on the transport of Atlantic Water towards the Petermann glacier ice shelf. The results show that changing from a landfast sea ice coer to a thin and mobile one in Nares Strait causes upwelling of Atlantic Water at the mouth of Petermann Fjord, thus increasing heat transport towards the glacier. The increased heat transport into the fjord in turn increases the melt of the ice shelf and increases circulation in the fjord. This study demonstrates how changes in the water column along the coast of Greenland propagate into the fjord and directly impact glacier melt.***
This study makes a useful contribution to the literature on circulation in the glacial fjords of Greenland, particularly since the model setup enables to study the impact of relatively far-field changes to the fjord. However, I think there are four main issues that need to be addressed to in order to clearly communicate and confidently interpret the results. In addition, I have quite a few minor comments and suggestions on how to improve the text and the figures.

AR: We are very grateful, and thank the reviewer for taking the time to carefully evaluate our manuscript and for providing constructive feedback. We have made changes to our manuscript following your suggestions, which we believe have resulted in significant improvements. We hope that you find these modifications satisfactory.

1.2. Major comments

RC: ***Major comment 1: Description of the model:***
The model setup is not accurately described in the Methods-section. The reader should get sufficient information of the included model physics from this paper, not having to refer to the author's previous publication describing the development of the setup. Currently, the Methods-section does not describe

the included model physics, particularly which processes regarding sea ice and the ice shelf are included or excluded. Is the melt parameterized with the conventional three-equation formulation by Jenkins? Is surface runoff excluded? The fact that subglacial discharge is missing is only coming across in the Discussion. That is a key piece of information that should be explained and justified in the Methods, and thoroughly discussed in the Discussion, see the next major comment.
The flow of the experiments in this study should be (and partly is but not consistently), landfast to mobile to thin mobile, which represents the order of expected future changes in the sea ice cover (see a minor comment on the naming of the experiments). However, the model is initialized from the thin mobile experiment, apparently since this experiment was available from a previous study. This raises the question if the model has stabilized after changing the sea ice conditions, or if the thin mobile -regime still has an imprint on the hydrography. Model stability should be demonstrated, and this could be achieved, for example, by showing domain average temperature, flow speed and specific density through the entire run time of the experiments. This stabilization could be in the supplement and only briefly referred to in the main text, however, given how the experiments are initialized, stability should be demonstrated.
The paper is also lacking a comprehensive table of used model parameters, particularly the used turbulent transfer parameters and diffusivity and viscosity values. It is fine to have such a table in the supplement, but the information should nevertheless be available. In general, I suggest that the Methods should flow in the order of: Description of model, included physics (and justify excluded physics), domain and grid. Then the initial state and boundary conditions for the experiments with justification. Then present the differences between experiments and how long the experiments are run and the result metrics.

AR: Thank you for identifying this shortcoming, and for providing detailed instructions to help address it. We intend to rectify this by restructuring the methods section as suggested, supplemented by figures and tables which will be added to the appendix.

We intend to add sufficient information of the included model physics (and justifying excluded physics) in this paper, including a more detailed description of the ice shelf and sea ice modules. Information regarding ice shelf basal melt parameterization (following Holland and Jenkins, 1999 and Jenkins et al. 2001) and subglacial discharge are also included. A table of used model parameters will be added to the appendix, including the Γ_T, diffusivity and viscosity values used in the study. Please see suggested additions in the manuscript text box below.

We will fix the flow of experiments to be consistent throughout the study - landfast to thick mobile to thin mobile. Also, we will add a figure to the appendix, showing the domain average temperature, flow speed and specific density, to document model stability. Please see suggested additions in the manuscript text box below.

The manuscript line number and the line to which the text will be appended is highlighted in blue. The appended text follows in black.

l. 75-77: The unstructured grid, free-surface, 3-D primitive equation Finite Volume Community Ocean Model (FVCOM) (Chen et al., 2007) has recently been amended by an ice shelf module (Zhou and Hattermann, 2020) that allows modelling of oceanic processes in ice shelf cavities bounded by complex coastal geometries and fjord bathymetry. For a freely floating ice shelf with a known draft, the module modifies the horizontal pressure gradient forces to account for the effect of ice shelf basal topography as the σ-layers subduct below it. The top friction (generated by the ice shelf-ocean interface) is similar to the bottom (generated by the bathymetry), and is assumed to be quadratic (see Appendix Table 1). Thermodynamically, the ocean is assumed to be perfectly insulated from the atmosphere by the ice shelf cover, and the melting and freezing processes at the ice shelf-ocean interface are parameterized using the three fundamental equation formulated in Holland and Jenkins, 1999. The effective heat and virtual salt fluxes across the ice shelf-ocean interface are calculated following Jenkins et al. 2001, and applied as boundary conditions for temperature and salinity at the ice shelf base. Thermodynamics at the ice shelf-ocean boundary is governed by the steepness of the ice shelf basal topography and the interior ocean mixing parameterisation, and thus, the turbulent heat and salt transfer coefficients used are application specific, and are herein tethered to the PGIS draft and mixing schemes. Here, the horizontal and vertical diffusion of momentum and tracers are determined using Smagorinsky's eddy parameterisation (Smagorinsky, 1963) and the Mellor and Yamada level 2.5 (MY-2.5) turbulent closure model (Mellor and Yamada, 1982; Galperin et al., 1988), respectively (see Appendix Table 1).

Table 1: Turbulent transfer coefficients and mixing parameters used in this study.

Parameter	**Value**	**Description**
Z_o	1.0E-3	Roughness lengthscale
RO_{min}	2.5E-3	Roughness minimum
Γ_T	1.8E-2	Nondimensional heat-transfer coefficient
K_m	1.0E-5	Vertical eddy viscosity
P_v	1.0	Vertical Prandtl Number
P_h	1.0E-1	Horizontal Prandtl Number
H_c	1.0E-1	Scaling constant

l 77-80: Further, a new sea ice module (*Ice Nudge*) was implemented (Prakash et al., 2022) that allows to prescribe sea ice concentration (A_i), sea ice thickness (h_i), sea ice salinity (S_i) and sea ice velocities (U_i) as external surface boundary conditions to modulate the air-sea exchanges of buoyancy and momentum, and ocean dynamics. The *IceNudge* module reads in the sea ice concentration to determine where to modify the ocean surface fluxes. The difference in velocity between the sea ice and ocean is used to compute the stress exerted at the sea ice-ocean interface (Hunke et al., 2015), which is combined with the wind stress at the sea ice free part to modify the surface stress in the presence of sea ice. In its current implementation, several assumptions are used in the calculation of thermodynamical fluxes. The annual cycle of Arctic sea ice mass budget is largely characterized by basal growth and melt processes, and where lateral melt accounts for less than 10% of the total sea ice melt (Tsamados et al., 2015; Keen et al., 2021). We assume there is no snow, and the contribution from lateral melt is not included. Further, the surface (2-m) air temperature is used to set the sea ice surface temperature, and the sea ice is fresh when calculating the conductive heat flux and freshwater flux. The depth of the uppermost model layer is treated as the mixed layer depth, and is a reasonable assumption (Peralta-Ferriz, C. and Woodgate, R.A., 2015). The energy balance between conductive and oceanic heat flux at the sea ice-ocean interface is used to determine sea ice basal growth or melt. The two equation sea ice-ocean boundary approach is used, where the oceanic heat flux is parameterised based on the Reynolds averaged turbulent heat flux, and where the surface freezing temperature of seawater is a linear function of the mixed layer salinity. Additionally, heat content of melted water and shortwave flux through sea ice are combined to derive the net heat flux at the sea ice-ocean interface. The net heat flux and shortwave flux at the sea ice-covered part are combined with the corresponding fluxes over open ocean, and these combined fluxes are used as boundary conditions for the temperature equation (see Chen et al., 2007 and Prakash et al., 2022). In the sea ice formation and growth phase, the total oceanic heat available for both basal and frazil ice freezing at the sea ice covered and sea ice free ocean, respectively, are computed, and is equated to the amount of sea ice being formed via the latent heat of fusion. A virtual salt flux approach (Jenkins et al., 2001) is used to compute freshwater and salt fluxes, which are summed up as a boundary condition for the salinity equation.

l 81-82: For a detailed description of the model setup and a standard run (including selected results from it), see Prakash et al. (2022). We note that the model in its present configuration does not feature subglacial discharge at the grounding line. While it is known to impact fjord-scale circulation and basal melt rates at Petermann (Cai et al., 2017), the aim of our study is to distinguish the impact of long-term changes in regional sea ice cover on PGIS basal melt. Thus, we posit that the mechanisms identified here in response to a changing sea ice cover, and the associated anomalies (departing systematically from a baseline experiment) in basal melt, are robust regardless of the omission of subglacial discharge. Nonetheless, conjectures regarding its omission on our results are presented in the discussion section.

l 133-134: ..to generate the tidal forcing (see Prakash et al., 2022 for details). The domain averaged temperature, flow speed and specific density are shown (see Appendix Figure 1) to document the stable model solution of the Thin Mobile run (as on January 01, 2016 00:00:00 UTC) from which the Mobile and Landfast experiments (see Sect. 2.3 below) are initialised. These diagnostics are also shown for the Mobile and Landfast experiments for the entire duration (January 01, 2016 00:00:00 UTC - January 01, 2017 00:00:00 UTC) of the simulation (Appendix Figure 1). We note that the progression of the modelling procedure (i.e. the Thin Mobile experiment, which is used to create the Mobile and Landfast experiments) described here is separate from the evolution the experiments aim to present (i.e. Landfast to Mobile to Thin Mobile).

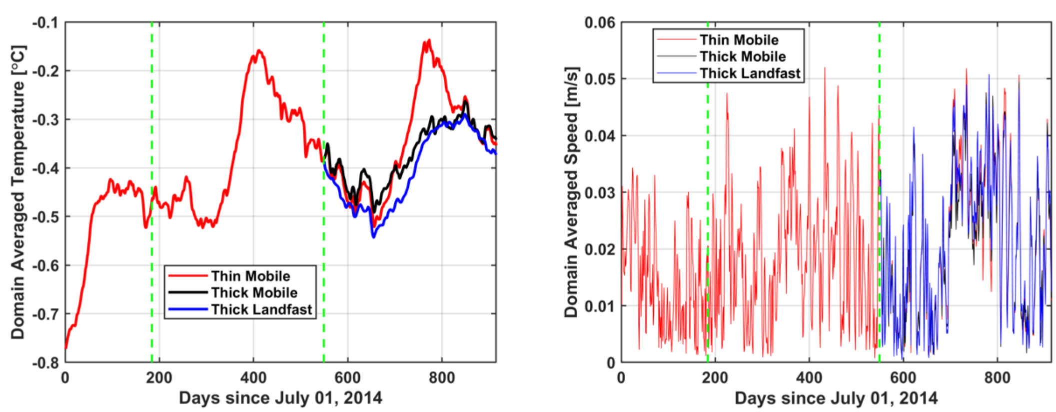

Figure 1: Domain averaged temperature (left) and speed (right) for the Thick Landfast, Thick Mobile and Thin Mobile runs. Green stippled lines represent days (since July 01, 2014) 184 and 549, which, respectively, denote the start of the 2015 and 2016 calendar year. Density plot will similarly be added.

RC: ***Major comment 2: Lack of subglacial discharge***

Since subglacial discharge is known to be a critical driver of the fjord circulation during summer (see reference list, particularly Carroll et.al., 2017), its exclusion should be clearly noted and justified in describing the model in the Methods, and implications thoroughly discussed in the Discussion. At the very least, the typical subglacial discharge volume flux should be compared to meltwater fluxes from the ice shelf (see for example Ehrenfeucht et.al., 2022 for the drainage of the Peterman subglacial system). I do see that since the ice shelf of Petermann is so extensive, subglacial discharge is likely to have a smaller role in the melt, but some attempts should be made to communicate the significance. A particularly interesting point to discuss is the implication of subglacial discharge to the modelled seasonal cycle; since discharge is only present in significant degree during summer, what does omitting it mean for the modelled range of the seasonality?

The experiments use an enhanced turbulent transfer coefficient to improve the model result. This process is not described in the paper, but only referred to the author's previous publication. However, since it seems like the turbulent transfer coefficient is used to some extent replace the missing subglacial discharge, this process and its meaning to the results should be examined in the Discussion. Overall, due to the uncertainties arising from the missing subglacial discharge, I suggest taking the focus of the results away from the numerical values of the melt rate, and focus more on the changes in the heat transport towards the glacier, and the processes driving those changes. These are in any case more directly related to the change in the sea ice cover, and relevant regardless of the questions relating to subglacial discharge.

AR: Thank you for the comment. We acknowledge that analogous to the setup of Shroyer et al. (2017), subglacial discharge is missing; a known catalyst of summer basal melting. Petermann Ice Shelf basal melt rate signal is muddied by a wide range of contributing factors; of which sea ice conditions and subglacial discharge are two. While the latter is likely a major player, the aim of our study is to distinguish (i.e. separate) the impact of long-term regional scale sea ice changes on PGIS basal melt. To that end, including subglacial discharge in the baseline run and also in the experiments is not deemed necessary, and is therefore not included as an objective for our study. Sensitivity of fjord-scale circulation and PGIS basal melt to subglacial discharge (conducted by systematically launching experiments with and without subglacial discharge) is a topic of further research. We are pleased to inform that work has been ongoing to extend the current setup to include subglacial discharge, and to create an experimental framework using appropriate datasets. Conducting such experiments with the updated model setup is the next logical step. We do agree that the missing subglacial discharge component needs to be brought to immediate attention, and to that end, we have noted and justified its omission in the Methods chapter, and as suggested, we aim to address this further by discussing its implications on the modelled seasonal cycle (see manuscript text box below).

Following comments from Reviewer 2, we have reconfigured our setup by correcting the A4 ocean boundary conditions (i.e. the cold bias) and adopting an appropriate Γ_T value that fits the corrected T-S profile. This will be addressed in the Methods chapter. In addition, we would like to clarify that the coefficient is not used to replace the missing subglacial discharge. Only few estimates of basal melt rates exist at PGIS. Those are largely representative of the annual mean melt (thus lacking seasonal response), or are acquired over sparse sampling sites over a few months for a given year (thus lacking both spatial (2-D melt map) and temporal (year-round) variability). Given the incomplete picture of the estimates and the model limitations, the objective, as far as basal melt rates are concerned, was to arrive at a qualitatively reasonable contemporary modelled melt rate (as addressed in lines 412-426). Departures from this "reference" melt rate, particularly after accounting for the bias, are numerically robust and solely reflect how long-term changes in regional sea ice cover influence PGIS basal melt, whereas factors other than sea ice, e.g., subglacial discharge and its interplay with sea ice cover changes, falls beyond the scope of this work. In addition, discussing melt anomalies without detailing the absolute "reference" melt values is not possible. However, we acknowledge and completely agree that the melt rate values presented here must be interpreted in the correct light, as the purpose of our study isn't to provide (quasi-)realistic values of PGIS basal melt (akin to remotely sensed/observational estimates). We intend to take further measures (see manuscript text box below) to make this abundantly clear to the readers, and we believe that RC-4 regarding restructuring of the discussion chapters under thematic grouping ("PGIS basal melt, uncertainty, and future outlook") will help in drawing immediate attention to it (revised structuring is presented in manuscript text box for RC-4).

> We have completely revised lines 412-426 (see below) to address the concerns that were raised above.
>
> Only few estimates of basal melt rates exist at PGIS. Spatial variability from annual mean estimates reveal basal melt rates of up to ca. 80 m/yr under the deepest regions of PGIS base, however, they mostly range between 40-50 m/yr, decaying rapidly over a distance of ca. 20 km seaward of the GL to ca. 10 m/yr (Wilson et al., 2017). Between 23 August - 08 December, 2015, Washam et al. (2020) estimated area averaged basal melt rate maxima of ca. 40-170 m/yr upstream of a location 16 km from the GL in the central basal channel, regulated by the buoyant subglacial discharge which produced strong sub-ice shelf currents. We note that these estimates are either representative of the annual mean melt (thus lacking seasonal response), or are acquired over sparse sampling sites over a few months for a given year (thus lacking both spatial (2-D melt maps) and temporal (year-round) variability). There are also inconsistencies between the model sea ice regimes and period as compared to the reported estimates (cf. Table 1 (manuscript), Kwok et al., 2010, Moore and McNeil, 2018). In addition, our experiments do not include subglacial discharge at the GL. These factors limit direct comparisons, however, it is imperative to highlight the context in which our modelled melt rate values should be interpreted, as the purpose of our study isn't to provide (quasi-)realistic values of PGIS basal melt (akin to remotely sensed/observational estimates). Given the incomplete picture of PGIS basal melt rate estimates and model limitations, the objective concerning tuning of the modelled basal melt rates (see Table 1) was to arrive at a qualitatively reasonable contemporary modelled (reference) melt rate. The annual mean modelled basal melt rates under the deeper regions of the PGIS base near the GL (Figure 6(a)) compares well with the estimates of Wilson et al. (2017), largely exhibiting values in the range of ca. 50-80 m/yr, and decreasing rapidly to ca. 10-40 m/yr at ca. 20 km from the GL. Furthermore, the modelled summer mean basal melt rate maxima of ca. 80 - 150 m/yr seen along the basal channels near the GL lies within the estimated limits provided by Washam et al. (2020) (Figure 6(a,c)). Departures from the reference values are numerically robust and solely reflect how long-term changes in regional sea ice cover influence PGIS basal melt; while other mechanisms such as melt rate increase through subglacial discharge and its interplay with sea ice cover changes falls beyond the scope of this work, but needs to be considered in addition when assessing the response of PGIS basal melt to a future warming climate. Studies have documented the role of subglacial discharge on the seasonal melt cycle and fjord-scale ocean circulation (Caroll et al., 2015; Cai et al., 2017). We hypothesize based on their findings that the discharge enhanced buoyant meltwater plumes would likely act in concert with the loss of landfast and thick sea ice cover to further strengthen the overturning circulation and entrain more AW in the PGIS cavity during summer. Thus, the modelled summer mean melt rate anomalies presented here are likely lower bounds of the true anomalies that should be expected in the presence of subglacial discharge.

RC: ***Major comment 3: Missing analysis of density and presentation of the results***
Since previous work on the glacial fjords of Greenland has so clearly demonstrated the role of buoyancy in forcing circulation (see ref. list), it is surprising that buoyancy forcing has not been considered at all in the analysis of the model results. The lack of isopycnals in the plots and the lack of fjord stratification as a background makes it difficult to understand the processes driving the changes within the fjord, to an extent that the drivers seem to be somewhat uncertain to the authors as well (line 373). I suspect buoyancy is a

key forcing in the fjord, and I think careful analysis of the changes in density both between seasons and between the experiments should be made. See previous work on circulation in high-silled glacial fjords, such as Carroll et.al. 2017, Hager et.al., 2022, Kajanto et.al., 2023.
I suggest that the authors review the presentation of the model results, since there are a lot of interesting results that are not shown in the figures; seasonal temperature and salinity in absolute terms, both in Nares Strait and the fjord, isopycnals and flow rates over the sills were not presented. I suggest including all of these, and in the minor comments below, I have included detailed suggestions on how to improve the text and particularly the figures.

AR: Thank you for the comment, and for bringing this to our attention. We have revised the text and plots following the detailed suggestions provided in the minor comments. Suggested updates to the text are provided alongside the corresponding minor comments (see Sect. 1.3), and the revised figures are presented in section 2.

RC: ***Major comment 4: Discussion in a broader context***
Currently, the Discussion considers a rather narrow range of references, and the results from this study are not put to context with the extensive literature on the circulation and heat transport in the glacial fjords of Greenland. Furthermore, model uncertainties are not clearly communicated and discussed, and without putting the uncertainties to context, the reader is left to wonder at the validity of the presented results. I suggest restructuring the Discussion thematically, clearly communicating the uncertainties, and making attempts at quantifying these uncertainties. It will add to the value of the results. I have made detailed suggestions in the minor comments below.

AR: Thank you for the comment. Following the detailed suggestions in the minor comments, we have broadened the scope of the discussion (see corresponding minor comments in Sect. 1.3). Additionally, the uncertainties have been communicated and discussed, and the discussion section has been thematically restructured (see manuscript text box below, and the corresponding minor comment in Sect. 1.3).

> 4.1 Topographic control on the mean circulation in the fjord
> 4.2 Drivers of fjord-scale circulation under a landfast sea ice cover
> 4.3 PGIS basal melt, uncertainty, and future outlook

1.3. Minor comments

RC: ***General comments regarding writing style:***
- Please review the use of tilde in the text. Tilde should in general be avoided and be replaced with text whenever possible (with words such as about or approximately). In addition, tilde is used in the manuscript in front of numerical values with several significant digits. It is unclear what is the meaning of the tilde in front of a number that is not rounded up. Also, you should not use tilde in phrases such as "up to 100 m". Review the use of tilde throughout the text and do not use it unless necessary. I have indicated some of the instances in the line-by-line comments below, but not all.
- The authors tend to describe the results from summer and winter within the same sentence by placing values and descriptive words in parenthesis. This is impossible for the reader to follow, please replace these formulations with proper sentences, such as "Conversely, during winter the heat transport..." or ", while in winter the increase in heat transport..."
- Use active language to make clear what are model results from your study ("Our model results show..." etc.). When discussing results from previous studies, always indicate if the results are from model studies or observations.
- Please review throughout the manuscript that you use a systematic notation of terms such as "high-

resolution 3-D" etc., so that the hyphens and capital letters are always spelled similarly.

AR: Thank you for the suggestions. We will address these by:
- Removing tilde throughout the manuscript, and replacing it with "ca." wherever necessary. Also, tilde or ca. will not be used in front of numerical values with several significant digits.
- Removing such difficult to read formulations throughout the manuscript, and replacing them with suggested phrases instead.
- Switching to active language throughout the Results, Discussions and Conclusions chapters to emphasize our results, and making clear "model" or "observations" when referring to literature.
- Reviewing the manuscript to ensure the use of consistent hyphenation and capitalisation throughout the study.

RC: ***Line by line comments:***

RC: ***Line 1: Title "Abstract" should have its own line.***

AR: Agreed. However, this comes from the EGUsphere preprint template, and is most likely fixed in the finalised version upon acceptance.

RC: ***Lines 1–2: The glacier is called Petermann Glacier, not PGIS. Also, it is "seasonally shielded" from what?***

AR: Thank you. The use of em dash has made this sentence confusing. We have rephrased this as follows:

> One of the last remaining floating tongues of the Greenland Ice Sheet (GrIS); the Petermann Glacier Ice Shelf (PGIS) is seasonally shielded from warm Atlantic Water (AW) by the formation of sea ice arches in the Nares Strait.

RC: ***Line 6: Do not use the manes of the experiments in the abstract, rather describe the changes "from a thick and landfast sea ice regime to a mobile and further thin and mobile sea ice regime" etc.***

AR: Thank you for the suggestion. We have fixed this as follows:

> ..the implications of transitions in the Nares Strait sea ice regime; from a thick and landfast sea ice regime to a mobile, and further, thin and mobile sea ice regime, on the PGIS basal melt.

RC: ***Line 7: "the implications ... on the PGIS basal melt". Consider if you should rephrase this to heat transport to the PGIS, see major comment.***

AR: Please see our reasoning in Major Comment 2 regarding this to keep it as is.

RC: ***Lines 7–8: "Across all three regimes ..." What do you want to say with this sentence? Are you describing seasonality? That basal melting increases during the summer compared to winter? But then what do you mean with the under deeper regions part when does melt rate increase there and relative to what?***

AR: Yes, we are describing the seasonality in all three experiments first, before exploring differences between the experiments arising from changes in sea ice cover. For eg., manuscript Figure 6 could provide a useful point of context, along with lines 257-259: *"These seasonal patterns are qualitatively similar for all runs (not shown here), with differences that arise due to changes in the sea ice cover investigated further in Sect. 3.2.1."*. The seasonality (shown in Figure 6 for the Thick Landfast experiment) is qualitatively similar across all three experiments. This is what is being described here first.

Indeed, we mean to say that consistent across all three experiments, basal melting increases during summer as compared to winter and that this increase occurs under the deeper ($\geq$250 m) regions of the ice shelf. Please see below for suggested corrections to lines 7-12.

RC: ***Lines 8–9: "Diagnosing this variability ..." The purpose of this sentence is unclear. I suppose you want to say you disentangled the thermal driver from the velocity driver in your study? In this case, say something like: "Our diagnosis of the drivers of the change in melt rate ... suggests that the increase is primarily a result of a higher thermal driving, while the increase in the friction velocity are a secondary effect" etc. But it is still unclear to me which change you are talking about.***

AR: We intend to say that we are using the melt rate drivers to diagnose the seasonal variability in melt. This is, indeed, made possible by the fact that we are able to disentangle thermal driver from the friction velocity driver. Regarding the change: We are not yet summarising cross-experimental changes. These lines (8-9, and also 10-12) are intended to summarise the reason (using the drivers of melt) behind the seasonal changes in melt (line 7) that are seen in each individual experiment. Please see below for suggested corrections to lines 7-12.

RC: ***Lines 9–12: I assume you are still describing seasonal changes in the model as opposed to differences between the experiments, but this is unclear so please add the word summer somewhere. You should highlight more that you are first describing seasonality in lines 7–12, in contrast to the changes caused by the changing sea ice cover, as you will do later. You could start the description of seasonality on line 7 with something like: "In all three sea ice regimes, the basal melt in the deep regions of the ice shelf presents a seasonal increase during summer. This seasonal increase in melt rate is primarily driven by ..."***

AR: Indeed, we need to do a better job of clarifying this. Thank you for this series of comments! They are very helpful and have made us see the shortcomings of our description. We intend to revise these lines (7-12) based on your suggestion as follows:

> In all three sea ice regimes, basal melt in the deeper regions of the ice shelf presents a seasonal increase during summer. This seasonal increase in melt rate is primarily driven by a higher thermal driving, which, as a secondary effect, increases the friction velocity slightly downstream. The increased meltwater production and a stronger melt overturning in the PGIS cavity deliver more meltwater from depth to the shallower regions which lowers the thermal driving and basal melt in these regions; with the winter season showing a converse pattern.

RC: ***Lines 12–15: "Modulations in surface forcing under a mobile and thin sea ice cover act to enhance the heat transport in the cavity", this is very difficult to read, I suggest rather saying "A thin and mobile sea ice cover enhances heat transport into the ice shelf cavity, by increasing wind-driven upwelling of Atlantic Water from the Nares Strait into the cavity"***

AR: Thank you for the suggestion. Note that "...increasing wind-driven upwelling.." would imply that upwelling occurs under a landfast sea ice cover as well, and is increased as the sea ice becomes mobile and thin, which is not the case. Please see the revised text below:

> A thin and mobile sea ice cover enhances heat transport into the ice shelf cavity. Here, wind and convectively upwelled AW from the Nares Strait enter the cavity. Further, wind driven inflow intensifies, ...

RC: ***Line 15: Skip the "Mechanically", it is confusing.***

AR: Okay, we will skip this. Please see the revised text above.

RC: ***Lines 17–20: I suggest adding "the accompanying decline in the Arctic sea ice extent..." and "the basal melt of PGIS"***

AR: Okay, thanks for the suggestion. We have rephrased it as follows:

> ...and the accompanying decline in Arctic sea ice extent and thickness will amplify the basal melt of PGIS, impacting the long term stability of the Petermann Glacier ...

RC: ***Lines 26–28: This sentence is really packed, consider simplifying or breaking it into two. Also, the dynamic thinning and acceleration is a consequence of the loss of buttressing forces, so it should come after.***

AR: We agree, and we have suggested the following revision:

> Such additional oceanic heat causes enhanced basal melting and calving at marine terminating glacier fronts. The disintegration of ice shelves and associated loss of buttressing forces on the upstream inland ice masses can lead to glacier wide dynamic thinning and flow acceleration (Hill et al., 2018; Aschwanden et al., 2019; Rückamp et al., 2019).

RC: ***Line 31: Is "sectorial" the North or Northeastern sector?***

AR: Both.

RC: ***Lines 36–43: Review the use of tilde, and replace with words when possible. You could refer to your map figure. Use en-dash when presenting a numeric range.***

AR: Thanks, we will review the use of tilde throughout the manuscript and replace it with words instead. We have referred to the map figure (see below) and used en-dash for numeric ranges:

> l 42-43: ... 30–50 km wide water way separating northwest Greenland from Ellesmere Island, and bridging the Lincoln Sea and Baffin Bay (Figure 1(a)).

RC: ***Line 39: Unclear what is meant by "it has been studied through various lenses".***

AR: Thank you for informing us about the formulation. Please see the revised formulation below:

> l 37-39: PGIS has been extensively investigated following the two recent episodic large calving events in 2010 and 2012 (Falkner et al., 2011; Münchow et al., 2014).

RC: ***Lines 43–44: This sentence is overly complicated, you could just say "Heat and freshwater exchange between the ... through the Nares Strait, and the exchange is influenced by ..."***

AR: Thank you. We intend to revise this following your suggestion:

> Heat and freshwater exchange between the Arctic Ocean and the western subpolar North Atlantic Ocean is facilitated through the Nares Strait (Kwok, 2005), influenced by fjord and ocean circulation and sea ice dynamics.

RC: ***Lines 45–49: Indicate the approximate locations of the sea ice arches to Fig. 1. The arches are even in your paper title, and it is hard to grasp where they are located.***

AR: Thank you for this useful suggestion. Please see revised text below:

> l. 48-49: ...and associated variations in sea ice transport through the Nares Strait (Kwok, 2005; Kwok et al., 2010; Münchow, 2016; Ryan and Münchow, 2017; Moore et al., 2021) (Figure 1(a)).

RC: ***Line 50: What do you mean by "short"?***

AR: We will include this in the text. Please see below:

> l. 49-50: During summer, sea ice is mobile for ca. 2–3 months.

RC: ***Lines 50–52: Sounds like the study by Shroyer et al., was very similar to this one. Consider somehow hinting here what is the added value of your study. At least, highlight the differences in the discussion, is it only the bathymetry? Or are there differences in model forcings and physics?***

AR: Thank you for the comment. We have answered these questions in detail, together with the comment on Line 70-72 (please see below).

RC: ***Line 53: "outflow" is not well defined here, say rather something like "Sea ice area and the flow of ice volume through the Robeson Channel..."***

AR: Thank you. We agree, and we will revise this based on your suggestion:

> l. 53-54: Sea ice area and the flow of ice volume through the Robeson Channel were reported to be not only the highest ...

RC: ***Line 55: Omit "flux gate"***

AR: Please see the revised text above.

RC: ***Line 56: "ice volume flux was smaller"***

AR: Indeed. We will replace "less" with "smaller".

RC: ***Line 61: "formation duration" this is unclear***

AR: Thank you for pointing this out. We suggest a revision as follows:

> l. 60-61: A continued decline in the Arctic sea ice thickness (Maslanik et al., 2011; Kwok, 2018; Kacimi and Kwok, 2022) and duration of ice arch formation, ...

RC: ***Line 70–72: See earlier comment, are there any other improvements compared to earlier work that you might want to highlight? Improved model physics?***

AR: Shroyer et al. (2017) presented the response of PGIS basal melt to seasonal shifts in sea ice cover (landfast in winter to mobile in summer). Their paper was one of the papers that provided motivation for our work. We extend their work by investigating the response of PGIS basal melt to long-term shifts in sea ice cover. We have stitched up these long-term shifts together by combining information from the extensive literature on regional sea ice cover from early 2000s until 2022 (as described in l. 93-114 in the methods chapter, and further discussed in l. 433-437 and l. 468-475).

Regarding the added value of our study, we have summarized them in two parts:
1) Model/Experiment differences: Our high-resolution unstructured grid σ-coordinate model is able to better resolve the irregular coastal geometry and steeply-sloping seafloor topography, as well as smoothly representing the sloping PGIS base and preserving vertical resolution in the deeper parts of the PGIS cavity. Bathymetry under the ice shelf in Shroyer et al. (2017) derived from the BedMachine v3 dataset is not accurate, and without necessary modifications will render an artificial shallow (ca. 200–250 m) sill, restricting warm water inflow to the interior of the ice shelf cavity (Prakash et al., 2022). We implement an improved sub-ice shelf bathymetry which also features a laterally resolved inner sill (see Tinto et al., 2015 and Prakash et al., 2022). Additionally, our model features a realistic PGIS basal topography as opposed to an idealised (smooth) ice shelf of Shroyer et al. which does not resolve basal channels.
2) These changes resulted in noticeable improvements, few of which are highlighted here: We document the nuanced effect of variations in sea ice thickness on PGIS basal melt. Further, we provide a breakdown/disentanglement of the drivers of melt (thermal driving and friction velocity). We are able to provide high resolution maps documenting the topographic (improved bathymetry + realistic ice draft) control on the fjord circulation and (channelized) basal melt rate. Importantly, we are able to provide a hypothesis for a likely caveat in the setup of Shroyer et al., which has been mentioned in the studies of Cai et al. (2017) and Washam et al. (2020) as it contradicts, respectively, their modelled and observed findings, but to our knowledge, there exists no explanation for it (see l. 452-461).

We suggest revising lines 69-72 as follows, to highlight these differences:

> In this study, we use the Finite Volume Community Ocean Model (FVCOM) (Chen et al., 2007), amended by an ice shelf (Zhou and Hattermann, 2020) and a sea ice module (Prakash et al., 2022), to investigate the implications of long-term (i.e. several decades) changes in the Nares Strait sea ice regime on the PGIS basal melt. Here, variations in both the sea ice mobility (dynamics) and sea ice concentration and thickness (thermodynamics) are examined. The high-resolution unstructured grid σ-coordinate 3-D ocean-sea ice-ice shelf model is able to better resolve the irregular coastal geometry and steeply-sloping seafloor topography, as well as smoothly representing the sloping PGIS base and preserving vertical resolution in the deeper parts of the PGIS cavity. Other noticeable improvements over similar existing numerical setups include the implementation of a realistic ice shelf basal topography, an improved sub ice-shelf bathymetry, as well as the implementation of a laterally resolved inner sill ca. 25 km from the GL. We find that climate warming driven transition towards a mobile and thin sea ice cover from a landfast and thick one could result in up to twofold increase in melt. In such a scenario, wind and convectively upwelled warm AW enter the PGIS cavity. Further, in summer, under the deeper regions of PGIS, more efficient melting occurs in a more turbulent cavity, without any noticeable increase in thermal driving.

RC: ***Line 80–82: It is clear that the author wants to advertise the new package, but it is important that the study of this paper is comprehensible and thus the description of the model in this paper should be independent. Summarize here what are the included model physics (see Major Comment 3 on the suggested flow of the Methods-section), and refer to the previous paper for details (not for basics or results). Do not use the term "standard run" when referring to the previous paper, it is very confusing, particularly given that what is called "standard" is the Thin Mobile experiment, while what could be called the standard run in this paper is the Thick Landfast experiment.***

AR: We agree, and we have added a standalone summary of the sea ice module (please see Major comment 1). Regarding the flow of the Methods-section, we greatly appreciate this comment, and the series of minor comments (below), and intend to revise the flow by following up on all these suggestions. Regarding the "standard run" nomenclature, we agree and are thankful for this very valuable suggestion. We definitely wish to avoid confusion and improve readability, and will therefore remove all such occurrences. This occurrence of standard run in l. 81-82 will be rephrased as follows:

> For a detailed description of the model setup ~~and a standard run (~~ including selected results from it ~~)~~, see Prakash et al. (2022).

RC: ***Lines 88–89: Justify (just shortly here), why you use the pre-2010 draft of the ice shelf, and consider discussing the impact of this choice in the Discussion, or the impact of a change in the ice shelf geometry in more general terms.***

AR: Thank you for the comment. As indicated, we use the pre-2010 draft as this is what is provided in the BedMachine dataset. Regarding the impact of this choice/a change in the ice shelf geometry (e.g. pre-2010 vs present), we believe that it is difficult to speculate without conducting appropriate sensitivity tests, without which, we cannot provide any meaningful suggestions.

RC: ***Line 91: So, is one of the key novelties in this study that you have a second deep sill in the domain? If yes, then this should come across in the results more clearly, and you should discuss the impact of the sill to the circulation and heat transport in the cavity.***

AR: Thank you for the comment. Please see Minor Comment (Line 70-72) above regarding key novelties and noticeable improvements resulting from it. Given the clarification provided above, we believe that the improvements seen in the results (spread across the Results chapter) are now easier to distinguish. Regarding the inner sill, for the purpose of this study, it has been implemented to provide a "realistic" (i.e. as realistic as possible) representation of PF. In other words, it is seen as an indispensable component of the setup, and a scenario where it does not exist is regarded as unrealistic (as is the case based on ground reality). However, one of the goals in the future is to conduct systematic sensitivity tests w/o inner sill and w/o subglacial discharge with our setup. Those, however, require considerable amount of work, both in terms of preparing the input files and tuning, and thereafter, analysis. Without such tests, it is difficult to extend this (ice arches) study to draw such comparisons which falls beyond the scope of this work, and which would, in any case, be purely speculative.

RC: ***Line 91: Here at the latest you should explain how you include the ice shelf in the model. How do you parameterize the ice-ocean interaction? Explain and justify that you don't have subglacial discharge.***

AR: Please see Major Comment 1.

RC: ***Line 92: You should move the description of initialization and boundary conditions here, before you describe the different experiments***

AR: Thank you for this insightful suggestion. As stated above, we firmly agree with the ideas that have been put forward (here and elsewhere) regarding the restructuring. We will move the description of initialization and boundary conditions before going on to describe the experiment setup (i.e. l. 115-134 will come before l. 93-114 and Table 1).

RC: ***Line 94: Remove the subclause starting with which, you have already introduced the module.***

AR: Thank you for noticing this. Please see the revised sentence below:

> ...differing in sea ice concentration, thickness and mobility, ~~which are imposed through the Ice Nudge module (Prakash et al., 2022),~~ are conducted.

RC: ***Line 95: For general readability, I suggest calling the experiments simply "Landfast", "Mobile" and "Thin***

Mobile", and to use the full names throughout the paper and also in the figures. The abbreviations are too similar and impossible to follow.

AR: Thank you for this very helpful comment. We will rename the experiments as suggested, and remove all corresponding abbreviations to help improve readability.

RC: ***Lines 95–107: Introduce the experiments as a clear progression towards expected future conditions: Landfast represents the 90's conditions, Mobile when the sea ice arches fail to form and Thin Mobile the expected future, etc. Keep the same order you have in Table 1. throughout the paper.***

AR: Thank you for the comment. While we understand (and partly agree with) the reason behind the argument, opting for the chosen progression (only for this part of the manuscript) instead was a conscious decision. This is because the Landfast and Mobile runs are constructed by adjusting the A4-CICE sea ice conditions (Thin Mobile run). Thus, the Thin Mobile run needs to be presented first, following which the Mobile and Landfast runs can be described by simply explaining the tweaks that were made to the Thin Mobile sea ice conditions. Also, conversely, it is difficult to explain tweaks made to create the Landfast and Mobile experiments, before explaining what is being tweaked (i.e. the Thin Mobile run).

RC: ***Lines 97–99: This belongs to the paragraph where you present the initialization of the model, and again, do not use the name "Standard run".***

AR: Indeed, we will reposition this, and delete the usage of "Standard run" (as done above).

RC: ***Lines 102–103: This sentence is unclear. Use active language. It is not possible to follow the subclause ", which...", I suggest writing the point in a separate sentence.***

AR: Thank you for the comment. We have rephrased it as follows:

> In the Mobile experiment, we choose $A_i = 1$ to ensure that the region bounded by the sea ice arches (see Figure 2, Table 1) is completely ice covered. This affects the seasonality via removal of open ocean during summer.

RC: ***Line 105: "sea ice velocities are retained from the Thin Mobile run". This is very confusing, and the reader does not know the forcing or initialization procedure at this point. You should include a section describing the initialization and boundary conditions, before describing the experiments, as already commented earlier.***

AR: This is absolutely correct, thank you for noticing this. As stated above, we will move the initialisation and boundary conditions part before this part, and also create a separate subsection for it.

RC: ***Line 108–111: As I said before, you should carefully present and justify your initialization procedure, and demonstrate that the runs have stabilized after these perturbations. However, it should be made clear that the progression of the modelling procedure is separate from the evolution the experiments aim to present. Currently, these are muddled together. Since these progressions are opposite, you should make very clear that it is ok to initialize the runs in this way.***

AR: Thank you for the comment. Following your suggestions, we intend to revise this as follows:
- Move the initialisation and boundary conditions text block before presenting the experiment setup, and also give it its own subsection.
- In this new subsection, present and justify the initialisation procedure (i.e. make it clear that it is okay to initialize runs this way). Support this by demonstrating stability (see Major Comment 1).
- Make it clear that the progression of the modelling procedure (Thin Mobile, which is used to create Mobile

and Landfast experiments) is separate from the evolution the experiments aim to present (Landfast to Mobile to Thin Mobile) (see Major Comment 1).

RC: ***Lines 115–134: Now you finally describe the initialization and boundary conditions, this is critical information to know before describing the experiments, move up and make its own subsection.***

AR: Thank you for the comment. Please see above.

RC: ***Line 115: Start by saying that you run the model from 2014 to 2017, and only then say when the model results are presented.***

AR: Thank you for the comment. We will move l. 118-119 (*"The simulation period spans from July 01, 2014 00:00:00 UTC - January 01, 2017 00:00:00 UTC"*) up.

RC: ***Line 117: I do not understand the subclause starting with "for which ..."***

AR: Thank you for informing us. It means that we have the required data to be able to simulate (out of the years 2016-2019) the year 2016. We have revised it as follows:

> Note: Red color indicates deleted punctuation.
>
> l. 115: ... UTC, as it is closest to the (2016-2019) period ~~,~~ which has been characterised by frequent collapse/absence of ice arches, reduced ice flux stoppages and extraordinarily high ice area flux through the Nares Strait (Moore et al., 2021), and for which the model can be be initialised and run using realistic atmosphere, ocean and sea ice boundary conditions.

RC: ***Lines 119–122: I'm not sure I get this right, so you get atmospheric temperature from RACMO and maybe winds? And which sea ice conditions from the A4 ROMS? State these clearly! And then you use the IceNudge module to transfer these to boundary conditions for the ocean? Be clearer, what variables do you get from which model? What are the time steps? Do you have some downscaling procedure?***

AR: Yes, you are right, we get air temperature, winds and a few other variables from RACMO (please see revised text below). We have also included the sea ice variables that we get from A4-CICE in the revised text below. Yes, the IceNudge module is used to transfer these to boundary conditions for the ocean (see Major Comment 1). Time steps are also included in the revised text. The downscaling process is cumbersome to detail here, but we have now mentioned it here and referred to the previous paper for technical details.

> l. 122: ...run (Hattermann et al., 2016; Hunke et al., 2010) respectively. We obtain the 3-hourly 2-m air temperature, surface air pressure, relative humidity, downward longwave radiation, net shortwave radiation, eastward and northward wind speed, precipitation, and evaporation from RACMO2.3p2. These are used to compute and apply the net heat flux, momentum flux and net freshwater flux at the sea ice free (i.e. open ocean) fraction of a given cell area. Further, the daily avergaed sea ice concentration and thickness, bulk ice salinity, and sea ice velocities are obtained from A4-CICE. The mechanical and thermodynamical fluxes at the sea ice-ocean interface are provided by the Ice Nudge module (Prakash et al., 2022). ~~Therein, the state of sea ice, and the mechanical and thermal fluxes to the ocean at the sea ice-ocean interface are imposed as a surface boundary condition to the model using the Ice Nudge module (cf. Prakash et al. (2022)).~~

RC: ***Lines 124–125: I don't follow, se is the model set up to create the arches? Or you have chosen a period when this happens in the model?***

AR: No, the model is not set up to create ice arches. We have chosen a period when this happens in the model. We will clarify this in the revised text as follows:

> In this period, the A4 ROMS-CICE run forms the northern sea ice arch, with thick (multiyear) fast ice conditions north of it, and a thin, year round mobile ice to its south..

RC: ***Lines 125–126: I'm even more confused, so a thin and mobile sea ice cover agrees well with what and when?***

AR: We intend to say that the formation of such a sea ice cover (as described in the previous line) in A4-CICE during the simulation period is in agreement with observed findings from the 2016-2019 period, and thus validates the model. We will clarify this as follows:

> The formation of a younger, thinner and mobile sea ice cover in the Nares Strait in A4-CICE during our simulation period is in agreement with observational findings reported by Moore et al. (2021) and Kacimi and Kwok (2022) for the 2016-2019 period.

RC: ***Lines 126–127: Remove the reference to the standard run, I think it is at the root of the confusion.***

AR: Thank you, we will remove it.

RC: ***Lines 129–131: Which fields? Temperature, salinity, velocity?***

AR: Thank you for the comment. We have clarified this along with the comment below.

RC: ***Lines 130–132: I'm confused, are these the hourly conditions you just mentioned? Why this period for the average?***

AR: Temperature, salinity and velocity come from A4-ROMS. The sea surface height comes from A4 + AOTIM. Since we noticed a (cold) bias in the A4 temperature and salinity time series (2007-2017), we could not use data from the 2014-2016 period. Instead, to alleviate the bias, we chose the years from the start of the A4 simulation (2007-09) when the bias was negligible to construct monthly climatologies that were then interpolated to hourly forcing intervals. This resulted in noticeable improvements, however, the AW in the fjord remained colder and fresher compared to observations. We have now rectified this further, such that, modelled temperature and salinity from the fjord are consistent with observational data from this period (Johnson et al., 2011). We suggest revising l. 128-134 as follows:

> As described in Prakash et al. (2022), the FVCOM grid is nested within the A4 ROMS grid. To alleviate the (cold) bias in the upstream A4 temperature and salinity time series (2007-2017) (see Hattermann et al. (2016) and Prakash et al. (2022)), monthly climatologies of temperature, salinity, and velocity were constructed from the start of the A4 simulation (2007-09) when the bias was negligible, which were then interpolated to hourly forcing intervals. While this procedure resulted in noticeable improvements, the AW in the fjord remained colder and fresher compared to observations (cf. Johnson et al., 2011; Jakobsson et al., 2020; Prakash et al., 2022). To that end, we have implemented a depth-dependent bias correction (as suggested in Prakash et al. (2022)), such that, the modelled temperature and salinity are now consistent with observational data (Johnson et al., 2011) from this period. Lastly, the hourly AOTIM (Arctic Ocean Tidal Inverse Model; Padman and Erofeeva, 2004) sea surface height (SSH) solution from the simulation period is added to the A4 SSH field from the same period to generate the tidal forcing (see Prakash et al., 2022 for details).

RC: ***Lines 115–134: Ignoring the previous paper, is the general picture that the model was calibrated to reproduce a thin and mobile year since there was quality forcing data and observations for these types of conditions? And then you create the experiments with more sea ice by changing sea ice parameters indicated in Table 1? If yes, please make sure that this idea is clearly communicated. I still don't know at which point of the Landfast and Thick Mobile experiments the sea ice conditions were changed, if the model stabilized in reference to this change and if any other parameters or forcings were changed. Be sure to communicate these clearly. And show in the supplement that the model stabilizes.***

AR: Please note that we do not (re-)produce sea ice conditions, as would be the case in a coupled sea ice-ocean model. We are, indeed, bounded by data availability. In the 2016-2019 period (described in the manuscript, and in the comments above) which is characterized by a thin mobile sea ice cover, 2016 is the closest we could come in terms of data availability (note that to be able to initialise and stabilise the model, we also need data from at least a year prior for safe measure). By data, we mean ocean + atmosphere data alongside sea ice data. Once we have this setup working, we indeed, create experiments by changing sea ice parameters indicated in Table 1. Please see Minor Comment (Line 117) and Major Comment 1 for answers to the remaining questions.

RC: ***Line 137: How is the melt rate computed?***

AR: Please see Major Comment 1.

RC: ***Lines 138–142: I don't see how this information is necessary***

AR: Thank you for the comment, we have resolved this together with the comment below.

RC: ***Lines 143–182: This is a very detailed description of how to process model output, which is unnecessary information for most of the readers interested in the results. I suggest moving this diagnostics part into a supplementary. You don't really need to have the subsection of "Model output and diagnostics".***

AR: We agree, and we will move this to the supplementary section.

RC: ***Lines 184–186: In reference to Major Comment 2: I suggest reformulating this to more generally examining the circulation and heat transport from Nares Strait to the fjord, and further into the PGIS cavity. And the yes, you will show the melt rate as well, but put more focus on the processes in between.***

AR: Please see Major Comment 2.

RC: ***Line 190: This is for the whole domain, right? Fig. 3 is also partly of Nares Strait. I recommend showing the seasonal temperature and salinity vs depth of the inflow to the fjord.***

AR: We are looking at the Petermann Fjord, and partly the Nares Strait. The whole domain is not shown here. Please see below for revisions to figures.

RC: ***Line 191: Remind the reader here how you define the seasons. Have you defined a "wider PF area" somewhere?***

AR: Thank you for the comment. Please note that a reminder for the definition of seasons is already mentioned in l. 191. Regarding the "wider PF area", please see corrections below:

> ...in the wider PF area, which includes the Petermann Fjord, and the Hall Basin and Robeson Channel in the Nares Strait, is shown in Figure 3(a,b).

RC: ***Line 195: "lateral inflow and outflow" of which directions? Write it in the sentence, don't use the ",***

respectively".

AR: Thank you for the comment. We prefer the formulation that is in place due to its conciseness. However, we are willing to amend it based on some suggestions, as we are not sure how to formulate it otherwise.

RC: ***Lines 196–198: I find the references here confusing, just refer to the discussion.***

AR: Okay, we will omit the references, and just refer to the discussions.

RC: ***Line 200: You should also show the flow along the fjord.***

AR: Please see Sect. 2 below regarding revisions to Figure 5.

RC: ***Lines 205–206: Write separate proper sentences to each of the seasons, this formulation is very difficult to read.***

AR: Thank you for sharing this useful information with us. Please see revised formulation below:

> In winter (Figure 4(a)), inflow into, and outflow out of the PGIS cavity of up to ca. 0.15 m/s is modelled.

RC: ***Lines 206–208: When?***

AR: In winter; please see l. 205. We revise this as follows for clarity:

> ... is modelled. Further, at the GL depth of ...

RC: ***Lines 208–212: It's nice to see the across-fjord flow, but since we have not seen how temperature and salinity change between the seasons in Nares Strait, it is difficult to put the change here in the fjord to context. Also, along-fjord isopycnals are essential to see what role density-driven inflow over the sills play.***

AR: Thank you for the comment. We have revised the figures following the suggestions that were provided. Please see Sect. 2 below.

RC: ***Lines 216–218: Where do I see this? Again, along-fjord profile with isopycnals is missing.***

AR: In Figure 4 (c,d). Thank you for indicating this to us. Please see below for the revised version:

> ... Sect. 4.1.2). Further, in winter, water .... increase in the near-bottom temperatures (Figure 4 (c,d)).

RC: ***Lines 219–22: This is very dense and difficult to follow. Why do we need to know the annual mean? I think summer and winter inflow and outflow values would be enough. Percentages of increase relative to annual mean have little value, summer increase relative to winter would be more informative. Write proper sentences, and do not use parentheses.***

AR: Thank you for the comment, and also for providing useful suggestions to improve this. Please see the rephrased version below:

> A winter mean glacierward heat inflow (H_{IN}) of 2.6 TW, and a summer mean H_{IN} of 3.3 TW is computed normal to the PGIS cross-section (Figure 1(b)). The corresponding mean winter outflow (H_{OUT}) is -2.39 TW, whereas the summer H_{OUT} is -2.81 TW. Thus, a net mean heat inflow (H_{NET}) of

0.21 TW in winter and 0.49 TW in summer is directed into the PGIS cavity (see Sect. 4.1.3; further discussed in the context of changing sea ice cover in 3.2.3 and 4.2.3).

RC: ***Line 225–228: Rephrase. The modelled annual mean of what? Moreover, why do we need to look at the annual mean, I don't see its value.***

AR: Thank you for the comment. We will rephrase this introductory paragraph to describe the revised figures presented in Sect. 2.

RC: ***Line 225: See suggestions to Figure 5***

AR: We have revised the figure based on the suggestions that were provided. Please see Sect. 2 below.

RC: ***Lines 230–235: You should start by describing the stratification within the fjord. You have a strong pycnocline separating the Polar Water layer from the Atlantic Water. Due to the difference in density, the seasonal response of these layers is different. Throughout the results you should describe the changes relative to the layers, not the depth. This way you will avoid clumsy formulations like "beneath the shallower ice base". Along-fjord study of the density is key to understand what drives the circulation. Seasonal along-fjord flow patterns would also be of key importance, right now they are not shown. See suggestions to Figure 5.***

AR: Thank you for bringing this to our attention, we will describe the stratification first. We have revised the figure following the suggestions (see Sect. 2 below). While we understand the thought behind the layer based formulation of sentences, an ice shelf draft based formulation is used in order to be consistent with previous studies (e.g. Shroyer et al., 2017) and facilitate inter-model comparisons. In Shroyer et al. (2017), results are segmented in two parts based on the ice shelf draft criteria (greater and less than 200 m). This segmentation provides an ice shelf based perspective which remains fixed across seasons and experiments, and also two different models. Moreover, our results also show contrasting characteristics in these two domains, and using this representation, we are also able to highlight the likely caveat in Shroyer et al. (2017) (see Minor Comment on Line 70-72).

RC: ***Line 238: First go through the circulation in the fjord properly, and describe the melt rate in a new paragraph.***

AR: Thank you for noticing this. We will make a clean cut between the fjord circulation and melt rate (which will be moved to a new paragraph).

RC: ***Lines 239–244: Write separate sentences for winter and summer, and give the values for mean melt rates for winter and summer, not the anomalies.***

AR: Thank you for the comment. As stated above, we will revise all such complicated parenthesis based formulation here and also elsewhere throughout the manuscript, and replace them with separate sentences. The winter and summer mean melt rates have been provided (l. 243), however, we will rephrase the sentence appropriately as per the suggestion.

RC: ***Lines 245–249: What is the difference of the "regions of strong melt aligned with the PGIS flow direction" and the "substantial lateral variability". Aren't they the same thing? What is meant by "and by up to 50 m/yr"?***

AR: Thank you for the comment. "regions of strong melt aligned with the PGIS flow direction" is referring to the increased melt seen in the south-north direction (owing to the length of the basal channels that run almost along the entire length of the ice shelf), whereas "substantial lateral variability" is referring to the control exerted by the width of these channels. Both combine to produce the result seen in these 2-D melt maps. We will make this clear, please see the suggested addition below.

"and by up to 50 m/yr" is indicating the upper (or lower, in case of winter) limit of the melt anomalies (which are largely around 10-30 m/yr) seen on the eastern sector.

> l. 249: The mean modelled magnitude and spatial characteristics of the PGIS basal melt, that are largely controlled by the length and width of the basal channels, compare well with...

RC: ***Lines 251–253: Why the "presumably" and "likely"? Shouldn't you know from the model if it due to AW inflow? Note that you have not defined friction velocity.***

AR: Indeed, thank you for noticing this, we will remove them. We have defined the friction velocity before (please see l. 228).

RC: ***Lines 254–256: Rephrase this in terms of buoyancy and the pycnocline.***

AR: Please see Minor Comment above (l. 230-235). We are describing processes occurring at the ice shelf-ocean interface/directly underneath the ice shelf draft (already included in the definition of thermal driving and friction velocity). There is no ambiguity regarding the "location" that is being discussed. Moreover, for such 2-D ice shelf plots without pycnoclines, we believe that an ice shelf draft based formulation is more intuitive to follow.

RC: ***Line 257: If you would have an along-fjord plot of flow streamlines, then you would know and would not need to guess.***

AR: Thank you for this very useful comment. We have revised the figures based on the suggestions (please see Sect. 2 below). Since we are not showing annual mean and anomalies relative to it, we will remove this sentence.

RC: ***Lines 257–259: This sentence belongs to Section 3.2***

AR: Thank you for the comment. It is important to state that while we only show the patterns for the Landfast run (Figure 6), similar patterns are seen across the other two experiments as well. This needs to be said while Figure 6 is being discussed, and also before we depart towards discussing changes driven by sea ice cover.

RC: ***Line 260: Rename this section, and do not include the name of the package. "Response to decreasing sea ice" etc.***

AR: Thank you for the suggestion. We agree, that the section should not include an unfamiliar package name. We suggest the following:

> "Response of PGIS basal melt to climate warming driven changes in sea ice cover"

RC: ***Line 261: Don't go to the basal melt first. Keep the structure as in 3.1, and start from far field and work your way in towards the melt rate. This way it is possible to track how the impact of changing sea ice conditions propagate into the fjord.***

AR: Thank you for the suggestion. We agree, and we will change the order as suggested to keep the structure consistent with 3.1.

RC: ***Lines 275–277: Rewrite without the parentheses, remove "calculations show". Instead of "deeper regions", say "below the pycnocline" or "within the Atlantic Water".***

AR: Thank you for the comment. As stated above, all parentheses based sentences here and elsewhere will be revised. We will remove "calculations show". Note that we are referring to basal melt rates that correspond to ice shelf draft (deep/shallow) and not the ocean (l. 272), and we will make this clear. Please see the revised formulation below:

> As the sea ice regime, in addition to being mobile, becomes thin, the winter mean melt for the deeper regions of the ice shelf increases by an additional 0.13 m/yr, while in summer, the increase is 1.8 m/yr.

RC: ***Line 281: "primarily driven by the strengthening of the fjord circulation". This is very interesting, and yet we know nothing of the fjord circulation and along-fjord transport***

AR: Thank you for the comment. Please see the revisions regarding along-fjord transport in Sect. 2.

RC: ***Lines 283–285: This sentence is difficult to follow, rewrite without the parentheses.***

AR: Thank you for the comment. As stated above, all parentheses based sentences here and elsewhere will be revised. Please see the revision below:

> Further, the Thin Mobile run shows a decreased melting of shallower ice in winter due to the heat that is lost to the atmosphere. Contrarily, in summer, an increased melting of shallower ice is seen due to the additional heat that is gained from strong shortwave heating over open ocean (Figure 8(b,c,e,f), see Sect. 3.2.2 and 4.2.1).

RC: ***Lines 291–293: "In winter, the relative increase ..." This sentence is hard to follow.***

AR: Thank you for notifying us about this. Please see a revised formulation below:

> The relative increase in salinity is lower in winter compared to summer (Figure 9(c,g)). However, a higher increase in temperature is seen under the deeper regions of the PGIS base in winter as compared to summer (Figure 9(a,e)).

RC: ***Lines 293–295: Again, this sentence is difficult to follow. State first the general trend before giving numbers: "As the sea ice becomes mobile, the deep water masses seaward of the sill get warmer and saltier both in winter and summer." etc.***

AR: Thank you for the comment and the suggestion. Please note that the general trend, as suggested, has been described in l. 290-292.

RC: ***Lines 295–298: "As the sea ice thins in addition to becoming mobile, ..."***

AR: Thank you for the suggestion. Please see the revised formulation below:

> ~~With a regime characterized by Thin Mobile sea ice instead of a Thick Mobile one,~~ As the sea ice thins in addition to becoming mobile, nominal heat and salt ...

RC: ***Lines 298–299: "The colder water masses are advected into the PGIS cavity". This is too vague. At this point, you have not discussed the transport processes into the fjord, and it really should be done. It is not clear what is the process that delivers the change into the cavity.***

AR: Thank you for the comment. Please see the revisions made to the figure based on the suggestions in Sect. 2.

RC: ***Lines 305–310: Finally, we get to this, but this needs a better figure to clarify the process, see comments to Fig. 10.***

AR: Thank you for the comment. We have revised this figure based on the suggestions in Sect. 2.

RC: ***Line 316: Have you defined the upper ocean layer?***

AR: Thank you for noticing this. Please see the revised formulation below:

> ..salinification of the upper ocean layer (i.e. first σ-layer)...

RC: ***Lines 322–325: This is probably the most important result, and yet you say "it is likely". Shouldn't you see what happens in the model? You will once you study the isopycnals relative to sill depth.***

AR: Thank you for pointing this out. Based on the revised figure (see Sect. 2), we will remove the "likely".

RC: ***Lines 325–327: These are important results, write more explicitly. What is "this mechanism". In the following sentence you switch to discussing cold water. Which cold water and how does that follow from the previous sentence?***

AR: Thank you for the comment, and for the suggestions. Please see our revised formulation below:

> Also, heat loss and salt gains of comparable (to the Nares Strait section) magnitude driven by the convective overturning mechanism are seen seaward of the calving front (Figure 9(b,d)). Confined largely to the shallower outer regions of the PGIS base (Figure 9(b)), the colder waters formed as a result of this heat loss enter the cavity and act to lower the ΔT and basal melt (Figure 8(b,e)).

RC: ***Lines 329–332: Are you sure about this? The cavity is well sheltered from the impact of winds, and I presume what you see in the cavity is density-driven inflow.***

AR: Yes, we are sure about it. This is because the energy transferred over the open ocean region of the fjord and the adjacent Nares Strait region is unlikely to instantly dissipate. Without any barrier, the flow is largely unopposed as it enters the cavity. We see this in Figure 8, where for the deeper drafts, the difference in ΔT between the sea ice experiments is negligible. In this respect, the melt, and melt driven overturning is thus relatively similar across these experiments. The difference in basal melt, therefore, likely comes from a wind intensified buoyancy driven fjord-scale circulation (i.e. wind acting in concert with the overturning) which increases melt by increasing u^* at the ice shelf base (cf. also, outflow in the new Figure 9). However, we do concur regarding the role of density-driven flow, and we acknowledge it in the abstract (l. 15-17) and the summary (l. 529-532), but seem to have missed including it here. Please see the revised formulation below:

> l. 330-331: The resulting intensified flows work in tandem with the melt driven overturning to increase the u^* that regulates...

RC: ***Lines 334–335: Where do you discuss the heat budget?***

AR: Thank you for noticing this, we have revised this sentence (please see below). The heat budget is discussed in l. 481-482. Much of the heat entering the cavity exits without triggering any melt. This is consistent with observation based findings of Johnson et al. (2011) and Hueze et al. 2017. Thus, heat supply remains in excess of what is needed to generate contemporary estimates of basal melt. They suggest that mechanisms other than oceanic heat supply also need to be considered when assessing PGIS basal melt. We believe that the u^* driven increase in melt seen during summer in the Thin Mobile experiment, which is generated due to the enhanced vertical current shear (and without the need for any increase in thermal driving), is one such mechanism (Figure 8 (c,i)). As noted in the previous minor comment (i.e. abstract/summary), this increase in u^* results from wind intensified currents acting in concert with the stronger melt driven overturning in the Thin Mobile run. We have made revisions to the text as follows:

> l. 334-335: Further, the annual, winter and summer mean heat budget of the PGIS cavity for all three experiments is shown in Table 2, and discussed further in Sect. 4.2.3.
>
> l. 481-482: Similar to the Thick Landfast run, summer H_{IN} is substantially higher than winter H_{IN} for both the Thick Mobile and Thin Mobile runs, with much of the H_{IN} leaving the cavity (H_{OUT}) without triggering any basal melt (Table 2). Hydrographic data collected from the fjord also show that heat supply to the fjord has remained in excess of what is needed to generate contemporary estimates of basal melt (Johnson et al., 2011; Hueze et al., 2017), suggesting that hitherto undetermined mechanisms, besides oceanic heat supply, must also play a part in controlling PGIS basal melt. We posit that during summer, as the sea ice becomes mobile and negligibly thin, wind intensified currents act in concert with the stronger melt driven overturning to generate substantial shear driven turbulence, which is sufficient to drive considerable increase in melt without the need for any noticeable increase in thermal driving (Figure 8 (c,i)).

RC: ***Line 336: "As the sea ice becomes mobile, the inflow during winter along the western fjord sector strengthens and intensifies with depth, being up to ..."***

AR: Thank you, this is indeed a better formulation. Please see the revised text below:

> As the sea ice becomes mobile, the inflow during winter along the western fjord sector strengthens and intensifies with depth, being up to 0.03 m/s higher at depths of ca. 700–800 m (Figure 11(a)).

RC: ***Line 338: What is an "increment in the inflow"?***

AR: Thank you for the comment, we have revised it as follows:

> There is a further strengthening of the inflow, being up to 0.01 m/s higher, when transitioning ...

RC: ***Lines 345–347: This looks like a numerical error, are you sure that this is real?***

AR: Thank you for the comment. We interpret this as a weakening of the inflow (as opposed to outflow), since we are subtracting only the inflow component between the experiments. This could likely be associated to the gap (open ocean part) in our cross-section.

RC: ***Lines 349–350: Do not use the exact same titles as you had in results. I recommend rethinking the structure of the Discussion, perhaps organize thematically or in the order of your most significant findings.***

AR: Thank you, this is a very good suggestion. We would prefer organising it thematically. We will categorize it under the following titles (please see below):

> 4.1 Topographic control on the mean circulation in the fjord
> 4.2 Drivers of fjord-scale circulation under a landfast sea ice cover

> 4.3 PGIS basal melt, uncertainty, and future outlook

RC: ***Lines 351–353: modelled mean flow where? Is consistent in terms of what?***

AR: Thank you for the comment. Please note that these are already included in l. 351-353. We have rephrased this for clarity:

> The modelled mean flow in summer from the Hall Basin and north of the PGIS calving front is consistent with remotely sensed summer snapshots of the surface circulation from this region, which provided evidence of a general southward current in the Nares Strait, with a cross-strait component in the Hall Basin (Johnson et al., 2011).

RC: ***Line 353: What do you mean by "was seen"?***

AR: Thanks you for informing us, we have revised it for clarity, please see below:

> ...a similar (cyclonic) gyre was noted in the remotely sensed snapshot near...

RC: ***Line 354–356: I cannot follow this sentence, please rephrase.***

AR: Thank you for informing us. Please see the revised version below:

> Near the fjord mouth, the lateral inflow along the western fjord wall is directed equatorward, whereas the outflow along the eastern fjord wall is directed poleward (Figure 3). This resembles a Kelvin wave propagation, and is in agreement with the modelled findings reported by Shroyer et al. (2017).

RC: ***Lines 353–359: I don't get it, why the "likely"? "it is thus speculated", who speculates? Why do you need to speculate if you have the results from all experiments, as you say in the next sentence?***

AR: Thank you for the comment. We agree, we should take more ownership and avoid using such guess-words here. Please see the revised formulation below:

> l. 358-361: Our results evidence that bathymetry exerts a major control on the mean circulation prevailing in the PF (Figure 3, A1), as the modelled circulation patterns are relatively similar across the three experiments and persist irrespective of season.

RC: ***Line 361: I don't think it is due to air-sea momentum transfer, the density gradient should be driving this, but since you results lack the analysis of density, we cannot see it from the figures. See Carroll et.al., 2017, Kajanto et.al., 2023 and Hager et.al., 2022 for similar work in other glacial fjords.***

AR: Thank you for the comment, and also for providing us with useful literature. Please see Minor Comment on l. 329-332, and also the revised figures in Sect. 2. Indeed, density as a driver is important (see revised discussions in Minor Comment on l. 369-372, linking our results to the wider literature on other high-silled glacial fjords of Greenland), and it acts in tandem with wind driven flow intensification in the absence of a landfast and thick sea ice cover. Please see the revised formulation below:

> Yet, they vary in magnitude, in response to modulations in the buoyancy forcing and air-sea momentum transfer induced by a mobile and thin sea ice cover (further discussed in Sect. 4.2.3).

RC: ***Line 362: Who suggests? Are you saying that you find that there is water mass exchange and renewal in the fjord throughout the year? This a very interesting result, but you should state what processes are driving the water mass renewal, and discuss the role of missing subglacial discharge.***

AR: Thank you for the comment. We will use active language. Yes, we are saying this because the mean circulation is influenced by topography (year-round), and is additionally modulated by buoyancy and momentum transfer (and also, the missing subglacial discharge). As these modulators are amplified/more active in summer compared to winter, the renewal times are likely lower in summer compared to winter (here, we have also added relevant references to link it to the wider literature). We have revised this sentence as follows:

> Thus, given the influence of topography on the mean circulation in the fjord, we suggest that the exchange of water masses between the Nares Strait and the PF is maintained all year round which facilitates the renewal of water masses in the fjord. Further, the warm and saline AW that enter the PF at depth are effectively circulated in the PGIS cavity and transported to the GL. However, we note that since the circulation is additionally modulated by factors such as buoyancy, momentum transfer and subglacial discharge (not included in this study) which are amplified/more active in summer compared to winter, the renewal times are likely to be lower in summer as compared to winter (see Carroll et al., 2017 and Hager et al., 2022).

RC: ***Line 364: What do you mean by "can be"?***

AR: Thank you for the comment, we have reformulated it. Please see the revised sentence in the comment above.

RC: ***Line 368: Why the "may be"?***

AR: Thank you for the comment, we have reformulated it. Please see below:

> ..(cf. Figure 4) is caused...

RC: ***Lines 369–372: Rephrase, and no need to repeat all the result values. I think you are trying to say that the inflowing water over the sill is warmer and thus causes more melt, which in turn creates a stronger buoyancy forcing that drives a stronger circulation in the cavity.***

AR: Thank you for the comment. We have revised the text based on the suggestions, and added relevant references to link it to the wider literature on high-silled glacial fjords of Greenland, as follows:

> l. 367-372: Under a year-round Landfast sea ice regime in the Nares Strait, strengthening of the inflow of the warmer and denser AW into the PGIS cavity during summer as compared to winter (cf. Figure 4) is caused by a stronger melt driven overturning circulation (Figure 6). ~~The AW that overflow ... (Figure 4 (a,b)).~~ Previous studies on other high-silled glacial fjords of Greenland have highlighted the role of buoyancy in forcing fjord-scale circulation and driving seasonality (see Straneo and Cenedese, 2015, Hager et al., 2022, Kajanto et al., 2023). The stronger summer inflow events, in turn, deliver larger volumes of warmer AW deep into the PGIS cavity (cf. Table 2).

RC: ***Line 373: "Irrespective of the causes". How can you say this? Understanding the causes is the key!***

AR: Thank you for the comment. Please note that in light of the correction implemented to fix the cold bias and the Γ_T value following comments from Reviewever 2, this paragraph will be removed as it is no longer applicable to our setup. The missing subglacial discharge and its implications will be addressed below (see also, Major Comment 2). We apologise for the inconvenience.

RC: ***Line 375: Quantify the cold bias. Is the bias systematic around the year or does it have seasonality? Is the bias connected to the missing subglacial discharge?***

AR: Thank you, and please see Minor Comment on l. 373.

RC: ***Line 377: Remember to explain this modification of turbulent transfer already in Methods. Remember to include a table of all model parameters into the supplement. Write here the values and compare them to the values suggested in Jackson et.al. 2020. Discuss the implications of having an increased melt over the entire ice shelf as opposed to having the subglacial discharge plume.***

AR: Thank you, and please see Minor Comment on l. 373.

RC: ***379–380: Who assumes? You can't say this without the comparisons and discussion of the previous comments.***

AR: Thank you, and please see Minor Comment on l. 373.

RC: ***Lines 381–382: And subglacial discharge? Reference?***

AR: Thank you for bringing this to our attention. We have included the references at the first mention, and also acknowledged the missing subglacial discharge in our setup when comparing to summer observations (Johnson et al., 2011; and Heuze et al., 2017). Please see the revised version below (Minor Comment on l. 386-388).

RC: ***Lines 385–386: I don't follow. Is this your model result you are describing?***

AR: Thank you for informing us about this. The intention was to give an explanation for the modelled subsidiary outflow on the western flank seen near the fjord mouth during summer (Figure 4), as was also observed by Johnson et al. (2011) and Heuze et al. (2017). l. 381-384 provide information from observational studies, and l. 385-388 uses information from l. 381-384 to put our model results (l. 385: *"...that the modelled outflow along the eastern fjord sector (Figure 4)..."*) into perspective. Please see the revised version below.

RC: ***Lines 386–388: Are you describing your model results? Or observations? Why the "likely"? Then you apparently switch to discuss observational analysis of melt channels mid-sentence? Subglacial discharge plumes often erode channels under ice shelves, but how does that connect to your model since you don't have plumes? These are interesting points but you should elaborate and carefully consider what your model can say. The connection with the ice shelf shape is interesting and merits more thought.***

AR: Thank you for the comment. These are the model results. The "likely" is used because we have not run simulations to investigate meltwater pathways, and thus, cannot draw direct comparisons with the study of Hueze et al. (2017). Thus, even if the modelled outflow carries PGIS meltwater with it, since we don't analyze it ourselves, we use "likely".

In l. 387, we are not discussing observational analysis of melt channels. Please note that the BedMachine v3 ice shelf basal topography implemented in our setup is observational, and includes the subglacial discharge eroded basal channels. The modelled basal melt rates are characterized by the steep slopes which support stronger entrainment in the buoyant ice shelf meltwater plumes, and are thus, highly channelized (as shown in the results, and discussed in detail in 4.1.3).

Hueze et al. (2017) attributed the subsidiary outflow (see Minor Comment on l. 385-386) to the enhanced channelized melt along the western and central channels that run almost along the entire length of the ice shelf base, and locally thin it to ca. 50 – 70 m near the calving front. The enhanced channelized melt (western and central sector) during summer is also seen in our model (courtesy of the realistic basal topography that has been implemented). The inclusion of subglacial discharge in our setup, expected to be completed soon, would likely intensify the channelized basal melt that is seen in this study. We will revise the text to remind the reader of our ice shelf geometry, please see below.

> l. 381-383: Summer snapshots acquired during hydrographic surveys in the PF region provided evidence of a thick layer of PGIS meltwater, located in the 50 – 300 m depth range (Johnson et al., 2011; Heuzé et al., 2017). This meltwater layer was reported to be bounded by cold and fresh polar surface waters above, and warm and dense AW below, as well as to leave the PF along its eastern part.
>
> l. 386-388: ... out of the fjord. We note that though our setup does not feature subglacial discharge, the realistic ice shelf basal topography implemented in our setup does feature the subglacial discharge eroded basal channels which control the spatial distribution of melt (Figure 6(a-c), further discussed in Sect. 4.1.3). Thus, the subsidiary summer outflow in the 50 – 250 m depth range on the western flank (Figure 4(d)) is also likely to export PGIS meltwater resulting from enhanced melt along the channels that run almost along the entire length of the PGIS base, locally thinning it to 50 – 70 m in the central and western sectors near the calving front (Rignot and Steffen, 2008).

RC: ***Line 392: Were these both observational studies?***

AR: Thank you for noticing this. Please see the revised version below:

> the first order observational net heat flux estimates ...

RC: ***Line 395–397: Which one are you talking about now?***

AR: Thank you for noticing this. Please see the corrections below:

> l. 394-397: ... periods corresponding to the ~~reported~~ observational estimates (Kwok et al., 2010, Moore and McNeil, 2018). However, the ~~reported~~ observational estimates were deduced from summer snapshots of geostrophic velocities that did not span the entire width of the fjord, which could be a reason for the underestimation of ~~reported~~ observational H_{NET} when compared to the modelled one.

RC: ***Line 397–398: This is a very harsh sentence after the comparison you just did.***

AR: Thank you for letting us know, we will remove this sentence.

RC: ***Line 401: It is unclear what the reference to Holland et.al. Is for here.***

AR: Thank you for noticing this. It appears to be a leftover typo. We will remove this as well.

RC: ***Lines 401–404: This is the general shape you get with the Jenkins' 99 three-equation parameterization that I supposed this model also uses (which is information that should appear in Methods).***

AR: Thank you for the comment. Indeed, this is used by the model, and is now addressed in Major Comment 1.

RC: ***Lines 405–410: This paragraph is just listing a number of effects that could have an impact, some less interesting (like the well-known pressure-dependency of the melting point), and some very interesting, like potentially plume-eroded channels in the shelf. I recommend reviewing a bit more of the relevant literature***

on subglacial discharge plumes (some suggestions in the reference list below) and revisiting your melt rate parameterization, and carefully considering what are the clear factors impacting the distribution of melt in this model, and what are factors that are excluded in the model, but could potentially still impact the melt rate.

AR: Thank you for the comment. Please see Minor Comment above (l. 386-388) for clarification regarding the plume-eroded ice shelf basal channels. Please also see Major Comment 1 and 2 addressing the missing subglacial discharge and its implications on our results.

RC: ***Lines 413–415: How does this relate to depth/thermocline?***

AR: Thank you for the comment. This has not been addressed in their study.

RC: ***Lines 416–417: Elaborate on what is the significance of no subglacial discharge. What sort of error does that cause in the results and why are the melt rates given in this study relevant?***

AR: Thank you for the comment. We have now addressed this following the Major Comments on subglacial discharge. Please see the revised text in Major Comment 1 and 2.

RC: ***Lines 413–414: what inconsistencies, and how is the Table relevant? Elaborate on what is the meaning and impact of these inconsistencies to this study.***

AR: Thank you for the comment. We believe this is in reference to l. 418. The inconsistency is simply referring to the difference in sea ice cover between the model run (year-round Landfast ice cover representative of late 90s/early 2000s, and Table 1 provides details) and observations (2011-2015); which is one of the many reasons that limits like for like comparisons between model and observational estimates. Please see the revised version in Major Comment 2.

RC: ***Line 420: What novel mechanisms do you mean?***

AR: Thank you for the comment. Regarding novelties, please see Minor Comment on l. 70-72. But more importantly, we were intending to imply that even though our model has several limitations when compared to reality (thus limiting like for like/robust comparisons), the scope of our work isn't to provide (quasi-)realistic values of PGIS basal melt (akin to remotely sensed/observational estimates), but rather to distinguish how long-term changes in regional sea ice cover influence PGIS basal melt. Please see the revised version in Major Comment 2.

RC: ***Lines 420–421: I think you want to say that even though you are not calculating the melt rate correctly, the relative changes caused by a change in sea ice are robust. You could also point out that your estimates of the change in the Atlantic Water properties at the mouth of the fjord are only little impacted by the lack of subglacial discharge. It would be really good if you could quantify how much you're tweaking the turbulent transfer and neglecting subglacial discharge impacts the circulation regime. I suspect you have runs, at least in the Thin Mobile configuration with different turbulent transfer coefficient? How much does the change in the coefficient mean in terms of meltwater flux from the glacier? Or in terms of inflow over the outer sill? It is hard to compare to the buoyancy flux from a plume, but some attempts at quantifying this uncertainty should be made. You could just attempt to make an estimate of the relative increase in the plume melt rate based on the increase in Atlantic Water temperature you project, see Ezhova et.al., 2018. This is exactly why I suggest on taking the focus more towards the change in the inflowing Atlantic Water properties and heat transport into the fjord, since you are on a more solid ground in terms of reliability of the results.***

AR: Thank you for the comment. Yes, that is what we intended to say. Please note that following the comments from Reviewer 2, we have fixed the cold bias and the tweaking of the turbulent transfer coefficient. The implications of the omission of subglacial discharge on our results have been discussed. Moreover, the context in which melt rate and associated anomalies from this study should be interpreted have been explicitly indicated (and we have also pointed out that the role of subglacial discharge and its interplay with sea ice cover changes is beyond the scope of this work). Please see Major Comment 1.

RC: ***Line 423: What kind of estimates are these, model?***

AR: Thank you for spotting this. Please see our correction below:

> ...well with the remotely sensed estimates of Wilson..

RC: ***Lines 440–444: This is too long, break into shorter sentences. You're talking about basal temperatures and calving front temperatures within the same sentence. Be clear in what you mean. Give references to undercutting.***

AR: Thank you for the comment. We have revised the sentence and added relevant references.

> Under a Thin Mobile sea ice cover, ~~besides changes in the summer u* (see Figure 8(i), Sect. 4.2.3) and the convectively upwelled AW during winter (Figure 8(e)) that drive the basal melting under the deeper regions of the PGIS base,~~ the substantial increase in ΔT near the PGIS calving front due to shortwave warming over open ocean (Figure 8(f)) could have serious implications for enhanced melt driven undercutting and glacier calving (Rignot et al., 2015; Slater et al., 2021) in the (near) future as the Arctic sea ice continues to decline in extent and thickness.

RC: ***Line 446: Are you talking about future projections? Are these references predicting a future warming?***

AR: Thank you for noticing this. Please see the revised formulation below:

> The continued warming of the Nares Strait AW observed over the last two decades (Washam et al., 2018; Jakobsson et al., 2020b), could, on ..

RC: ***Line 448: "the upwelling mechanisms" this is too vague. I know by know what you refer to with this, but be more specific. Upwelling where?***

AR: Thank you. This is correct, we have revised this sentence to be more specific. Please see below.

> The presence of a considerably warmer AW in the future is likely to amplify the heat upwelled in the Nares Strait, and thus, the heat content of PF.

RC: ***Line 448: Speculate also what would the impact from increased freshwater flux from Greenland be***

AR: Thank you for bringing this to our attention. We have included this as follows:

> The increased freshwater flux resulting from elevated basal melting and (largely in summer) subglacial discharge (Ehrenfeucht et al., 2022) would further strengthen the buoyancy driven fjord-scale circulation and basal melt.

RC: ***Lines 452–460: If you analyze the results in density space as I have suggested, you can add here that your model is able to show the density-driven inflow over the outer sill from Nares Strait into the fjord that fills the deep basin and reaches the grounding line.***

AR: Thank you for the comment, and moreover, for informing us about this useful diagnostic. Please see the revised figures in Sect. 2. We will include this sentence here.

RC: ***Line 452 Wind-driven upwelling, here and elsewhere***

AR: Thank you, we will fix this, here and elsewhere.

RC: ***Line 464: Be very careful, I don't think you have shown that wind drives the circulation in the PGIS cavity. Wind drives the upwelling of AW in Nares Strait and I presume that density drives the circulation in the fjord.***

AR: Thank you for the reminder. Please see Minor Comment on l. 329-332. We believe that wind does act as a catalyst in enhancing the buoyancy driven fjord-scale circulation. Indeed, the circulation is buoyancy driven, with wind acting to intensify it further. Please see the reworded sentence below:

> ..that wind intensified buoyancy driven ocean circulation results in..

RC: ***Lines 469–470: Did you not prescribe this in the model? Then it's hardly surprising that there is agreement.***

AR: Yes, this is correct. Learning that fast ice prevailed for up to ca. 270–300 days each year during the late 90s - early 2000s provided motivation for constructing the year-round Landfast experiment. Note that the norm is that fast ice occurs in winter, whereas summer has mobile ice cover. In these sentences, we were trying to emphasize that observations support having a fast ice cover over both the model winter (days 30-105) and model summer (days 170-245) periods, since later on, seasonal increases in the Mobile and Thin Mobile runs are shown over these periods. By doing so, we are able to validate that the increase seen in our study is not exaggerated, but quite plausible. However, we do realise now that this fits well in the Methods chapter, and we will move this there, and remove it from here:

> ~~Between 1996-2002 ... with respect to it, are plausible.~~

RC: ***Lines 470–475: You have set up the model to represent the conditions so this is not really discussion. You should rather move this to where you describe the runs and say that the experiments represent approximately the conditions of this and that period.***

AR: Thank you for noticing this. We have addressed this in the comment above.

RC: ***Lines 475–477: I like the sentence, but again, it is not clear that wind drives the inflow into the cavity, see Carroll et.al., 2017.***

AR: Thank you for the reminder. Please see the reworded sentence below:

> To that end, the wind enhanced winter ...

RC: ***Line 478: Remove the "interpreted", and reformulate to state that this is your model result.***

AR: Thank you for the suggestion. Please see the reformulated version below:

> Modelled changes in temperature and inflow alter the (modelled) heat budget of the PGIS cavity, ...

RC: ***Lines 481–482: Reformulate, "not all of the heat is taken up by ice melt", etc.***

AR: Thank you for the suggestion. Please see the reformulated version below:

l. 482: ...and *Thin Mobile* runs. Moreover, not all of the heat is taken up by the ice shelf basal melt, with much of the H_{IN} modelled to leave the cavity (H_{OUT}) without triggering any melt (Table 2).

RC: ***Lines 486–488: Again, I don't think you have properly analyzed the drivers to say this.***

AR: Thank you for the comment. Please see the revised figures in Sect. 2. Please note that this is written simply in regard to the higher increase in temperature seen in Figure 9 (a,e) (i.e. Landfast to Mobile - wind upwelled) when compared to Figure 9 (b) (i.e. Mobile to Thin Mobile - winter convective overturning).

RC: ***Line 493: The summary is very long and slightly repetitive, you should compress, and highlight the results that are new from this study.***

AR: Thank you for the feedback. We are in complete agreement, and we will write a concise version highlighting new insights from this study. Please see Minor Comment on l. 535-548.

RC: ***Lines 500–534: you don't have a bullet point explaining your findings on the impact of a change in sea ice to Nares Strait, and you don't have a bullet point dedicated to circulation in the fjord. Several bullet points describe similar points regarding melt rate, and are thus quite repetitive.***

AR: Thank you for the feedback. We have taken these into account in our concise summary. Please see Minor Comment on l. 535-548.

RC: ***Lines 535–539: References?***

AR: Thank you for bringing this to our attention. Please see the revised version below.

RC: ***Lines 535–548: This is borderline Discussion, and too long, write a short and clean paragraph on how your results describe the potential future, uncertainties are for the Discussion. Skip the a and b.***

AR: Thank you for the feedback. We have taken these into account in our concise summary. Please see below:

Here, we have presented results, showing in unprecedented detail, the impact of climate warming driven long-term changes in Nares Strait sea ice regimes following the loss of stabilizing ice arches on the basal melt at Petermann Glacier Ice Shelf (PGIS), North-West Greenland. Results were obtained using the unstructured grid, free-surface, 3-D primitive equation Finite Volume Community Ocean Model, amended by an ice shelf and sea ice module, and adapted further to render a nested high resolution 3-D regional model setup centered over PGIS and Petermann Fjord (PF), featuring an improved sub ice-shelf bathymetry and a realistic ice shelf basal topography (Zhou and Hattermann, 2020; Prakash et al., 2022).

Three experiments were set up, differing in sea ice concentration, thickness and mobility. These were characterised by a year-round landfast and thick (Landfast run), mobile and thick (Mobile run) and mobile and thin (Thin Mobile run) sea ice cover. We find that in each regime, seasonality is driven by a warmer AW which increases the thermal driving (ΔT) under the deeper regions of the PGIS base. This increases the basal melt locally and strengthens the melt driven fjord-scale overturning circulation, which in turn increases the friction velocity (u^*) (and melt) slightly downstream. The increased meltwater production and transport from depth to the shallower seaward regions of the PGIS base acts to reduce ΔT and basal melt, with the winter season exhibiting a converse pattern. As the sea ice becomes mobile and thin, both wind and (winter) convectively upwelled warm AW from the Nares Strait enter the PGIS cavity, and where warming is not limited to the shallower (<200 m) regions of

PGIS (as in Shroyer et al., 2017). Additionally, wind enhanced inflow intensifies, particularly during summer, where it acts in concert with the stronger melt overturning to generate substantial shear driven turbulence, which is sufficient to drive considerable increase in melt without the need for an increase in ΔT. We find that a transition from a winter fast ice cover to a summer thin mobile cover yields a twofold increase in basal melt.

Remotely sensed and in-situ observations over the past two decades from the Lincoln Sea - Nares Strait region provide evidence for a sustained increase in ocean temperatures and a continued decline of ice stoppage duration aided by the collapse/absence of ice arches which are becoming increasingly common (Washam et al., 2018; Moore et al., 2021). As warming of the Arctic Ocean and decline of its sea ice coverage and thickness is projected to continue until the end of the 21st century (Shu et al., 2022), the scenario presented in the Thin Mobile run is likely to continue in the future, and may amplify further. Thus, the intensified basal melting of PGIS, particularly, of its dynamically resilient deeper regions near the grounding line (Hill et al., 2018), under a likely year-round mobile and thin future regional sea ice cover, could accelerate mass loss from Petermann Glacier, potentially increasing Northern Greenland Ice Sheet's contribution to future sea level rise (Mouginot et al., 2019).

2. Revised figures and table

RC: ***Figures and Tables***

AR: Thank you for carefully analysing our figures and tables, and for providing useful suggestions to change/modify them. Following the comments from Reviewer 2, we have rectified our boundary conditions so as to alleviate the mismatch with respect to the temperature and salinity observations from the fjord, and to use an appropriate Γ_T value. We note that this has added to the value of our study, however, the qualitative assessment of our study and the synopsis remains the same. We will adjust the numerical values associated with the figures wherever necessary.

In order to communicate the new results, all such figures have been included below. Also, as suggested, we have included the new along-fjord flow diagnostics. We prioritised sharing the revised results from the bias-corrected runs, and thus, some minor adjustments that were suggested, and those that are also dependent on the manuscript page layout (e.g. optimizing the white spaces, including full experiment names (not abbreviations) etc.) have not been added yet, but will be included in the revised version of the manuscript. Thus, our response to the comments below details the steps that will be taken to make the (remaining) modifications, and also includes samples of the revised figures from the bias-corrected runs which are presented following the suggestions below. Please also note that between correcting the bias, tuning the model, running the simulations and analysing the output, we did not have sufficient time to focus on the presentation of the figures, and thus, some minor cosmetic adjustments/cleaning could be expected, as we get more time to work on it.

RC: ***Figure 1: I suggest putting quite a bit more effort to this figure. In 1a, I suggest cropping much closer to the domain, now much of the figure are is out of the domain, and the area of interest is too small. You can add a small inset figure indicating the location on a larger scale map. The blue and green colors of the background map are too dark, the red and yellow are too hard to read. The caption says that the sea ice arch locations are marked in the figure, but I cannot see them. In 1b, use different, brighter colors in the lines, they cannot be distinguished from this figure. I suggest including an along-fjord profile and indicating the sills, the location of the cross-sections and all of the other features you refer to later in the***

paper (for example Fig. 3a). This is important since bathymetry and draft are the key novelties in this study, and the inner sill is a new(?) feature. In all figures be more consistent with fonts and font sizes, the figure looks busy with so many different fonts and sizes.

AR: Thank you for this very valuable comment, and also for providing such detailed instructions on how to improve this figure. We have followed up on all of the suggestions, please see the revised figure below. Regarding the along-fjord profile (here, and in the suggestion for Figure 3), we realised that it is useful to provide the water column thickness map instead, since it provides a complete topographical picture of both the ice shelf covered, and ice shelf free regions of our domain. In this panel, the location of all relevant features mentioned later have been marked. We will make sure to be consistent with font and font sizes.

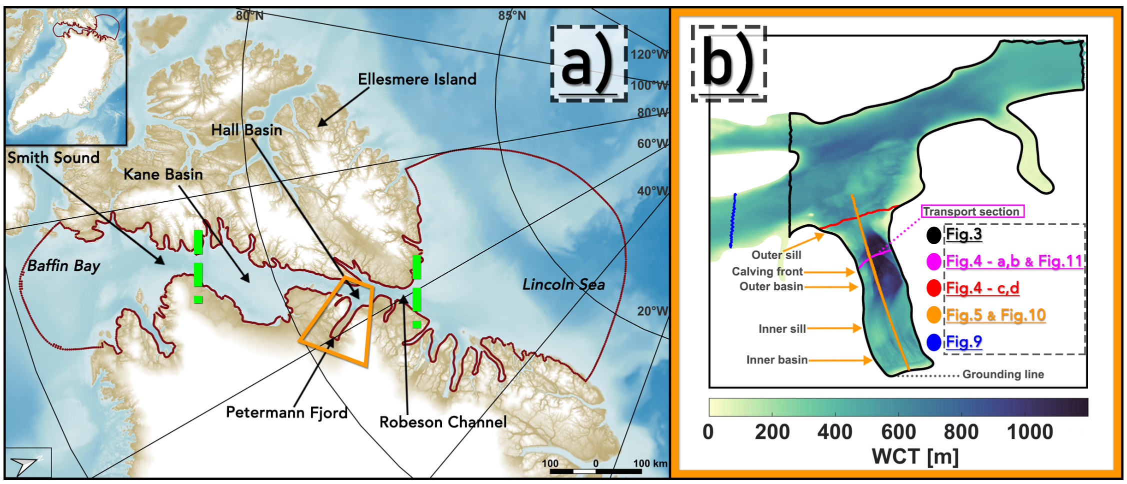

Figure 2: **Revised Figure 1**

RC: ***Figure 2: This figure is difficult to follow, but that can be fixed easily. I suggest the following changes: First column should be the Landfast experiment, since that represents earlier, "historical or pre-industrial", conditions in this paper. Do not use the abbreviations, use the full names of the experiments. Consider adding the words summer, winter and year-round as column headers.***

AR: Thank you for this comment. *"...since that represents earlier, "historical or pre-industrial..."*: Indeed, this is very true, and we agree completely with all of the suggested changes, and will update the plot accordingly in the revised version.

RC: ***Table 1: Once you have revised the text describing the initialization of the experiments, remember to revise the caption accordingly. It is now impossible to grasp which run starts from when and where. Remove the abbreviations, and update the experiment names. Give the duration of the summer and winter seasons also in days or months so that the reader does not need to calculate, and would be better to give the time in calendar days. Remove the "Standard run".***

AR: Thank you for the reminder. We agree with all of the suggested changes and we will revise the table accordingly. Abbreviations will be dropped throughout the manuscript, and experiment names will be used instead (text, tables and figures). "Standard run" will not be referenced anywhere in the manuscript.

RC: ***Figure 3: Review the use of space in this figure. White space and labels take more space than the actual***

results you attempt to show. Seasonal mean speed of what? You should say that these are ocean currents. Consider adding a small inset in the empty space of 3b) that would show a line plot of the mean depth along the fjord vs the Y-coordinate. Check that all of the features indicated in red in 3a) will be also indicated in the along-fjord profile of Fig. 1c). Indicate the gyres discussed in the text with arrows, and refer to the figure when describing the gyres.

AR: Thank you for the comment. We agree, here and elsewhere, we will optimize the white space. *"You should say that these are ocean currents."*: Indeed, we will include this. All of the features in 3a) have been indicated in Fig. 1b). We will indicate the gyres in the figure and add reference to the figure. Please note that Reviewer 2 hinted towards a possible error in this plot. During our investigation, we realised that we had not transformed the velocities (u,v) from the model output to u_{x} and v_{y}, and thus, the streamlines were not forming a closed contour. This has been addressed now, and the revised plot has been presented below. We note that this does not change the general (qualitative) interpretation that was inferred from the figure.

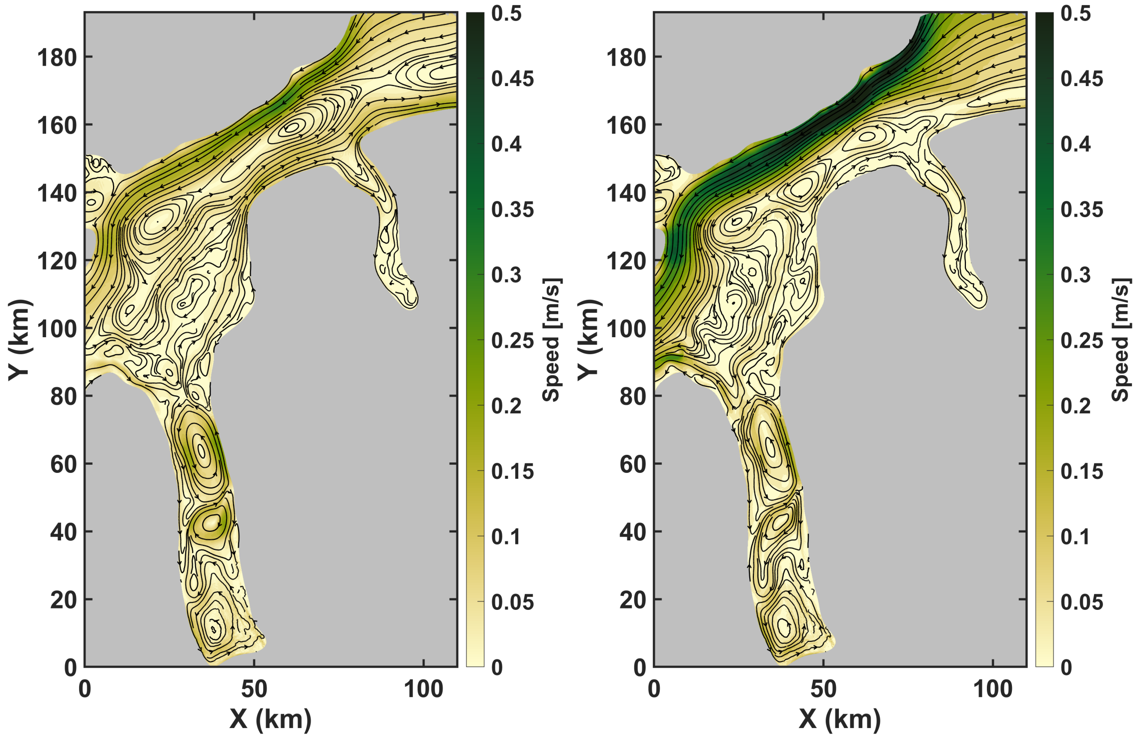

Figure 3: Revised Figure 3. Winter = left. Summer = right. Additional changes will be included in the final version.

RC: ***Figure 4: Why are the inner profiles (4a,b) above the outer ones (4a,d)? I think it would be more intuitive the other way around. I do not know how these cross sections relate to the fjord bathymetry (See comments to Fig. 1). I the black color on top of the water column the ice shelf? If yes, mark it with a different color than the terrain, and use the same color for the ice shelf throughout the figures. What is the gap in the ice shelf in 4a,b? Increase the font size of the temperature values of the isotherms. Blue and red are not good font colors on a blue-red background. Why the cropped width? There is some white space in between.***

Include in the caption which way is positive (towards the glacier?)

AR: Thank you for this helpful comment. We agree with all the suggestions, and a revised plot has been presented below as an example. We believe that the water column thickness map helps to relate the cross sections to the fjord bathymetry. The black colour on top of the water column was the ice shelf (now shaded in gray, and this colour will be used consistently throughout the figures). The terrain is now shaded in brown, and this colour will be used consistently throughout the figures. Regarding the gap: Due to the geometry of the calving front of the ice shelf and the unstructured grid ("zig-zag") cross-section, a small patch of open ocean region appeared when we defined the cross-section. We will acknowledge this in the caption. As stated above, we will adjust the width and white spaces in the final version. Indeed, positive is towards the glacier. These are mentioned in the text, and we will add it to the caption.

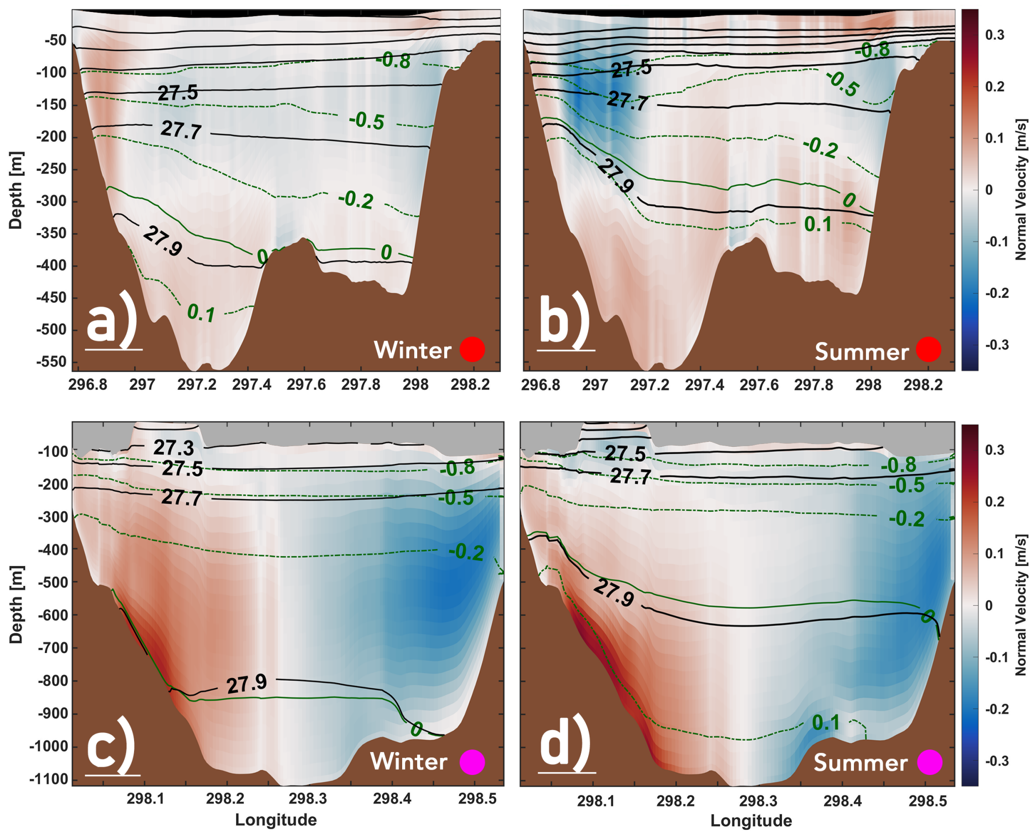

Figure 4: **Revised Figure 4**

RC: ***Figure 5: This is the first along-fjord figure of the paper, much overdue, and overall, quite unhelpful. Differentiate the ice shelf from the terrain by using the same color as in Fig. 4, now it is difficult to see the GL. I don't think the annual means are valuable at all, I would just remove them. Similarly, showing anomaly relative to annual mean is not meaningful. Also, the salinity anomaly is also rather uninformative, density should be salinity-dominated here anyway. I recommend showing isopycnals for both seasons, as well as temperature for winter and summer. Plot the isopycnals at even intervals, in 5a) it is hard to see the***

pycnocline since the contours jump from 27.1 to 25.9. You can explain the range of annual variability in the text. Consider also including the along-fjord heat transport overlaid with streamlines. Otherwise, we are not seeing along-fjord flow at all. This would mean four panels, which would make it possible to increase the panel size a bit.

AR: Thank you for bringing this to our attention, and more importantly, for the valuable suggestions. We have presented a revised plot below, following up on all of the minor and major changes that were suggested, which shows the absolute temperature for winter and summer (panels (a) and (b), respectively), with corresponding isopycnals (plotted in equal intervals of 0.2) overlaid. We have also included the winter and summer along-fjord flow (red = in, blue = out) overlaid with corresponding isopycnals (panels (c) and (d), respectively). Left column = winter, and right column = summer, which will be added to the figure in the final version.

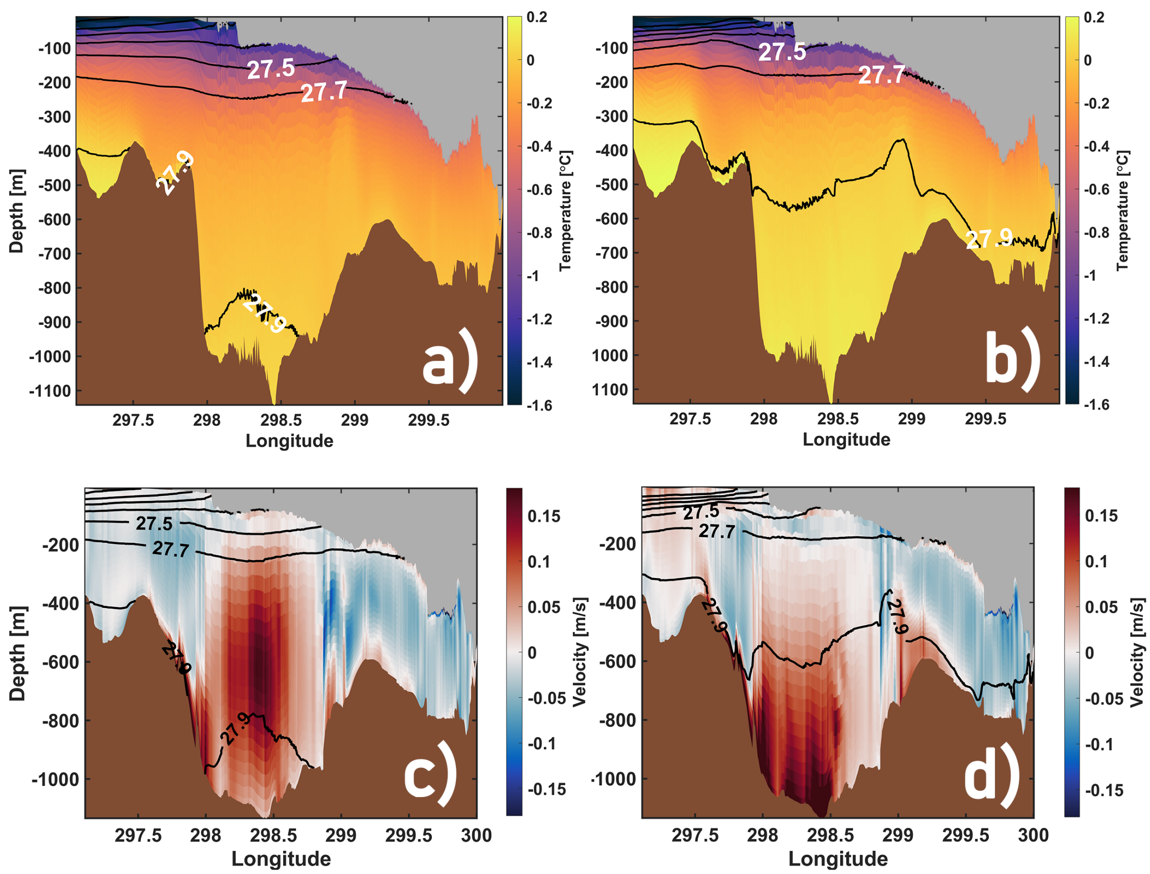

Figure 5: **Revised Figure 5**

RC: ***Figure 6: There is a lot to unpack in this figure, and again, I do not see the point of showing anomalies relative to annual mean. If you want to plot anomalies, why not plot the summer increase relative to winter? However, I would prefer to just show melt rate in winter and melt rate in summer with the same color range, on a much bigger figure. I find panels d—i deeply unhelpful, and I do not see the point. If you are trying to show the relative contributions of temperature increase and the increase in friction velocity***

to the melt rate, it is not working. Why are these anomalies? Could you just show the water temperature at the base of the shelf in summer and winter, and then the friction velocity. So as in e,f,h and i but in absolute terms? I can't imagine if it will be better, but something should be done. Also, note that you have not defined anywhere the friction velocity. Turn panels d and g so that ice draft is along the y-axis and increases downwards, it's more intuitive. Also, I recommend plotting temperature and friction velocity in absolute terms for both winter and summer. There is a significant amount of white space in the figure so should be possible to reorganize the figure so that the shelves appear bigger, right now they are too small.

AR: Thank you for the suggestions. We now show the winter (left column), summer (middle column), and the increase in summer relative to winter (right column) plots for the Landfast run. Top row = melt rate. Middle row = thermal driving. Bottom row = friction velocity. In the final version, we will turn the panels to plot the ice draft increasing downwards along the y-axis, and as stated above, we will optimize the white spaces.

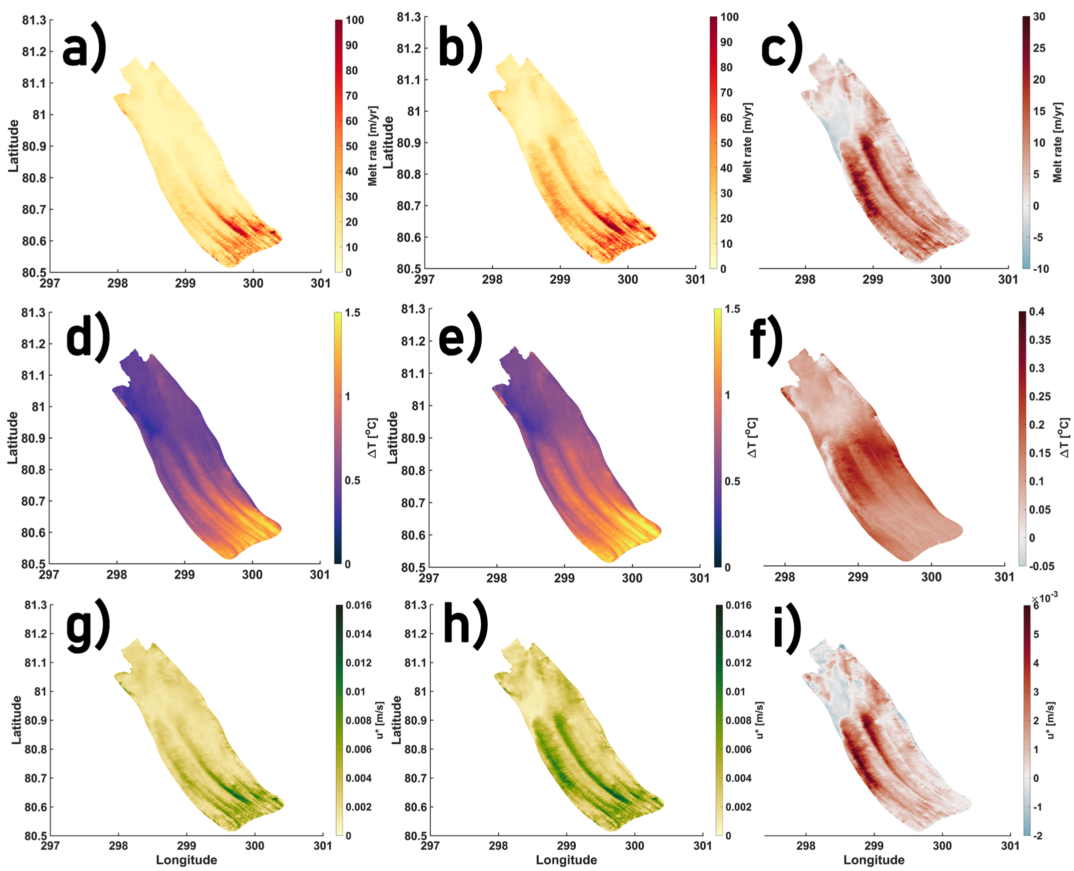

Figure 6: **Revised Figure 6**

RC: ***Figure 7: I recommend to plot in panels b and c the absolute annual mean melt rate in the Thick mobile and thin mobile experiments. This way your first row is comparison of annual mean melt rates. If you have included summer and winter melt rates (not anomalies) in Fig. 6, you can show panels e,f,h,i as they are. However, write on the panel if they are winter or summer, and use the full names of the runs***

instead of abbreviations (landfast to mobile etc.). Flip panels d and g to have the draft as y-axis increasing downwards. Reduce white space and make the shelves appear bigger. Reorganize to have seasons in columns, as you have in other figures. Use different colors to plot the substractions in panels d and g to avoid confusion, and keep the colors also in other figures.

AR: Thank you for the comment. The relative increase in the annual mean melt rate in the Thick Mobile and Thin Mobile runs are not easily discernible if plotted as absolute values, largely due to the fact that the largest increases occur under the deeper drafts where melt rates are quite high. Thus, we prefer to keep panels b and c as anomalies. *"If you have included summer and winter melt rates (not anomalies) in Fig. 6, you can show panels e,f,h,i as they are"*: This is what we have done. As stated above, we will include all other suggestions that are not presented yet in the figure below in the final version. Regarding the figure label, please refer to the manuscript, since the panel content and panel layout are the same.

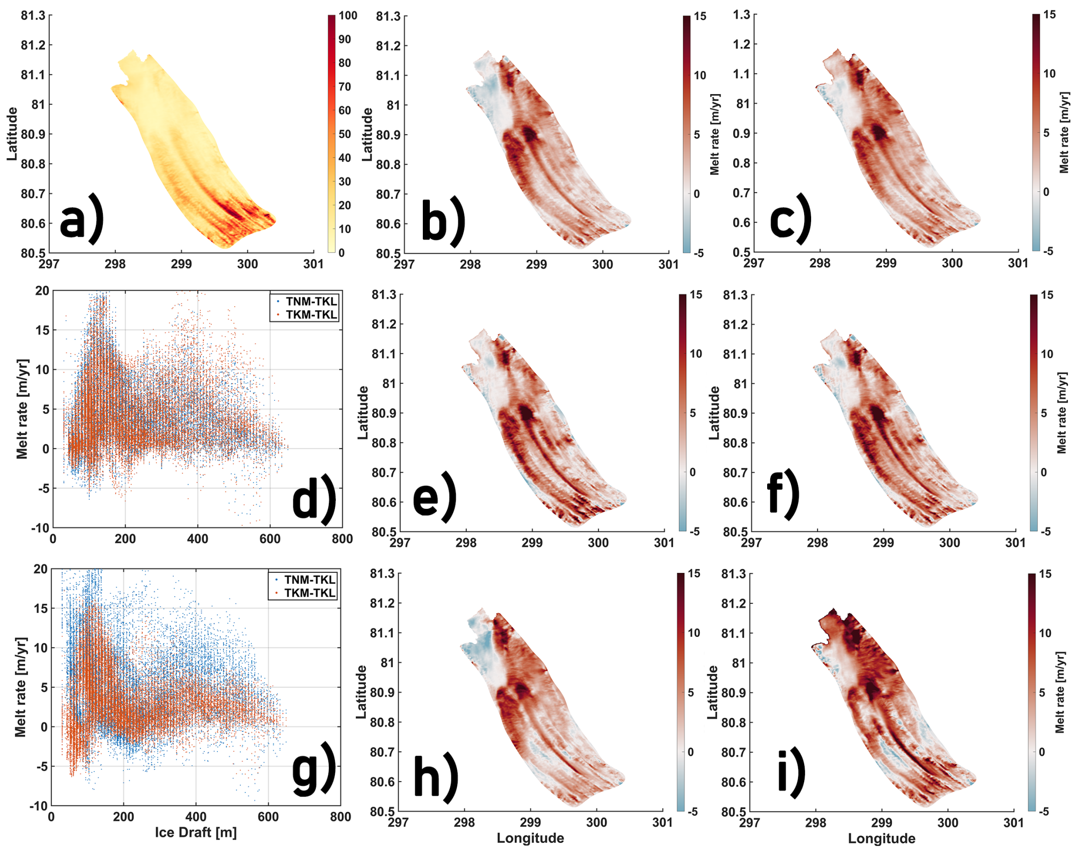

Figure 7: **Revised Figure 7**

RC: ***Figure 8: Flip axes and change color as in the previous figures. Here I think the difference in annual mean makes sense.***

AR: Thank you for the suggestions. As stated above, we will make these adjustments in the final version. Regarding the figure label, please refer to the manuscript, since the panel content and panel layout are the same.

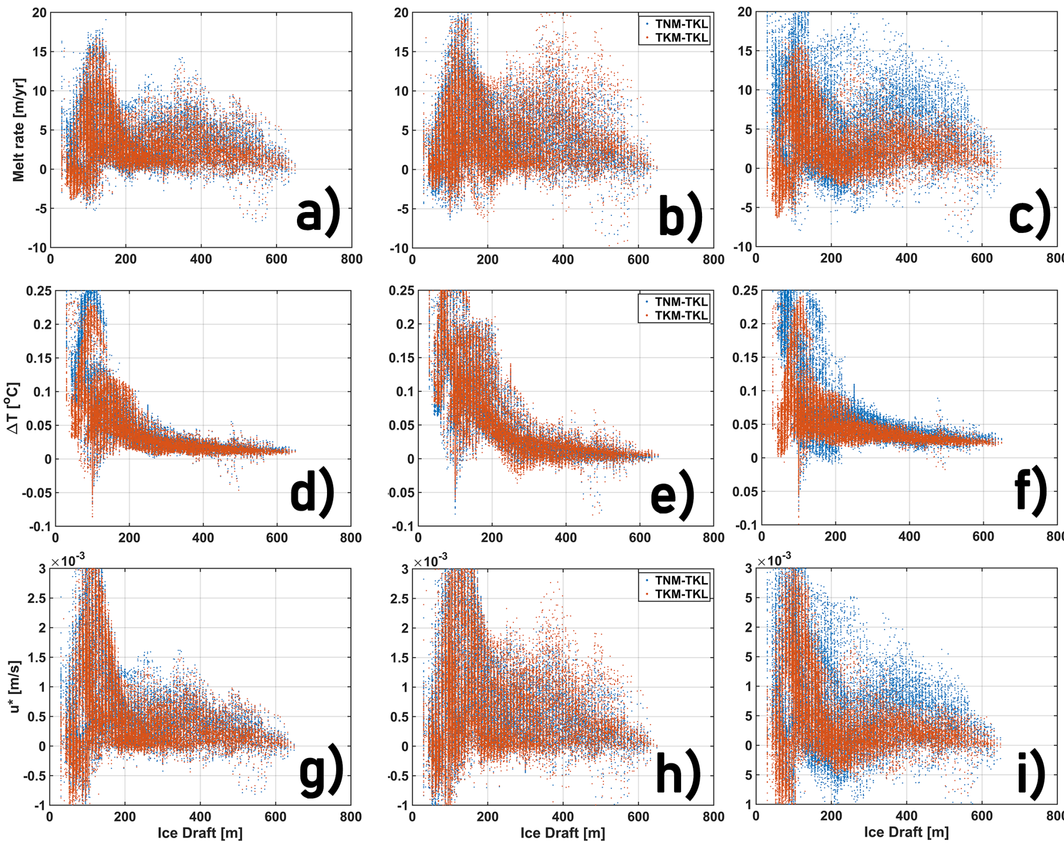

Figure 8: **Revised Figure 8**

RC: ***Figure 9: A lot of the same comments as earlier. Change the color of the ice shelf, keep the seasons in columns to be systematic in your figures and write summer and winter on top of the columns, do not use abbreviations of the experiment names but spell out the names. Again, I think in order to understand to flow under the ice shelf, we need to see isopycnals for both seasons and both, and since intensification of the circulation was a key result, you should plot the flow somehow, for example along-fjord velocity in blue-red. You could save space by skipping the difference in salinity if you show isopycnals, and only show the seasonal difference in temperature.***

AR: Thank you for the helpful suggestions. The ice shelf is shaded in gray and the bottom is shaded in brown. Winter = left column and summer = right column. As stated above, we will put the full experiment names in the final version. We have revised the figure to show winter and summer mean temperatures with corresponding isopycnals overlaid. Also shown are winter and summer mean along fjord (blue = out, and red = in) flow with corresponding isopycnals overlaid. Previous figure 9 (from the manuscript) is also shown below and we suggest to move it to the appendix.

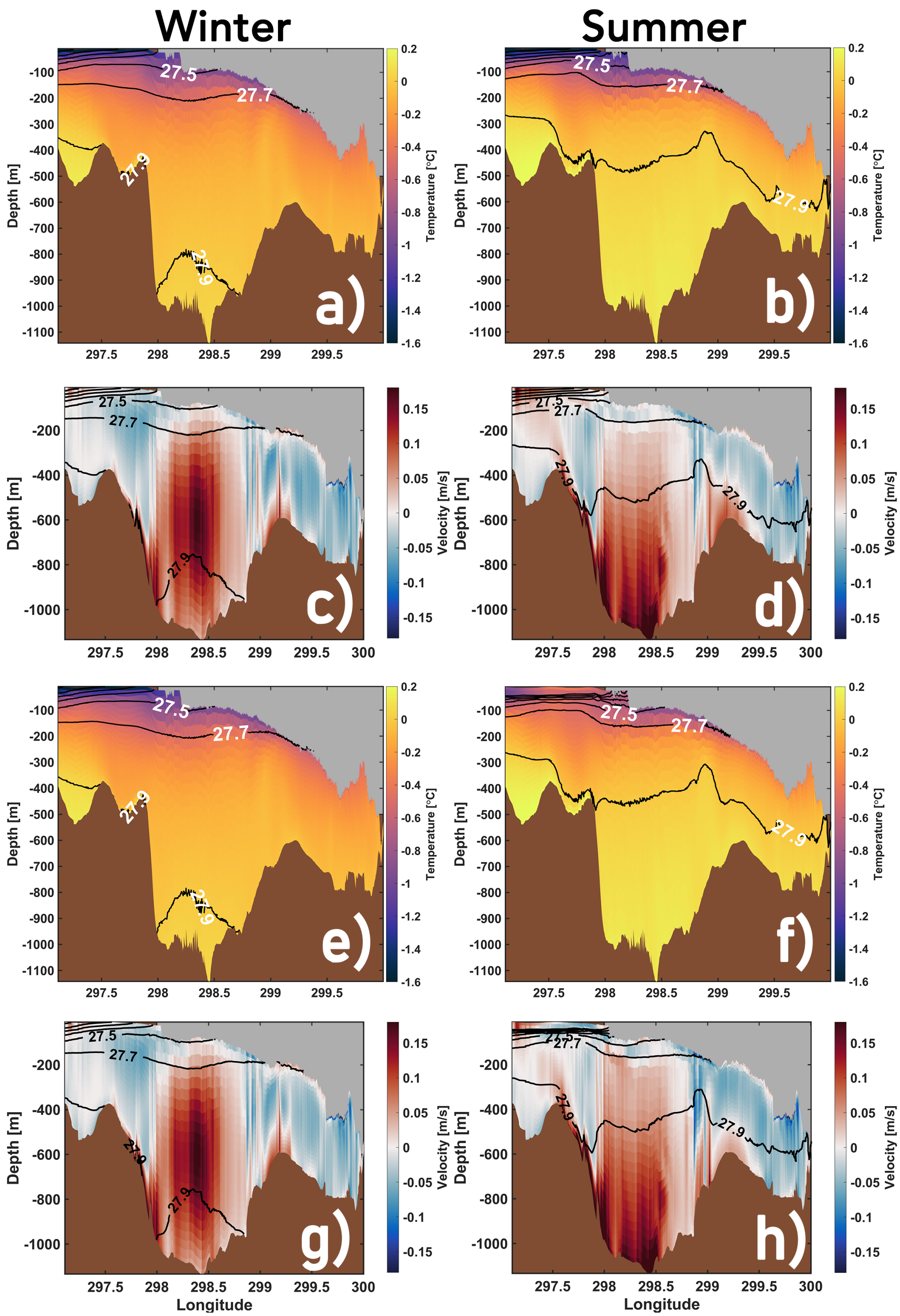

Figure 9: **New figure 9.** Winter = left column. Summer = right column. Panels (a)-(d) = Thick Mobile run. Panels (e)-(h) = Thin Mobile run. Temperature with isopycnals = a,b,e,f. Along fjord flow with isopycnals = c,d,g,h.

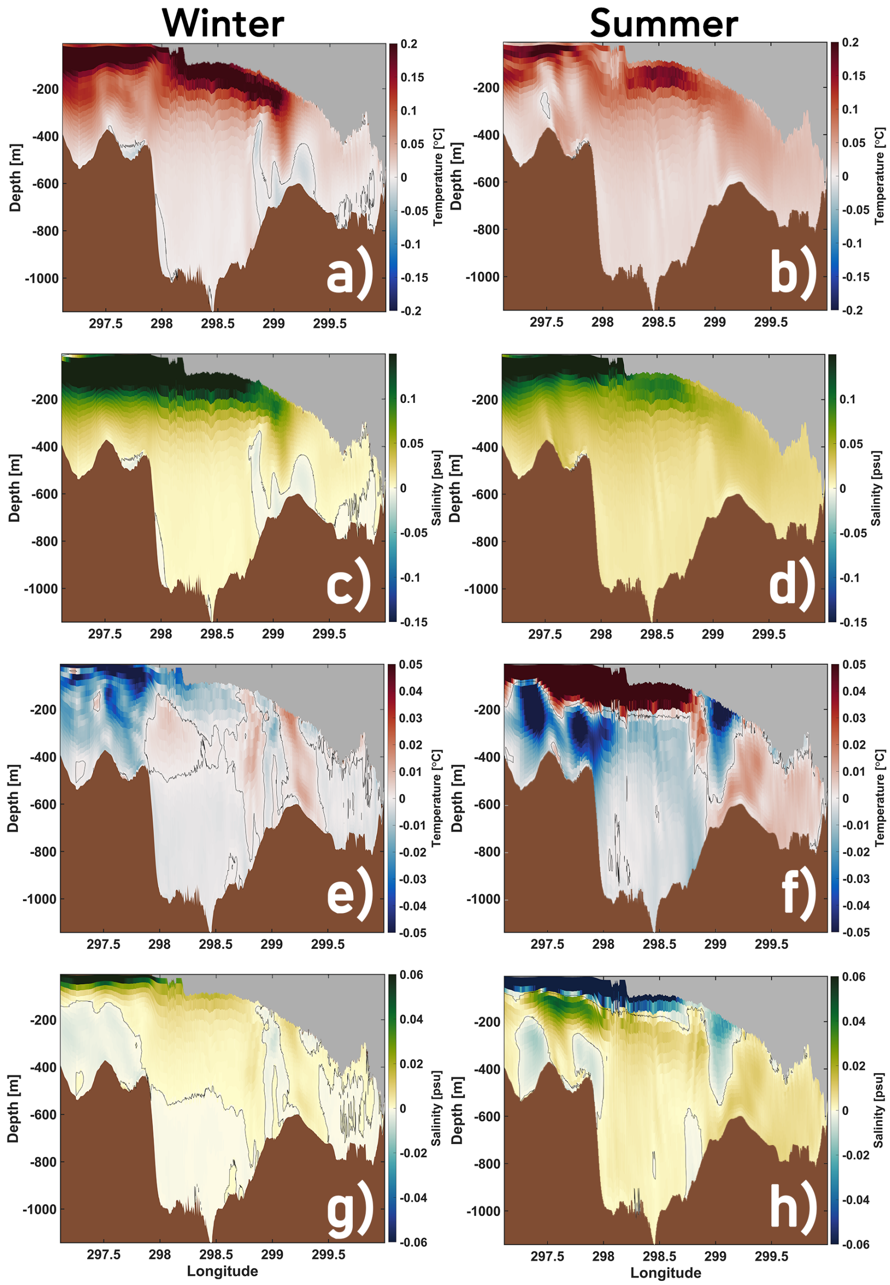

Figure 10: **Revised figure 9. To be moved to the appendix**. Panels (a)-(d) = Going from Landfast to Mobile. Panels (e)-(h) = Going from Mobile to Thin Mobile.

RC: ***Figure 10: In panels a—d) is there a black (sea ice) layer on top, or is it the darkest color of the color scale? If it is ice, plot it with a different color, if it is the darkest shade of the color scale, plot the terrain in brown (also in other figures). Consider writing Nares Strait on the figure, so that it is clear without reading***

the caption that this is not the fjord. Write winter and summer on top of the columns. Do not use the abbreviations of the experiments, and do plot isopycnals on this figure. 10 e and f are really informative and important, it is important that this figure comes earlier.

AR: Thank you for the helpful suggestions. The black on top is the darkest colour of the scale, and is now used consistently throughout the figures. The terrain has been shaded in brown in all the figures. Ice shelf, wherever present, is shaded in gray. Nares Strait is mentioned as the figure title, and the seasons are mentioned as column titles. Equally spaced seasonal mean (absolute) isopycnals (interval = 0.2) have been overlaid. Here, magenta isopycnals = Landfast run, red = Mobile run, and black = Thin Mobile run. 10 e and f are the same as in the manuscript. We will create a separate figure with these two panels and move them earlier.

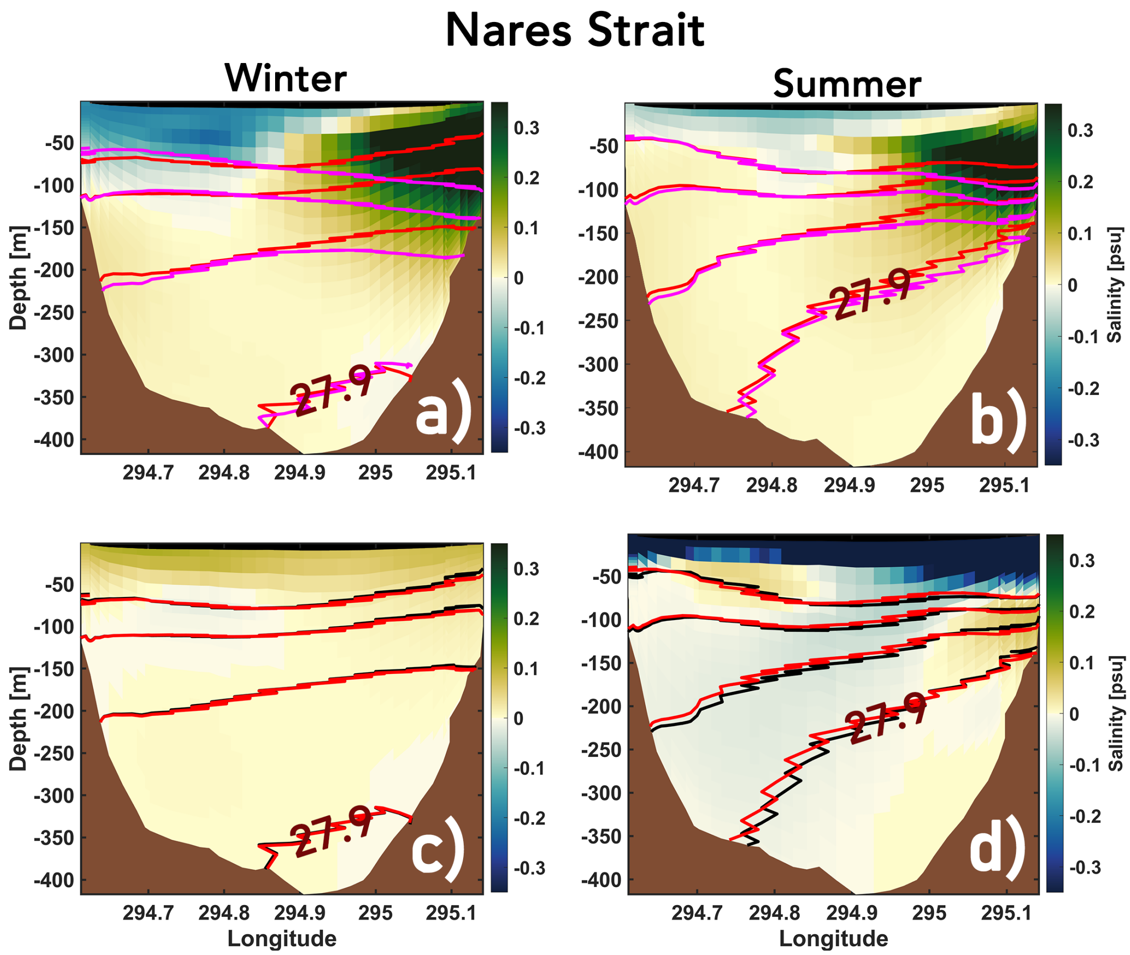

Figure 11: **Revised Figure 10**. Panels (a) and (b): Going from Landfast to Mobile. Panels (c) and (d): Going from Mobile to Thin Mobile.

RC: ***Figure 11: Keep seasons in columns, write winter and summer on the figure, do not use abbreviations, use a different color for ice to separate it from terrain. State in the caption with words where this cross section is from. Add the colored circle indicating the section, as you had in the previous fjord section***

figure. Check from the model what happens in the narrow return-flow column in 11d, and state that in the caption, it draws a lot of attention. It seems to be related to the gap in the ice shelf.

AR: Thank you for the comment. We have presented below a revised version of the figure which takes into consideration all of the aforementioned suggestions. We will include the location of the cross section in the caption. Please see the minor comment above where the gap in the ice shelf has been discussed. We will add it to the caption.

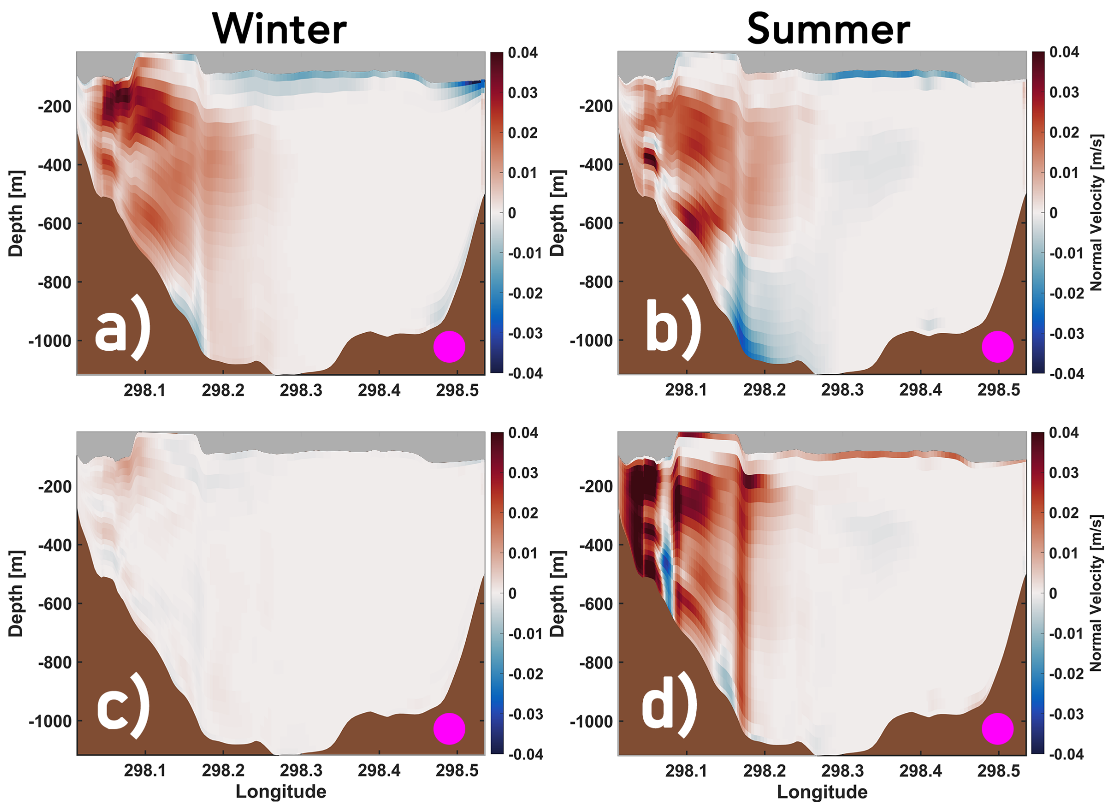

Figure 12: **Revised Figure 11**

RC: ***Table 2: Write the experiment names, no abbreviations. It is not clear where this is calculated from, is it the fjord mouth corss-section?***

AR: Thank you for the comment. As stated above, the abbreviated version of the experiment names will be dropped from the manuscript. The transport is calculated from the "transport section", as shown in Figure 1(b) and stated in its caption. We will state this in the Table caption again.

3. References

Below, we provide references that have been included in addition to those that are already provided in the manuscript.

1. Cai C, Rignot E, Menemenlis D, Nakayama Y. 2017. Observations and modeling of ocean induced melt beneath Petermann Glacier Ice Shelf in northwestern Greenland. Geophysical Research Letters 44:8396–8403.

2. Carroll, D., Sutherland, D. A., Shroyer, E. L., Nash, J. D., Catania, G. A., & Stearns, L. A. (2015). Modeling turbulent subglacial meltwater plumes: Implications for fjord-scale buoyancy-driven circulation. Journal of Physical Oceanography, 45(8), 2169–2185.

3. Carroll, D., Sutherland, D. A., Shroyer, E. L., Nash, J. D., Catania, G. A., and Stearns, L. A.: Subglacial discharge-driven renewal of tidewater glacier fjords, J. Geophys. Res.-Oceans, 122, 6611–6629, 2017.

4. Ehrenfeucht et.al., 2022, "Seasonal Acceleration of Petermann Glacier, Greenland, From Changes in Subglacial Hydrology", Geoph. Res. Lett.

5. Galperin B., Kantha L.H., Hassid S., Rosati A. A quasi-equilibrium turbulent energy model for geophysical flows J. Atmos. Sci., 45 (1988), pp. 55-62.

6. Hager, A. O., Sutherland, D. A., Amundson, J. M., Jackson, R. H., Kienholz, C., Motyka, R. J., and Nash, J. D.: Subglacial Discharge Reflux and Buoyancy Forcing Drive Seasonality in a Silled Glacial Fjord, J. Geophys. Res.-Oceans, 127, e2021JC018355, 2022.

7. Holland D.M., Jenkins A. Modeling thermodynamic ice - ocean interactions at the base of an ice shelf J. Phys. Oceanogr., 29 (1999), pp. 1787-1800.

8. Jenkins A., Hellmer H., Holland D. The role of meltwater advection in the formulation of conservative boundary conditions at an ice-ocean interface J. Phys. Oceanogr., 31 (2001), pp. 285-296.

9. Kajanto, K., Straneo, F., and Nisancioglu, K.: Impact of icebergs on the seasonal submarine melt of Sermeq Kujalleq, The Cryosphere, 17, 371–390, 2023.

10. Keen, A., and Coauthors, 2021: An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models. The Cryosphere, 15, 951-982, 2021.

11. Mellor G., Yamada T. Development of a turbulence closure model for geophysical fluid problem Rev. Geophys. Space. Phys., 20 (1982), pp. 851-875.

12. Peralta-Ferriz, C., and R. A. Woodgate, 2015: Seasonal and interannual variability of pan-Arctic surface mixed layer properties from 1979 to 2012 from hydrographic data, and the dominance of stratification for multiyear mixed layer depth shoaling. Prog. Oceanogr., 134, 19–53.

13. Smagorinsky J. General circulation experiments with the primitive equations, I. The basic experiment Mon. Weather Rev., 91 (1963), pp. 876-882.

14. Straneo, F. and Cenedese, C.: The Dynamics of Greenland's Glacial Fjords and Their Role in Climate, Annu. Rev. Marine Sci., 7, 89–112, 2015.

15. Tsamados, M., D. Feltham, A. Petty, D. Schroeder, and D. Flocco, 2015: Processes controlling surface, bottom and lateral melt of Arctic sea ice in a state of the art sea ice model. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 373, 20140167.

16. Washam P, Nicholls KW, Münchow A, Padman L. 2020. Tidal modulation of buoyant flow and basal melt beneath Petermann Gletscher Ice Shelf, Greenland. Journal of Geophysical Research: Oceans 125:e2020JC016427.