Response to reviewer comments for the manuscript *Stratified suppression of turbulence in an ice shelf basal melt parameterisation*

Second Round of Review
EGUsphere-2024-3513
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We thank the reviewers, Peter Washam and Carolyn Begeman, for their supportive comments on our manuscript, as well as their thorough reading of our revised manuscript. We have endeavoured to address their minor and technical comments in our second revised manuscript. We have also addressed some minor errors found during the preparation of the revised manuscript, reported at the end of this response.

Below we respond to each comment in turn (with our responses indicated in blue) and changes that we have made to the manuscript. Edits to the text are written in purple, and line numbers refer to the revised manuscript.

Reviewer 1

Overall Statement:

The team of authors have submitted a much-improved version of this bear of a manuscript that tackles the difficult, but important problem of including stratification in ice shelf-ocean models. Overall, the manuscript is quite good, with improved clarity and readability throughout. It is a big, chunky manuscript with a lot of detail that will inspire further work. Apart from some parts of the text needing improved writing structure and some improvement of the Methods Section, I feel that this manuscript will only need a minor review to be ready for publication. I have provided an assortment of comments below that are trivial and should not take much time to address. Great job turning around a solidly-revised manuscript!

-Peter Washam

We thank Peter Washam for your encouraging feedback, we appreciate your thorough reading of our manuscript.

Larger Comments:

Some poorly written sections that can benefit from: https://writersdiet.com/writing-test/ and https://www.amazon.com/Elements-Style-Fourth-William-Strunk/dp/020530902X .

Thank you for sharing these references. We have edited the text for improved sentence structure and hope the revised manuscript is improved.

Part of Methods section is still unclear – see comments below.

We have addressed these concerns below.

I don't think it is appropriate to cite manuscripts in review.

The Cryosphere policy accepts articles in review when available as preprints with a doi. All four of the articles that we have cited and are still in review satisfy this criteria. However, given our reliance on the Zinck et al. (in review) Pine Island Glacier melt rate data we agree that it would be ideal if the Zinck et al. (in review) preprint was accepted and published before the present manuscript. We have been in contact with the authors of this manuscript and expect that their paper will be published before the present paper, and have confirmed that their melt rate product has not changed in the review process.

Specific Revisions:

Abstract:

Li 2 – 15: This now fits within the 250 word limit of The Cryosphere. Still, it is quite a long abstract, so consider shortening it somewhat.

The abstract is now shortened to 190 words.

Li 4 – 5: This sentence is hard to follow. Please rewrite improve readability

Rewritten and shortened as

L3-4: Basal melting is controlled by small-scale processes, therefore ice shelf–ocean models rely on parameterisations to predict basal melt.

Li 7: rewrite to "stratification by accumulation of buoyant meltwater beneath a flat ice interface"

Done, but rephrased to "flat and weakly sloped ice interfaces.

Introduction:

Li 19 – 21: Insert somewhere in here that ice shelves have already entered the ocean and displaced sea level.

L15-17: Antarctic ice shelves are the floating extensions of the Antarctic Ice Sheet, and therefore have already displaced sea level. However, they buttress the ice sheet and slow its flow towards the ocean. Ice shelves melt...

Li 25 – 26: add "in the ocean models that produce melt rate projections (IPCC, 2023; Bennetts et al., 2024)."

Done.

Li 28: As someone who does observations, I appreciate it when modelers define a relative scale for models to place them into context. Can you express what scale this "large-scale ocean, climate and earth system models" are operating on? Is this referring to global or circum-Antarctic?

It could be a variety of scales, so we have replaced with 'regional and global':

L24-25: Antarctic ice shelf melting is controlled by ice shelf–ocean boundary layer processes, which occur on scales that are too small to resolve in regional and global ocean, climate and earth system models (Rosevear et al., 2025).

Li 52: You have not defined the sections of the ice-ocean boundary layer yet, so change this to "the far-field flow below the ice-ocean boundary layer"

Sorry, we are unsure what is being referred to, since in both the tracked changes and updated manuscript Line 52 is after the paragraph which defines the ice-ocean boundary layer.

Li 57 – 60: This is a quite verbose sentence with poor structure. Please rewrite to improve readability and be sure to mention that meltwater accumulation and stratification inhibits melting on ice that is flat or has low slopes. This then will lead into the next sentence.

We have rewritten it as

L54-57: In these warmer conditions, and beneath flat and weakly sloping ice shelves, the ice shelf-ocean boundary layer is stratified by buoyant meltwater. The stratification suppresses turbulence and creates a feedback on heat and salt transport, but this feedback is not captured by a constant transfer coefficient (Vreugdenhil and Taylor, 2019; Rosevear et al., 2022b).

Li 62: The Washam et al., (2023) reference is fine if you'd like to cite it, but I think the Schimdt et al., (2023) reference is more appropriate.

Added Schmidt et al. (2023).

Li 67 – 68: This is an incomplete sentence.

If we understand correctly, you are referring to this sentence (parentheses removed): "Other simulations use varying choices of basal melt parameterisations." It is complete, though short.

Li 73: "Vertical discretisation of the basal melt parameterisation" is a little heavy on the jargon. Please rewrite this to make it more clear what is trying to be communicated.

Rewritten as

L71-73: The biases could also be related to choices made in the vertical discretisation of the basal melt parameterisation (Gwyther et al., 2020), such as the sampling distance of the far-field flow conditions. However, it is difficult to determine sources of biases with a lack of observations.

Li 80: Not sure about citing a paper in review unless it will be out before this one.

See response on page 1.

Li 81: Specify what you mean by "future ice shelf regime changes."

Replaced with "future cold–warm ice shelf regime shifts"

Li 86 – 92: This sentence is incredibly hard to understand. I understand that the authors are trying to summarize the important missing processes in models, but please rewrite this with improved sentence structure to improve readability.

We have rewritten it in several sentences as

L83-90: For instance, studies have used idealised simulations and laboratory setups to explore melt-induced convective plumes (Gayen et al., 2016; Mondal et al., 2019; Zhao et al., 2024; Anselin et al., 2024; Kerr and McConnochie, 2015; McConnochie and Kerr, 2018). Idealised studies have also demonstrated the possibility of double-diffusive convection (Rosevear et al., 2021; Middleton et al., 2021), including the feedback of double-diffusive layers on vertical ice shape (Wilson et al., 2023; Sweetman et al., 2024; Guo and Yang, 2025). Other Large Eddy Simulation studies demonstrate the effect of stratification of melting (Vreugdenhil and Taylor, 2019; Rosevear et al., 2022b; Begeman et al., 2022). The effect of vertical resolution on boundary layer structure in turbulence-permitting ice-ocean melt simulations has also been studied (Patmore et al., 2023; Burchard et al., 2022).

Li 99: State the scale of "large-scale." Is this regional scale (1 large ice shelf or several small ones) or circum-Antarctic?

Rewritten as

L97-99: However, thus far, these parameterisations have not been implemented nor tested in realistic ocean models. In this work, we aim to bridge this gap between the insights created by idealised process studies, and the large-scale regional and global ocean models used in climate and sea level projections.

Li 100: The previous statements on scale should help the reader better understand the goal of the paper now.

See above.

Li 106 – 109: I think these sentences also refer to Schmidt et al., (2023).

Added alongside the existing references.

Li 112: What is "large-scale" here? Is this a regional model or a GCM? I would be amazed if a GCM had this sort of resolution.

There is a large range in possible resolutions, hence the "greater than" statement, but we have rewritten it as

L111-113: However, regional ocean models generally have horizontal grid sizes greater than $\mathcal{O}(10^3)$ m (and global models are even coarser) and vertical resolutions $\mathcal{O}(10^1)$ m and cannot resolve ice base variability at the required scales, nor do commonly-used bathymetry and ice base forcing products (Morlighem et al., 2020).

Li 119: Start this sentence with: "In this paper,"

Done.

Li 134 – 142: Nice summary of the upcoming contents of the paper.

Thank you.

Li 161: "Independent of these three unknowns" is vague. Please change to "is known" or something similar.

Technically when we make the transfer coefficient a function of L^+ and therefore the buoyancy forcing (and hence T_b and S_b), the system is no longer quadratic even though the transfer coefficient is known. It is only a quadratic system if the transfer coefficient is independent of m, T_b and S_b. Likewise, the Holland and Jenkins (1999) parameterisation with McPhee (1981) stability term does not reduce to a quadratic equation since it includes a dependence on L, the Obukhov length, which depends on the buoyancy forcing. We have rewritten it as

L159-160: Assuming the transfer coefficient is constant or only depends on known values, this system of equations reduces to a quadratic equation.

Melt Parameterisation Design and Validation:

The Three-Equation Melt Parameterisation and Transfer Coefficients

Li 145 – 146: Correct me if I am wrong here, but the problem is not the three-equation melt parameterisation, it is the inability to resolve the fluxes all the way up to the viscous sublayer, where then heat and salt diffuse at the molecular rate. The wording of these two sentences makes it seem like there is an inherent problem with the three equations that are: 1. Freezing point at ice base, 2. Conservation of heat, 3. Conservation of salt. Please clarify this.

You are correct. We have rewritten it as

L142-144: Ice shelf cavity-scale ocean models cannot resolve the turbulent fluxes within the ice shelf-ocean boundary layer. To address this issue, models generally employ the three-equation basal melt parameterisation (Hellmer and Olbers, 1989; Holland and Jenkins, 1999).

Li 186 – 187: Correct me if I'm wrong here, but I think the take home message from Schmidt et al. (2023) was that while maybe missing the physics, the unstratified shear-driven parameterisation performed closest to the observations under steep slopes.

We have revised the sentence in question so that the citations support the sentence:

L185-186: Significant basal slopes, such as those observed by Schmidt et al. (2023) and Washam et al. (2023), are also expected to contribute to deviations from the shear-driven J10 and HJ99-M81 parameterisations (McConnochie and Kerr, 2017, 2018).

Li 194: Did Davis & Nicholls (2019) explicitly state that the ice base of Larsen C was smooth? I would expect there to be scallops or ripples from the strong turbulence there.

Yes, Davis and Nicholls (2019) explicitly state that the underside is smooth. They obtain their drag coefficient from a velocity profile and state that "the ice shelf base at this location appears to be smooth over wide area." However they also predict based on the derived roughness length that "this suggests that the roughness elements (such as scallops) at the base of Larsen C ice shelf have a vertical extent of 1.3 cm."

Li 196: See Table 1 of Washam et al. (2023) for a summary of observed C_D beneath ice shelves, including what was observed in the Ross Ice Shelf crevasse

Note that Washam et al. (2023) is cited a few sentences down, with values from the Washam et al. (2023) Supplementary Material Table S1 used in Fig. 2. As requested, we have rewritten the sentence to include the Washam et al. (2023) Table 1 range, which encompasses the Lawrence et al. (2023) value.

L191-194: Most suggested values range from 0.0015 (Holland and Jenkins, 1999) to 0.0097 (Jenkins et al., 2010), with a value of 0.0022 estimated from turbulence measurements beneath the smooth underside of Larsen C Ice Shelf (Davis and Nicholls, 2019) and values between 0.0023 and 0.0068 estimated from basal ice morphology beneath the crevassed Ross Ice Shelf grounding zone (Lawrence et al., 2023; Washam et al., 2023).

Stratification Feedback on Turbulence – Insights from Large Eddy Simulations:

Li 221: Love the specification on Beta/alpha

Thanks.

Stratification Feedback Parameterisation Design:

Li 246: This is a small point, but does $\Gamma_{T,S}$ refer to the combined Gamma for the two-equation parameterisation or the individual Gammas for the three-equation parameterisation? I have not seen anything about a two-equation formulation at this point in the text.

They refer to individual gammas, we have rewritten them separately as "Γ_T and Γ_S" to make it clearer.

Li 260: Davis et al. (2023) and Schmidt et al. (2023) also published using a heat conductive flux term. I would check through all the values from the published papers before downplaying the importance of heat conduction.

We have added these references. Note that Schmidt et al. (2023)'s 12% best fit for ice heat conduction to ocean heat transport ratio is not dissimilar to the Holland and Jenkins (1999) value, and we have relaxed the strength of our statement by adding "order 10%". We want to emphasise that we are not saying that heat conduction doesn't matter, rather we are saying that its inclusion (which would be done through a somewhat simplified advection-diffusion model) would not qualitatively change the conclusions of our paper. To clarify this intent, we have revised some statements:

L255-260: Note that we also neglect the conductive heat flux term of Eqn. 2; although the conductive heat flux may be an important term in some ice shelf cavity conditions (Holland and Jenkins, 1999; Arzeno et al., 2014; Washam et al., 2020; Schmidt et al., 2023; Washam et al., 2023; Wiskandt and Jourdain, in review), melt rates are not expected to decrease by more than order 10% (Holland and Jenkins, 1999). Thus, we do not expect qualitatively different conclusions in the comparison of transfer coefficient parameterisations when we omit the conductive heat flux term.

Appendix A3, L821-822: We also neglect the heat capacity term with the ice conduction, as we have done with the shear-driven parameterisation (Section 2.3), as it is unlikely to qualitatively change the results when comparing melt parameterisations.

Li 281 – 284: This is not correct – In a stratified setting, $S_b < S_M$ and therefore $T_b > T_M$. So, by choosing a fully mixed setting in (9), you are artificially raising the thermal driving. This can be quite significant in stratified settings – take a look at the Washam et al. (2020) and Schmidt et al. (2023) estimates of T_b and S_b.

Thanks for pointing this out, this was a mistake made in revisions. As stated earlier in the paragraph, $T_M - T_{fr}(S_M)$ quantifies the maximum heat available for melting and is likely *larger* than the real ocean heat transport. We have fixed this and added the references too.

L281-284: Note this thermal driving may be larger than the actual temperature difference delivering heat from the ocean for melting that we computed using the three-equation parameterisation ($T_M - T_b$ in Eqn. 2), as observed in stratified conditions (Schmidt et al., 2023; Washam et al., 2023). However, this thermal driving definition is independent of transfer coefficient parameterisations and therefore more appropriate when comparing parameterisations.

Li 276 – 296: I realize that the authors attempted to clarify this paragraph to make it more digestible, but I am still having a hard time sifting through the unclear presentation of what has been done here. I realize that in the actual work, they are solving the 3 equations, but here it is a 2 equation formulation with no salt gradient through the boundary layer. Please rewrite this section of remove it, as it is quite confusing.

Here we are explaining the definition of the y-axis in Fig. 2 (and similar plots elsewhere). All melt rates are calculated with the three-equation parameterisation. Our parameter space explores both T_M and u_*, but we elected to plot the corresponding thermal driving T_* for each T_M as a more useful measure of heat above the freezing point. We could not use the true temperature above the freezing point because $T_M - T_{fr}(S_b)$ depends on S_b and therefore the transfer coefficient choice, which would not be a fair comparison across parameterisations on the same plot. We prefer to keep the plot as is, but have clarified what was done.

L275-284: We vary the far-field temperature T_M and friction velocity, u_*, and compute melt rates across this parameter space with the StratFeedback, ConstCoeff, J10 and HJ99-M81 transfer coefficients, assuming $S_M = 34.5$ psu and a pressure p_b of 500 dbar ($\sim$ 500 m depth). Rather than plot the far-field temperature on the y-axis, we instead plot the corresponding thermal driving,

$$T^* = T_M - T_{fr}(S_M) \ ,$$

which quantifies the maximum heat available for melting (where $T_{fr}(S) = \lambda_1 S + \lambda_2 + \lambda_3 p_b$ is the local freezing point as in Eqn. 1). Note this thermal driving may be larger than the actual temperature difference delivering heat from the ocean for melting that we computed using the three-equation parameterisation ($T_M - T_b$ in Eqn. 2), as observed in stratified conditions (Schmidt et al., 2023; Washam et al., 2023). However, this thermal driving definition is independent of transfer coefficient parameterisations and therefore more appropriate when comparing parameterisations.

Comparison to Observations:

Li 298 – 300: Consistent with the above comment, I do not understand if StratFeedback here is considering salt flux or a 2 equation formulation, as I interpreted the above section to state. This is so important for the reader to understand, since this paper is all about boundary layer stratification.

We use the same plotting convention as before, so solve the three equation parameterisation but plot the corresponding T^* given T_M and S_M. This is consistent with Rosevear et al. (2022b). We believe the rewritten paragraph (included in previous point response) clarifies this. We have also added "three-equation parameterisation" to this section and added an explicit link to the T^* definition in Eqn. 9 in the Figure caption.

L299-300: Following Rosevear et al. (2022a), we compare the melt rate produced by the StratFeedback and ConstCoeff three-equation melt parameterisations...

Fig3 caption: Thermal driving T^* (Eqn. 9) – friction velocity regime (b) updated from Rosevear et al. (2022b)

Li 298 – 321: This is a nice section that could be in the results after clarifying the above comment. Although, I understand that it is still motivation for the approach that will then be applied to the model.

We received a similar comment from the previous round of reviews, but feel that given the large amount of model experiments discussed in the Results, it is simpler to leave it here as a motivation.

Model Configurations

ISOMIP+ Setup and Modifications

Li 407: Can you please provide a sentence that defends your selection of the standard value for C_D.

We choose it to follow the ISOMIP+ protocol and enable easier cross-study comparison.

L410-412: In all ISOMIP+ simulations, the drag coefficient $C_d = 0.0025$ is used for the melt parameterisation and top and bottom boundary conditions for momentum, consistent with Asay-Davis et al. (2016) and Gwyther et al. (2020).

Pine Island Glacier Configuration:

Li 460: Perhaps it would be worthwhile to mention here that subglacial discharge could interact with the ice-ocean boundary layer in ways to alter the StratFeddback parameterization in locations of the ice shelf.

Added:

L465-466: However, subglacial discharge could modify the ice shelf–ocean boundary layer, thereby altering the effect of the StratFeedback parameterisation.

Results:

Realistic Pine Island Glacier Simulation:

Li 589: Please note the weakness of satellite-derived melt rates here. While they provide excellent coverage, they are only a first order estimate of what the true melt rate is (See Vankova & Nicholls, 2022 Fig. 8 for comparison with ApRES obs).

L596-698: To assess the parameterisation in a realistic situation where circulation is more complex and the results can be compared with observations, we use the MITgcm Pine Island Glacier setup of Nakayama et al. (2021) (model details in Section 3.2). We tune the drag coefficient to achieve melt rates similar to the Adusumilli et al. (2020) satellite melt rate product, though we acknowledge that satellite melt rates contain uncertainties and can differ from *in situ* ApRES measurements Vaňková and Nicholls (2022); Lindbäck et al. (2025).

Li 587 – 642: This section is much improved!

Thanks!

Discussion:

Li 669: Was Davis et al. (2023) in the diffusive convective regime? I think it is worth double checking and also taking a look at Davis et al. (2025): "Lateral Fluxes Drive Basal Melting Beneath Thwaites Eastern Ice Shelf, West Antarctica." This paper should be cited somewhere in the manuscript.

Davis et al. (2023) results are in the diffusive regime according to our definitions in Fig. 3, but in their paper they classify the observed conditions as being within the stratified turbulent regime.

We have removed the Davis et al. (2023) reference in the sentence in question and replaced it with a reference to Fig. 3.

L675-677: The StratFeedback parameterisation, though designed for the stratified regime, suppresses melt rates in the diffusive-convective regime and better matches observations and simulations in this regime (Fig. 3, Begeman et al., 2018), but is still an extrapolation in these low-velocity conditions.

Earlier, we also mention the difference between the Davis et al. (2023) classification and ours

L309-311: The original studies may also classify their ice shelf regimes differently, for example, Davis et al. (2023) categorise their observed Thwaites Ice Shelf conditions as stratified turbulence, whereas our definitions place it in the diffusive-convective regime (Fig. 3).

We have also added Davis et al. (2025).

L725-726: Davis et al. (2025) also recently demonstrated the importance of lateral processes beneath warm-cavity ice shelves, which ice shelf basal melt parameterisations do not include.

Figures:

There is a labeling convention switch for velocity units from m/s to ms^-1 in the figures. Please change them to be consistent throughout. Also, make m/yr or m yr-1 consistent throughout the manuscript. I suggest to make it consistent with the notation from the text.

We have made the units consistently $\mathrm{m\,s^{-1}}$ for velocity and m/yr for melt rate throughout the manuscript.

Fig. 1d: Totally optional, but it might be worthwhile to plot the Washam et al. (2023) means from Table 1 in here as grey lines, as well.

We have added these lines.

Fig. 5: there is a space missing in the m $s^{-}1$ labels on this figure

Fixed.

Fig. 6: there is a space missing in the m $s^{-}1$ labels on this figure

Fixed.

Fig. 7: there is a space missing in the m $s^{-}1$ labels on this figure

Fixed.

References:

Davis et al. (2025): "Lateral Fluxes Drive Basal Melting Beneath Thwaites Eastern Ice Shelf, West Antarctica."

Reviewer 2

Summary

Thank you for your thorough response to my review. Overall, I think the changes you made help the clarity of the manuscript, I just noted below a few places where the changes you made seem to me to make the manuscript less clear. I consider all of my suggested changes minor, and I do not have a need to re-review.

We thank Carolyn Begeman for your feedback. We appreciate your thorough reading of our manuscript.

Major comments:

L107: "It is important to highlight..." This paragraph feels like too large a detour from the flow of the introduction. I know you added it on request from reviewers. I think just deleting the sentence beginning "Therefore, although" could help the flow, and the content of that sentence doesn't seem crucial to me.

We have deleted the "therefore, although" sentence but kept the rest of the paragraph to satisfy feedback from other reviewers.

L693: "despite the different drag coefficients" I would delete this phrase. Since the drag coefficient is spatially uniform, I wouldn't expect it to lead to a different anomaly pattern.

Deleted as suggested.

L697: "align better with observational products near the grounding line" which predict melt rates near the grounding line of what magnitude? Is the maximum melt rate simulated by HJ99 less than the maximum observed value? I really like Figure D1. I think you may want to add a sentence to the main text indicating that the upper tail of the distribution is increased for your StratFeedback parameterization, bringing it in closer agreement with Zinck et al.

L633-637: Both the tuned StratFeedback and StratFeedback+MK18 experiments have a larger area of the ice shelf with melting greater than 50 m/yr compared with the tuned HJ99-neutral simulation (Fig. D1), and align better with the order 100 m/yr melt rates seen in high-resolution observational products near the grounding line (Zinck et al., in review; Shean et al., 2019). This improvement is demonstrated by the upper tail of the melt rate statistical distribution (Fig. D1) increasing in area with the StratFeedback parameterisation, bringing the distribution closer to the Zinck et al. (in review) product.

L699: “less aggressive tuning” Since only one coefficient is tuned in both and both are in the observational range, I would say the tuning is not more aggressive in one versus the other. I would say the same for L775 “required less drag coefficient tuning” I don’t think there’s anything special about the HJ99 value that means a parameterization that is closer to that value is better than one that is farther from it.

We have removed these sentences.

L748: “the strat feedback parameterization affected melt rates...” I found this sentence confusing. When I look at Figure 6, it seems that the values for the cold, most energetic cavity are in the range observed in Figure 3’s WGZ. I think it could be clearer to talk about what regime space you could not access even with a wide range of parameter combinations.

Thank you for your suggested reframing of this statement. Indeed, the tide experiments can access the parameter space of several of the ice shelf borehole observations. We have rewritten this sentence as

L669-673: However, even with explicit tides, our ISOMIP+ experiments could not achieve the thermal driving and friction velocity conditions observed at George VI and Pine Island Glacier ice shelves, nor Stewart (2018)’s summer Ross Ice Shelf observations (compare Figs. 3 and 6). This result suggests that idealised ocean models should be used with caution when assessing melt parameterisations or other ice shelf boundary layer physics, or indeed other aspects of ice shelf cavity circulation.

L784: “may not simulate true ice shelf melt regimes” this seems overly general to me.

Removed this statement. The sentence now reads

L696-699: Ocean models, particularly coarse-resolution models, may lack the small-scale flow variability observed at high frequencies beneath ice shelves, either through not resolving these scales of motion (through both horizontal and vertical resolution), not simulating tidal motion, or having anomalously smooth bathymetry and ice base shape.

L795: “exercise caution around the simulated velocities” Unclear what this means. Can you put it in practical terms like what kind of inferences we would make? I don’t think you’ve demonstrated that u* or L+ in your realistic simulation is clearly biased.

We have removed this part of the sentence.

L809: “are extremely sensitive to these ... parameters” Have you shown this? I thought that you only tested one prescribed tidal velocity and one minimum friction velocity? Or do you mean in comparison with one another?

We did some preliminary tests varying the strength of these parameters. However, since we are not showing them, we have rewritten as

L720: However, both melting and circulation are likely to be sensitive to these unconstrained parameters.

Technical comments:

L46: Do you mean “Multiple physical processes in the ice shelf–ocean boundary layer contribute to melting beneath ice shelves”?

Yes, rewritten as suggested.

L305: I think there is a duplicated “is”

Addressed.

L334: “might” >> “may”

Done.

L395: To me, it is unclear which entry in Table 2 corresponds to “a fixed transfer coefficient parameterisation choice”

We have rewritten this sentence to clarify our intent, which was to signpost what is coming (comparison of low-velocity limits with each of the ConstCoeff and StratFeedback parameterisation). We also updated Table 2 to clarify what ConstCoeff is.

L381-382: In this study, we assess the sensitivity to the choice of low-velocity limits with the transfer coefficient parameterisation choices, ConstCoeff and StratFeedback.

L645: “masked tuning melt rate” >> “the masked area over which melt rates were tuned”?

Done.

L649 and 655: “tuning drag coefficient” >> “tuned drag coefficient”

Done.

Additional modifications to manuscript

In addition to the modifications made in response to the reviewers, during the review process we identified two errors that we have since addressed.

1. Firstly, Figure 7 has been updated to reflect the correct averaging time period (last 180 days of simulations), with minor changes to the melt rates and stated percentages.

2. Secondly, we have made the borehole observation temperatures and salinities consistent. The original studies quote their temperature and salinity using a variety of thermodynamic quantities. We choose to use conservative temperature and absolute salinity to match the most recent studies Begeman et al. (2018), Rosevear et al. (2022a) and Davis et al. (2023), and therefore convert potential temperatures to conservative temperature and practical salinity to absolute salinity. This is a choice made for consistency and so that quoted units are correct. Fig 3 has minor changes as a result, as do the numbers in Table B1, and extra information is provided in Appendix B to explain how thermodynamic quantities were converted. In the main text in Sect 2.4 we have added

L304-309: Note that the studies that originally presented this data may have used slightly different melt parameterisations in their comparisons (e.g. Jenkins et al., 2010; Davis and Nicholls, 2019, where different drag coefficients and transfer coefficients were used) and recall we ignore heat conduction into the ice. Additionally, the studies may use different thermodynamic variables – here we use conservative temperature and absolute salinity with conversions performed using the Gibbs Seawater Oceanographic Toolbox (McDougall and Barker, 2011, Appendix B).

In Appendix B we added

L853-858: Where data was not reported as conservative temperature or absolute salinity, we have converted the values using the Gibbs Seawater Oceanographic Toolbox (McDougall and Barker, 2011). This choice was made for consistency to match the most recent studies presented (Begeman et al., 2018; Rosevear et al., 2022a; Davis et al., 2023), noting the variety and evolution in thermodynamic variables used previously. However, the choice does not significantly impact results. The thermodynamic variables used in the observational comparison differ from those used in the models, potential temperature and practical salinity, but we use the same linear freezing point equation of state coefficients throughout the study (Table 1).

3. We have also edited the text, rearranging some sentences for improved structure and readability as requested by Reviewer 1. These changes are mostly in the Introduction. There were also a few typos addressed throughout the manuscript, and some recent, relevant references added: Guo and Yang (2025), Lindbäck et al. (2025), Couston et al. (2021) and Yung et al. (in review). Please see the tracked changes for further details.

4. Shortly after resubmission of the second round of review on 20 July 2025, an error was found in two of the simulation experiments. The Pine Island Glacier MITgcm simulations with StratFeedback+MK18 parameterisations originally used a set of old parameters in the melt parameterisation. Specifically, these two experiments were run with slightly different constants A_T, n_T, A_S and n_S (see Table 1 in the manuscript) than what is quoted in the manuscript and used in other experiments. All other experiments were correct.

We re-ran the experiments and there were only very minor changes to the plots as a result (barely noticeable in the plots, and small changes to total melt rates on the order 0.1%). The plots and open model code/data on zenodo have been updated to reflect the correct simulations.

References

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