Reply to reviewer 2

General Comments:

The paper compares a coupled model setup (involving an ice sheet model coupled to a 2D single layer ocean model) to more commonly used parameterisations for calculating ocean-induced melt under a floating ice shelf. Both qualitative and quantitative differences are shown to be significant. The significance of the western boundary current in particular is highlighted, and its impact on buttressing through enhanced thinning of shear margins. This is an important, though not surprising, result, and advances the science of ice sheet – ocean interactions. The overall layout and the clarity of the text is excellent. I recommend the manuscript to be published with minor modifications.

Thank you for the positive feedback and for the constructive comments on how to further improve this manuscript. We agree with most of the comments and will implement them in the manuscript. Below, we provide a point-by-point response.

What I most missed in this study was a comparison with a 3D ocean model. Although coupling IMAU-ICE to a 3D ocean circulation model might be beyond the scope of this study, circulation patterns from ISOMIP+ and MISOMIP1 ocean simulations should be obtainable through those projects. It is clear that LADDIE had a stronger physical justification than the other parameterisations presented here, but does it's 2D pattern look similar to 3D ocean models for the same domain? I'm not going to insist on this for the current paper, but I do hope the authors will find an opportunity to compare LADDIE against some of the ISOMIP+/MISOMIP1 models. It is worth at least pointing out that the LADDIE description paper includes a comparison against mitgcm for a real world ice shelf.

Indeed, coupling IMAU-ICE to a 3D ocean model lies beyond the scope of this study. While we emphasise that the coupling to LADDIE is not meant to replace coupled ice-ocean models (l.520), we agree that such a comparison would be valuable and is also in line with the comparison presented in Lambert et al. (2023). Therefore, we will include a paragraph in the discussion section describing how LADDIE compares to the results presented in the references at the bottom of this document (ISOMIP+/MISOMIP1 studies).

Given that a significant component of the ice sheet retreat shown in this study is attributable to marine ice sheet instability, I think this needs at least a paragraph (or at least a sentence) at some point. Presumable the imposed melt triggers MISI, and the ensuing retreat and VAF loss is due to a combination of melt-induced thinning and MISI.

We will add a paragraph on MISI in section 3.2.3 to highlight its role in the initial phase of retreat. Specifically, we agree that the imposed melt likely triggers MISI, which contributes to the early grounding line retreat before it crosses the bedrock depression. In most simulations, this crossing occurs within 200 years and generally coincides with the peak rates of grounding line retreat.

Are there real world ice shelve cavities where a neutrally buoyant layer separates from the underside of the shelf at some depth? Presumably LADDIE would be valid for such cavities?

In LADDIE, the separation of the neutrally buoyant layer (for instance at Filchner Ronne) is represented through gradual detrainment, allowing a substantial portion of the meltwater to detach from the upper layer while maintaining a minimum upper layer thickness across the domain. We will clarify this treatment of detrainment in section 2.1.2 (LADDIE description).

The "collapse" of a part of the ice shelf on the west side needs more attention, both in terms of describing exactly how this is handled in LADDIE and whether this implementation is a physically realistic. More specifically, what are the implications for momentum and heat loss and is this what would happen in the real world?

We will elaborate on the implications for momentum and heat loss in the methods section as you suggest in one of the line-by-line comments below. Find a more detailed response in how we plan to do that in our response to that comment.

There's a lack of explanations of what the abbreviations actually stand for. E.g. LADDIE, PICO etc. Do these stand for something? I'm sure LADDIE does, not sure about PICO? And IMAU-ICE? Please mention the full names when you first introduce the abbreviations. [Edit: I see the LADDIE full name appears on line 113; please move it earlier]

We will move the LADDIE full name up to the introduction and we will add the full name of PICO. We will also add the full name for ISMIP7 at the end of the discussion.

The authors choose a power law sliding equation and quote a reference indicating that choice of sliding equation doesn't have a large impact. I haven't read this paper, and I am happy to accept that it is correct in the context of the referenced paper, but it is very clear that, in general, choice of sliding equation can have a huge impact on simulated marine ice sheet behaviour.

We agree that the current phrasing may give the impression that the choice of sliding law has little influence on ice dynamics in general, which is not what we intended to communicate.

We will rephrase this paragraph to something similar to:

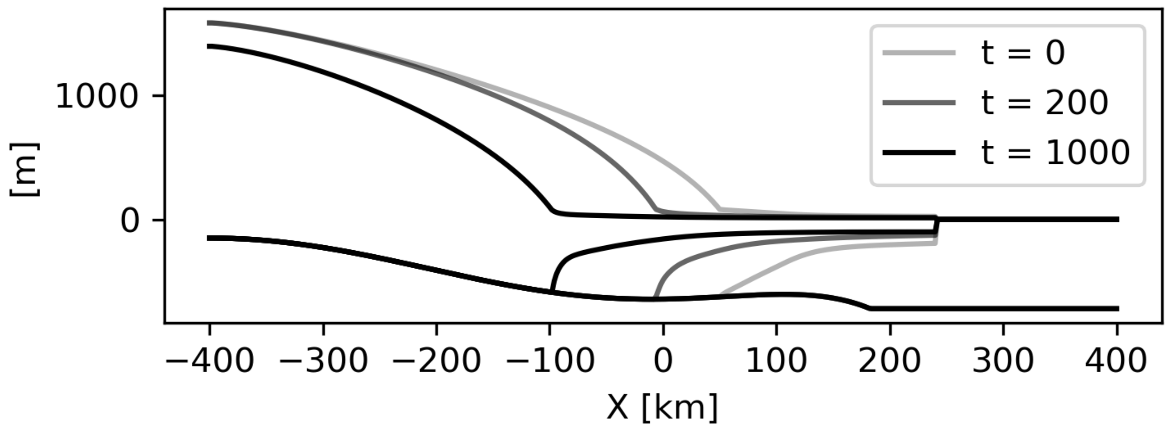
"While the choice of sliding law has been shown to influence model results (Cornford et al., 2020), Berends et al. (2023), using the same ice sheet model and idealised domain as in our study, demonstrated that its impact is much smaller compared to that of the sub-shelf melt implementation. Given the focus on the latter, we conduct our experiments using a single sliding law: the Coulomb-limited modified power-law relation introduced by Schoof (2005). This choice is motivated

by Joughin et al. (2019) which shows that Coulomb-limited laws best capture the dynamics at Pine Island Glacier."

Related to this, Figure 4i seems to have a very steep gradient in the ice shelf close to the grounding line. Grounded ice geometry is not shown, but I presume that a very steep thickness gradient exists across the grounding line? A sliding equation that represents the dependency of basal resistance on effective pressure, which must decrease toward zero in the vicinity of the grounding line, is unlikely to exhibit this kind of feature. I would like to see at least one plot showing ice thickness, or ice upper surface height, also for grounded ice upstream of the grounding line. This could also be in supporting material or an appendix, and doesn't need to be shown for all simulations. Just something to indicate the shape of the ice approaching the grounding line. I'd also like to see some comment on the potential of different sliding equations to impact on the results.

Indeed, the surface gradient close to the grounding line is very steep in Fig. 4i, also shown by the geometry cross-sections for the same time slices as in Fig. 4 (see below). We will include this figure in the supplementary material.

We acknowledge that there may be an interaction between the melt pattern and the choice of sliding law, and we will add a paragraph to the discussion to reflect on this potential influence, also reflecting on the figure below.



Line by line comments:

13. "first period" is not meaningful to the reader at this point. Are we talking about a few decades here? Please make this a bit clearer to the reader who has not yet read the experiment design.

Agreed, we will replace "first period" by "300 years".

15. Suggest "parameterisation" -> "simpler parametrisation" or "more common parameterisation" because to my mind the 2D LADDIE could be viewed as a

parameterisation (albeit a more sophisticated one) so it wasn't obvious to me that LADDIE is not being discussed here.

Although the application of LADDIE is, in this study, comparable to the application of the parameterisations, the fundamental nature of LADDIE is different. It is a regional 2D numerical model which integrates a set of conservation equations in time. We believe the classification of parameterisation does not do justice to this complexity, and hence prefer to retain the distinction between parameterisations and LADDIE.

85-86. I'm missing a reference to a description of the SIA/SSA model as implemented here. The Berends paper describes the DIVA implementation in IMAU-ICE and states that there is also a hybrid SSA/SIA model in IMAU-ICE, but doesn't describe it. Please include a reference to a paper that describes the SSA/SIA hybrid as it is implemented here. Perhaps a Beuler PISM paper?

Thanks for pointing this out. The SIA/SSA model is implemented in IMAU-ICE following the same method as Beuler & Brown 2009 indeed, so we will add the reference there.

155. How have you determined that 4 days is enough to spin up the 2D ocean from rest? This seems like a very short time to me!

A full spin up from rest is only performed during the initial spin up phase of LADDIE (step 2 in Fig. 1b), where LADDIE requires roughly 40 days to equilibrate. This time scale equals approximately twice the flushing time of the meltwater layer. This meltwater layer flushing time is considerably shorter than the flushing time of the entire cavity due to the smaller volume of the meltwater layer and the higher velocities within it. We will add this to the LADDIE spin up paragraph (l.175-176).

In the coupling phase (step 4 in Fig. 1b), LADDIE is not spun up from rest; instead, it is restarted from the equilibrium state obtained in the previous coupling step. Since the forcing remains constant, and the changes in geometry between coupling intervals are small due to the short coupling time step, the model only requires 4 days to adjust and to reach near-stable conditions. We will clarify this in the manuscript.

Fig. 2. Would be nice to also see the spun up LADDIE state.

The spun up state of LADDIE is identical to the state at t = 0, and therefore already shown in both Fig. 4 (melt rates and velocity) and Fig. 5 (only melt rates). To avoid repetition and to focus on the initial ice sheet geometry, we prefer to leave the spun up state of LADDIE out of Fig. 2.

236-243. I didn't understand this first time through. I think you're saying that in general the central flow is being held back by lateral transmission of stress across the shear margins, so if this transmission was removed the central part would flow faster and the narrow strip along each side would flow slower. So the "collapse" (thinning to zero) of ice in the western shear margin allows the central flow to increase, and the strip along the side to slow down. Is that right?

Indeed, the central flow is affected by lateral transmission of stress across the margins. For the western side, this transmission is mostly removed, hence the central flow can speed up compared to the eastern side.
Additionally, because of the slow-down of the grounded strip, the east-west oriented grounding line at the very edge of the domain (which we referred to as WM grounding line) cannot retreat further. This contrasts with the EM grounding line, where the decoupling of the velocity field is not apparent.

To avoid confusion, we will explicitly clarify the role of lateral stress transmission across the shear margins in relation to the dynamical detachment of the velocity field.

I think I was initially confused by the labelling, direction and interpretation of “margin”. Given that “margin” could plausibly refer to the margin of the domain itself, and that “WM” and “EM” are horizontal in the figure, I was confused between the short section of east-west grounding line at the very edges of the domain and the approximately north-south sections of grounding line close to the shear margins within the domain. I was thinking of the former when I think you were referring to the latter. Can you align the text with the grounding line that you intend to refer to? And can you clarify exactly what you mean by “western margin” and “WM grounding line”? I guess “margin” means either “shear margin” or “cavity margin” and not “domain margin”. You could also edit the text in paragraph 223-228. This is also relevant for correctly understanding your description at line 340.

Regarding the terminology, we understand the confusion around the use of the word “margin” and how it may be interpreted either as the domain boundary or as a shear margin. In our manuscript, by “margin” we specifically mean “shear margin”, not the outer boundary of the model domain.

The “WM” and “EM” grounding lines refer to the east-west oriented grounding lines located at the edges of the domain. These are the features we intended to emphasise, so your initial interpretation was correct. We recognise, however, that the dynamical detachment also occurs along the north-south oriented section of the western shear margin.

We agree that the current definitions are unclear and may contribute to misinterpretation. In the revised manuscript, we will address this by:

- Renaming (‘WM’ and ‘EM’ → **‘GL_W’** and **‘GL_E’**) and clearly introducing the east-west oriented grounding lines in both the text and the figure
- Refering to the **‘western and eastern shear margins’** when we discuss the north-south oriented shear margins.

Fig 4. It is very hard to see the Layer velocity arrows. The speed comes across fine, but, except for the top plot, it is hard to interpret the direction of flow. The flow pattern does not look complex, so perhaps it would be ok to have fewer arrows and scale them up a bit?

We will remove some arrows and scale them up to better visualise the flow direction.

Is there a reason to stop at 300 with the ice draft contours? Come to think of it, is there a reason to start at 100? Also, I find it a bit confusing that you have both a full colour plot and contours, each using a different colorscale, for what appears to be the same quantity (I think these are both for the ice draft if I understood right?)

Yes, both the full color plot and contours represent ice shelf draft. We use the explicit contours to better visualise the asymmetry. We chose to stop at 300 m because it marks the boundary of our tuning area. The current range provides a clear overview without overwhelming the plot with too many lines.

We agree that the different colorscales can be confusing, so we will consider a non-linear colorscheme which highlights this range better for the revised manuscript.

I think you can simply say "draft" instead of "draft depth".

We will replace 'draft depth' by 'draft' throughout the text and in the figures.

There is dark grey shown on the left side of the panels in the second row. I'm not quite sure what this is – perhaps am ice – ocean mask discrepancy? Can you clarify this in the caption please? [Edit: having read further I think this might be when the ice shelf vanishes completely; i.e. zero draft? The meaning of "the ocean is masked by dark grey" is not completely obvious because LADDIE is of course representing some aspect of the ocean]

Indeed, these dark grey patches on the left side indicate locations of melt-through (where the ice shelf completely vanishes). We will, also considering the last part of the above comment, rephrase to the following:

"In all panels, the **ice-free** ocean is masked by dark grey. **Dark grey pixels enclosed by ice shelf pixels indicate areas of melt-through.**"

The ice flow looks pretty stagnant in 4l through much of the shelf. Maybe a log scale would show the ice flow better?

We prefer to use a linear scale for the ice velocity plots, as it more clearly shows the differences between the different sub-shelf melt options and time slices. The ice flow in Fig. 4l is indeed quite stagnant, and this is shown to emphasise the low velocities at the end of the simulations. In the revised manuscript (l.260), we will explicitly state the contrast between these stagnant velocities at the end of the simulation and the previous time slices shown.

244. Can you be clearer what you mean by "collapse"? It looks like a very thin ice shelf, but I don't know if it actually goes to zero?

These are areas where the applied melt rate exceeds the ice shelf thickness divided by the ice sheet model time step, effectively removing all ice and resulting in ice-free conditions (i.e., zero ice thickness). We now realise that 'collapse' is a confusing term here. Therefore, we will change 'collapse' to 'melt-through' throughout the text, except for two occasions where we refer to possible break-up of the ice shelf not solely caused by melt (l. 36, l. 534).

246. I think "the buoyant plume exits the cavity" and "assumption in LADDIE" is not quite sufficient explanation here. I suggest you add a paragraph in the model description describing boundary conditions in LADDIE and how vanishing ice shelf is handled. I guess you impose zero normal velocity in LADDIE at the lateral (east and west) boundaries? Then what exactly do you do when ice shelf vanishes? I haven't read the LADDIE paper and I don't know what it's state variable(s) are (I guess T, S, and velocity?). Can you write down the boundary condition equations used at the transition to a vanishing shelf? Or is it not handled as a boundary as such, in which case, what exactly happens?

The areas of melt-through are treated identically to ice-free cells at the calving front. At these transitions from ice-covered to ice-free cells, we prescribe zero gradients in the state variables temperature (T), salinity (S), layer thickness (D), and horizontal velocity components (U, V).

In the momentum equations, the horizontal pressure gradient force assumes zero draft for open ocean cells. This drives a strong outflow of the meltwater across the calving front. Any momentum, heat, or salt advected beyond this boundary is lost to what is treated as an infinite open ocean. Hence, momentum, heat, and salt cannot be transferred across an ice-free cell — this is what we meant when referring to the plume "exiting the cavity."

We agree that this treatment deserves a clearer explanation in the manuscript. As you suggest, we will add a dedicated paragraph in section 2.1.2 (LADDIE description). We will also refer to this when discussing the results.

247. Well presumably sea ice would form in the real world, limiting heat loss? The Southern shear margin in the PIG ice shelf might be pretty similar to this situation?

Indeed, in reality, sea ice formation could limit heat loss to the atmosphere and potentially influence the meltwater layer dynamics. We will add a reflection on this in the discussion section, linking it to LADDIE's treatment of ice-free areas (see our response above). This treatment may be more limiting for isolated melt-through holes than for open ocean conditions at the calving front, and we will clarify this distinction accordingly.

Fig. 7. "For the control experiment, only the VAF loss rates are shown" What does this mean? It looks to me like you show VAF and GL retreat for the control also…

Thanks for pointing this out, this was left from an older version of the figure. We will remove this part from the figure caption.

456-457. What does “schematic character” mean? Do you mean the idealised MISOMIP domain?

Here, ‘schematic character’ indeed points to the idealised domain (or geometry) as well as to the idealised ocean forcing. We will clarify this in the manuscript and replace:

“Hence, we believe that despite the schematic character of our simulations these observations of Crosson ice shelf can likely be explained by the 2D horizontal meltwater flow.”

by:

“**Although our simulations are schematic in terms of geometry and forcing, we see qualitatively similar behaviour. Hence, we believe that these observations of Crosson ice shelf can likely be explained by the 2D horizontal meltwater flow.**”

545. “advanced” doesn’t have a specific meaning here. How about “physically motivated” instead?

We agree, we will follow your suggestion and change it to ‘motivated’ instead.

553. Suggest “Parameterisations” -> “Commonly used parameterisations”.

We will follow your suggestion to emphasise that Quadratic, PICO and Plume are among the commonly used sub-shelf melt parameterisations.

REFERENCES

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