Response to reviewer comments for the manuscript *Results of the second Ice Shelf – Ocean Model Intercomparison Project (ISOMIP+)*

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We thank the three reviewers (in two reviewer teams) for their time and effort reviewing our manuscript and for their thoughtful and supportive feedback and useful suggestions that helped us to improve it.

Below we respond to each comment in turn, with our responses indicated in blue, with changes that we have made to the manuscript in response to the reviewers' suggestions. Line numbers refer to the revised manuscript.

We have also revised the figures to make them more accessible regarding colour schemes, as requested by the Editorial Office. We have also rewritten the short summary section as requested.

Manuscript Validation

1) Please adjust the short summary section and provide at least one written-out version of "ISOMIP+". Please consider the 500-characters limitation (including spaces) for the short summary section.

We have rewritten the short summary section as follows:

> The second Ice Shelf-Ocean Model Intercomparison Project, ISOMIP+, compares 12 ice shelf-ocean models with a common, idealised, static configuration, aiming to assess inter-model variability. Models show similar basal melt rate patterns, ocean profiles and circulation but differ in ice-ocean boundary layer properties. Ice-ocean boundary layer representation is a key area for future work, as are realistic-domain ice sheet-ocean model intercomparisons.

(454 characters)

2) Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator and revise the colour schemes accordingly. Figs. 6, 11, 12, 15, 16, S1

We have modified these figures to add different linestyles. We would like to keep the colour scheme the same as it is one of the best schemes we have found for normal vision, and it is difficult to find an accessible colour scheme for 12 individual colours, but we have added linestyles to ensure the lines can be distinguished with a colour blindness simulator. See below for examples of revised Fig. 16 and Fig. 15.

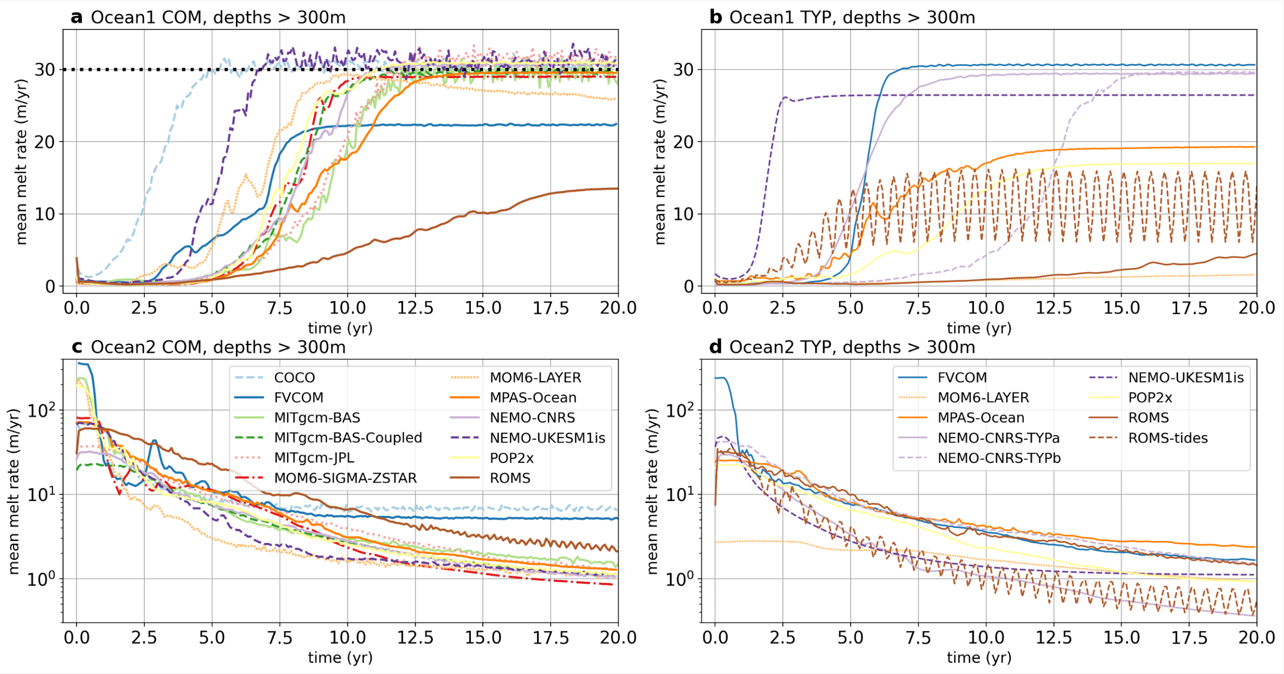

Figure R1: Revised Fig. 16

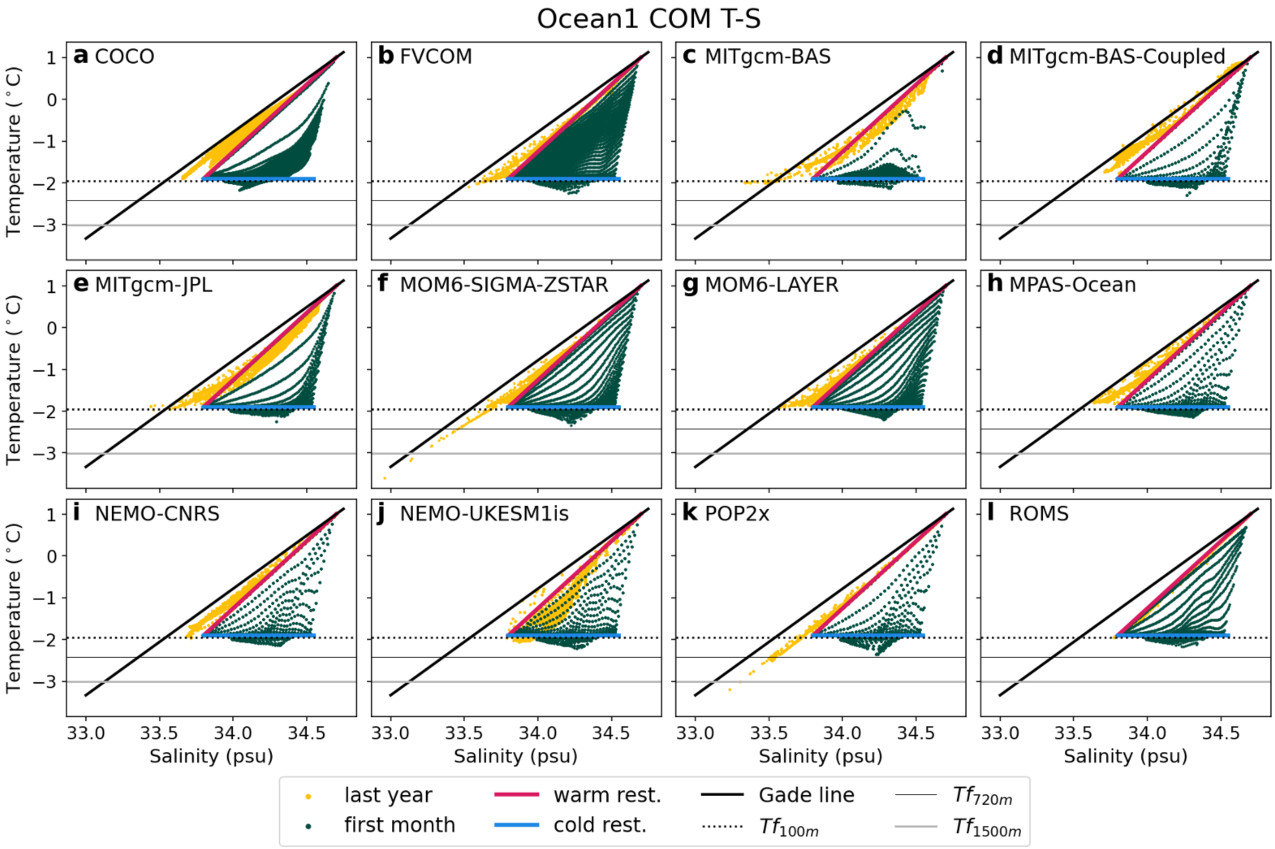

Figure R2: Revised Fig. 15

Reviewer 1

This paper presents the results from the ISOMIP+ Project, in which a number of ice-shelf-enabled ocean models were compared by running a series of carefully defined experiments. The details of the experiments have been published elsewhere, and the experiments themselves appear to have been run a while ago, which makes this a slightly unusual review, which can only really cover presentation and not substance.

Many of the model codes have changed a little since the experiments were run, but those updated codes are used frequently for standalone ocean and coupled ice-sheet-ocean experiments, as were the earlier versions, and the results pervade the literature. This work represents the only direct inter-comparison of those models and is thus incredibly valuable, even if the configurations are now slightly historical. I commend the authors for pursuing the task of analysing and publishing the results.

Thank you for your thoughtful review of our paper and for your support.

The presentation is generally clear and logically structured, and I while I do have comments on substance, I'm not expecting those to be acted on, in terms of redoing analyses, prior to publication. So, most of my comments should be seen as more philosophical in nature, concerning the broader approach adopted in the work and how it might be built on in future. Normally I dislike papers talking about what should or will be done in future, feeling that it is generally better to focus on what has been done and what has been learnt from that. However, in this case, the lessons learnt about how to compare models are probably as important (if not more so) than what can actually be learnt about (the historical versions of) the models themselves from this inter-comparison.

It seems to me that the results of the ISOMIP+, or similar, exercises are of potential value in (at least) two ways: to document differences between models and thus provide insight into how the various models behave in separate applications (both realistic and idealised); and to explain model differences with the ultimate goal of adapting codes towards gaining improved (or at least more consistent) results. Lines 90-91 hint at the latter aim, but the paper really addresses the former, and perhaps that point could be clarified. Arguably, documentation of the differences is all that can be done at this stage. Without knowledge of a "correct" solution, more consistency around a potentially incorrect solution might even be a bad thing. Nevertheless, the community should (in my opinion) be striving towards improved models rather than simply accepting that each implementation will be different and documenting those differences. So, I think the ongoing inter-comparison efforts should keep that other goal in their sights.

Which brings me to my main point that there is maybe something missing from the paper. The ISOMIP activity is clearly continuing, and there is much that can be learnt from the successes and problems encountered in this activity. It would be valuable to briefly discuss those and how they might shape future activities.

Thank you for your comments. We agree that ongoing model development with the aim of improving model agreement is important, and ideally all models would converge towards a "true" solution. Unfortunately, limitations in the number of experiments in our intercomparison mean we are generally unable to probe the "why" of observed model differences, nor provide a concrete direction for model development due to its idealised nature. We agree that it is important to highlight what might be done in the future to move forward in validating ice-shelf ocean models and achieving better model agreement, which may involve the next generation of ISOMIP/ISOMIP+ experiments. We have added suggestions for further work along the themes of ISOMIP in the Discussion (see last major comment response). We have also clarified the intent of the paper in the Introduction by rewriting L105 onwards:

> L105 - 109: While we verify the internal consistency of the basal melting parameterisation and water mass conservation, we cannot validate the idealised simulations with observations, and no analytical solutions have been found. Instead, we present and compare the model results as a diagnostic benchmarking exercise, and aim to understand the causes of their similarities and differences where possible. We identify key aspects of model variability for which future work can focus on as we progress towards improved ice shelf–ocean model fidelity.

For example, there was never a formal comparison of models run on the original ISOMIP configuration, but where results have been presented in different papers, they appear to be far more consistent than the ones presented here. Why is that? Does the simpler geometry minimise the differences that arise from discretisation of the topography (see later comments)? Or was it that modellers were freer to tweak other model settings to match a range of outputs from the original 1997 paper, rather than only alter transfer coefficients to match a prescribed melt rate? Wouldn't the answers to those questions be worthwhile exploring, with that second goal in mind?

We agree that where ISOMIP comparisons have been published, results are more similar than for ISOMIP+ models, particularly for the Experiment 1.01 models. Firstly, note that parameters used in these models are actually quite similar, all following the Hunter (2006) Experiment 1.01 protocol (Grosfeld et al., 1997; Losch, 2008; Mathiot et al., 2017, though the latter uses a different equation of state), and all use the same constant transfer velocity in the melt parameterisation. Note that ROMS and Oz-POM model tests shown in Galton-Fenzi (2009), Gwyther et al. (2015) and Gwyther et al. (2016) use the different ISOMIP Experiment 2 geometry, and also differ from Experiment 2 in Grosfeld et al. (1997) by the removal of an open ocean wind forcing, making it slightly more difficult to perform an inter-model comparison with the available published results. We have added a line in the introduction after the ISOMIP studies are mentioned to highlight the similarity:

> L43 - 44: Many of these ISOMIP studies also show good model agreement with the Grosfeld et al. (1997) benchmark, providing confidence in the numerics and physics of ice shelf–ocean models, at least in an idealised setting.

ISOMIP was a critical first step in ice-shelf ocean model development, by building community, and through its experiments, demonstrating the robustness of ice shelf-ocean models. However, model agreement in ISOMIP, which creates a more trusted "benchmark" (i.e. the Grosfeld et al. (1997) results), does not necessarily imply the experiment is more useful for assessing model differences if our aim is to achieve model agreement in realistic settings (and agreement with observations).

As you point out, the simplified geometry of ISOMIP likely explains much of the similarity between published ISOMIP Experiment 1.01 models. The uniform ice faces in ISOMIP, with a large, cold cavity and vertical side walls, differs greatly from real cavities but relies on fewer model choices than ISOMIP+. ISOMIP+, on the other hand, includes a grounding line, nonuniform topography and an ice front that allow the topographic control of circulation to be explored. The forcing of Circumpolar Deep Water tests transient adjustment, and the warmer cavity allows for a more realistic (though still highly parameterised and idealised) boundary layer representation, which reveals differences in the boundary layer and its parameterisation. In particular, the use of a constant transfer velocity in the melt parameterisation in ISOMIP also removes a degree of freedom. ISOMIP+ exposes more model differences, an inevitable consequence of increased model complexity, and reveals some of the parameters that will really matter for model diversity in more realistic models – e.g. vertical coordinates and associated choices. There is hence a tradeoff between including processes that occur in reality and having a clean test case with fewer degrees of freedom.

Perhaps these different experimental designs serve different purposes: ISOMIP is invaluable for verifying model numerics and comparing with an established solution. However, when approaching a benchmarking process that informs priorities in realistic model development, adding elements of realistic physics is important, which motivated the ISOMIP+ work, and will likely motivate adding further complexity in the future. We have modified the Introduction to discuss this complexity and the expected increased model spread:

> L58 - 65: The ISOMIP+ protocol specifies experiments testing the response to both warm and cold ice shelf cavity conditions and the transition between them. ISOMIP+ also builds on the first generation of ISOMIP by using a higher spatial resolution (from ∼3-9 km horizontal resolution and at least 10 vertical layers to ∼2 km and 36 vertical layers Hunter, 2006; Asay-Davis et al., 2016), a velocity-dependent basal melt parameterisation (Holland and Jenkins, 1999; Jenkins et al., 2010), and a more complex bottom topography and ice draft that introduces aspects of reality, but is still constrained by the topography requirements for the ice sheet models in the parallel MISMIP+ and MISOMIP1 experiments. The added complexity of ISOMIP+ compared with ISOMIP likely amplifies the effect of model choices and therefore increases model spread, but also exposes what model choices may be important in realistic ice shelf–ocean simulations.

As it stands, the paper seems to suggest that the ISOMIP and ISOMIP+ configurations are now finished with and the way ahead lies in increasing the complexity still further with an intercomparison of realistic domains. But isn't that likely to throw up even more model diversity and put that second goal even further out of reach? I guess the argument is that with a realistic domain you have a "known answer", but is that true? You have a known answer in terms of the melt rate, but you have no idea of what the complex circulation should look like and only a patchy knowledge of the seabed geometry that has a major influence on it and hence the melt rate. It's not clear to me that tuning certain parameters to improve the match with observation when such vital information is so lacking really helps in improving the models.

Thank you for your comment, which highlighted that we had not included suggestions for future research that encompass idealised experiments. Intercomparisons between models in realistic domains are indeed likely to have significant model diversity, due to their complexity, and untangling cause and effect to facilitate model convergence will become much more difficult. Both the RISE (Galton-Fenzi et al., 2025) and MISOMIP2 (De Rydt et al., 2024) experiments are more flexible in modelling parameters, which likely decreases the burden on model developers and enables increased participation, as well as avoiding pushing models out of their typical regime with strict parameters, but makes untangling differences difficult. These realistic intercomparison experiments serve different aims to that of the idealised experiments, in that they capture the current state of ice shelf–ocean modelling and potentially inform priorities for future process and parameter studies (which may involve idealised models).

Whilst it is true that *in situ* observations of Antarctic ice shelf cavity ocean conditions and circulation are lacking, this is unlikely to dramatically shift in the coming years, and is therefore a fundamental limitation of ice shelf-ocean modelling. This shouldn't preclude us from trying to achieve model-observation agreement, but these future studies will need to acknowledge the inherent uncertainty of the observations and therefore of the "truth" that models aim to achieve.

We have added a comment on the interlinked realistic and idealised model development in the Introduction where we mention RISE and MISOMIP:

> L96 - 99: Whilst realistic model intercomparisons facilitate validation with observations (where they exist) and future projections, they also have added complexity that makes untangling the consequences of model choices more difficult: therein lies the value of idealised models as a well-controlled verification and benchmarking tool, which will likely continue to be used in future model development as we seek to achieve model agreement with the real world.

I'm not saying that such experiments would not be of interest. They would be really valuable in addressing that first goal of documenting differences on domains that are actually in use for projections. But if the community is not to lose sight of the second goal of genuinely improving the physical representation of the processes within the models, wouldn't a return to ISOMIP, and more work on ISOMIP+ be equally valuable? Running all the current models on ISOMIP would be interesting and you could maybe run three sets: one with a COM-type setup; one with a TYP-type setup; and one with a TYP-TUNE-type setup, where modellers would follow their typical tuning procedure to match the original 1997 results as closely as possible (which is what I assume has been done in the literature to date). In fact, that last set is arguably what is missing from the experiments in the current paper. In having the tuned COM experiments defined with most choices constrained, while the unconstrained TYP experiments are not tuned, the authors have arguably done the models a disservice and maximised the discrepancies. I accept that without knowledge of the "correct" solution there is not really a target that models can be tuned too. However, therein lies the advantage of ISOMIP. There is at least an "accepted" solution, even if we don't know much about its correctness.

Thank you for the suggestion. This is an important aspect of future work to include in our manuscript: the fact that model development is non-linear, where we both aim to go to more complex and realistic domains, but also improve our parameterisations, and add and assess new model features starting with idealised experiments. The field needs both of these approaches and will need to cycle between them as our models improve and become more complex.

As we have alluded to, there are benefits and drawbacks between using ISOMIP, ISOMIP+ or more complex setups, depending on what question you wish to answer. We should also keep in mind the burden on modelling groups, who must prioritise their work: many groups are now using realistic domains and therefore realistic intercomparisons align nicely with existing efforts. Your TYP-TUNE suggestion is certainly an avenue for future work, and would allow for much more concrete conclusions than the TYP experiments presented in our paper. If the ISOMIP domain is used, the simplicity of the domain may mean efforts are focused on code verification and parameter tuning rather than assessing Antarctic-relevant physics – but it is still a useful avenue of work. We have added this suggestion to the Discussion

> L841 - 845: Future work could more systematically probe causal links between model choices and consequent model solutions by performing parameter testing experiments or by tuning the TYP experiments to, for example, achieve a certain melt rate (as in COM; only the NEMO and FVCOM models used this approach in the TYP experiments presented here, Fig. 17). These experiments would test whether model states can be made more similar if their parameters are freer to vary, noting that results likely still depend on the complexity of the model configuration used.

It should also be mentioned that progress towards an analytical solution of ISOMIP+ or other idealised models with elements of realistic ice shelf physics is also an avenue for future work, and would solve the issue of the lack of a "true solution". Alternatively, we could explore modelling methods to achieve convergence, such as increased vertical resolution that allows for melt rates to converge (Scott et al., 2023), even if the solution does not include all the realistic boundary layer physics. In this way, idealised experiments with added complexity could have a more accepted solution, like in ISOMIP. We have added some text to the discussion on this:

> L877 - 882: In lieu of observations or an analytical solution in idealised models, the latter of which is an area for future work, attempting to achieve model solution convergence with increased resolutions may better establish a truth for benchmarking of these models.

I accept all this is actually something that should form part of a community discussion, but I cannot help thinking that a short paragraph or two devoted to the best route forward and in particular the various merits of increased versus decreased complexity as well as re-running the ISOMIP+ experiments in the light of lessons learnt would be of value.

Thank you for all your thoughtful comments. These are certainly important questions that should be addressed to the whole community. We have rewritten the final three paragraphs of the Discussion in summary of this conversation:

> L846 - 882: The variability seen in the ISOMIP+ ensemble may also be influenced by the model protocol. Models appear more similar in the original ISOMIP (Grosfeld et al., 1997; Losch, 2008; Holland et al., 2008; Galton-Fenzi, 2009; Gwyther et al., 2015, 2016; Mathiot et al., 2017), but this inter-model consistency largely reflects the simplicity of that configuration, which omits key processes relevant to Antarctic ice-shelf cavities (e.g. warm CDW inflow, thin turbulent boundary layers, complex topography). ISOMIP+ was designed to stress those processes, though still with a highly idealised geometry, and consequently highlights where models may differ: e.g. in the boundary layer, associated with vertical coordinates, or due to topography interpolation and smoothing differences at the boundaries amplified by the small domain. Other differences in ISOMIP+ may be exacerbated by the simplified forcing conditions: the open-ocean gyres that differ greatly between models are likely an artifact of the idealised, sponge-forced model setup that is highly sensitive to geometry choices (Zhao et al., 2022). The model variability in ISOMIP+ highlights that conclusions made with one idealised ocean model may not be directly applicable to other ocean models. This may be an important consideration for melt parameterisation development, particularly with machine learning methods. The results also demonstrate that the model representation of the ice shelf–ocean boundary layer is an area for future development.
>
> Though there are clear differences between the ice shelf–ocean boundary layer and melt rates across models, there are already some methods to reconcile these. Different models may need different model parameters to achieve the same state, unlike the prescriptive COM protocol here. By calibrating the depth over which temperature, salinity and velocities are sampled in the three-equation melt parameterisation and the distance over which freshwater or a virtual salt flux is distributed, similar melt rates can be achieved with different vertical coordinates and resolution (Gwyther et al., 2020). Melt rates can also converge as the vertical resolution is enhanced (Scott et al., 2023). Models will likely require tuning and development of new melt parameterisations in future Antarctic ice shelf simulations, in both idealised and realistic configurations, to achieve melt rates and ice shelf cavity conditions consistent with *in situ* observations. The integration of melt parameterisations with the model-specific interior vertical mixing schemes, which may depend on vertical coordinates, is also an area for future work.
>
> When simulating realistic geographic domains, model choices are expected to also

> play a large role between models (Naughten et al., 2018b; Richter et al., 2022; Galton-Fenzi et al., in press). However, ocean models may be constrained to prescribed atmospheric forcings or tuned to achieve an ocean and ice state similar to existing observations, using greater parameter freedom than in ISOMIP+. Realistic ice shelf–ocean model intercomparison projects such as RISE (Galton-Fenzi et al., in press) and MISOMIP2 (De Rydt et al., 2024) are therefore able to validate models and assess biases compared to limited observations, something ISOMIP+ cannot do. These realistic projects will provide the next assessments of our ice sheet-ocean modelling capabilities and guide future model development. As has been done in the past, this model development will likely rely on idealised models of varying complexity: highly idealised for verification, and adding complexity for benchmarking. For example, the next generation of idealised ice shelf–ocean models might consider including features not included in ISOMIP+, such as rough topography, eddies and more realistic ice shelf cavity stratification, and learn from the difficulties of the ISOMIP+ sponge boundary. They may also consider a higher vertical resolution to better resolve the ice shelf–ocean boundary layer. In lieu of observations or an analytical solution in idealised models, the latter of which is an area for future work, attempting to achieve model solution convergence with increased resolutions may better establish a truth for benchmarking of these models. Going forward, we see idealised and realistic experiments as complementary, and expect the community will continue to oscillate between the two in order to both understand mechanisms and test against observations, as we work towards improved ice shelf–ocean model fidelity.

Specific comments:

Lines 58-59: This sentence summarises the study of Gwyther et al (2020), but with such abbreviated prose that I think the meaning will remain obscure to anyone lacking a detailed knowledge of melt rate parameterisations. Perhaps the sentence could be expanded a little for clarity?

We have rewritten the sentence as

> L67 - 69: Gwyther et al. (2020) use three ISOMIP+ models to assess the sensitivity of melt rate to specific model choices in the melt parameterisation; specifically, the distance over which a far-field temperature is sampled, and the distance over which freshwater or melt fluxes are distributed.

Line 60: What does "melt range convergence" mean?

We are referring to the sensitivity of melt rate to vertical resolution, where Scott et al. (2023) find melt rates converge to a value at high vertical resolution. We have clarified this, writing

> L70 - 72: Additionally, Scott et al. (2023) explore the sensitivity of melt rate to vertical resolution and find that melt rates converge at high vertical resolution, whilst Zhou and Hattermann (2020) quantify pressure gradient errors.

Line 68: "Zhou et al. (2024) use two ocean ..."

We have added "use".

Lines 74-78: A slightly muddled sentence. Do you mean "Model developments ... realistic domains for future projections of ... and guide melt parameterisations for ..."

We have rewritten as suggested,

> L87 - 91: Model developments in idealised experiments can support and ultimately transition to realistic domains for future projections of Antarctic ice shelf melt (e.g. Timmermann and Hellmer, 2013; Naughten et al., 2018a; Siahaan et al., 2022; Jourdain et al., 2022; Kusahara et al., 2023; Mathiot and Jourdain, 2023; Naughten et al., 2023; Bett et al., 2024; De Rydt and Naughten, 2024) and guide melt parameterisations for simulations used in current and future Intergovernmental Panel on Climate Change (IPCC) reports (Jourdain et al., 2020, 2022; Burgard et al., 2022).

Lines 113-116: Not easy to follow. Does the calving front vary between models and differ from the BISICLES ice front?

Some of the models smooth the BISICLES calving front. We have rearranged the paragraph so that the mention of BISICLES calving front is earlier. We have also added the mention of smoothing as follows

> L133 - 134: However, where interpolation results in an ice shelf thickness less than 100 m, the thickness is set to zero to represent a steep calving front, except where smoothing of the calving front was required for model stability.

Lines 126-128: What was the motivation for keeping the density profile the same between the two experiments? That choice takes you away from setups that you can really describe as "like" the cold versus warm regimes typical of realistic continental shelves. The key point is that the cold shelves are cold because the stratification is weak. Having strong stratification in the cold case is what causes the deep detachment of the melt-laden outflow and lack of freezing, which put you in an atypical regime.

The density profile was kept the same between the two experiments in the Asay-Davis et al. (2016) experimental protocol, where the motivation for keeping the density profiles constant is stated as to reduce "convective instabilities resulting from the transitions between COLD and WARM conditions that occur in Ocean1–2" experiments. This method also means that stratification gradients are controlled by meltwater fluxes rather than the boundary forcing. We agree that the cold salinity profile varies from conditions typically seen in cold Antarctic ice shelves. We have modified the text to highlight this:

> L145 - 149: By making use of the experiment's linear equation of state, the cold and warm profiles are designed to have the same density profile (Fig. 6 from Asay-Davis et al., 2016) to reduce convective instabilities. Having the same density profile also means that density variations are solely created by ice shelf meltwater, not by the boundary (Holland, 2017). However, the salinity stratification of the cold profile is stronger than the conditions observed in cold Antarctic ice shelves (e.g. Orsi and Wiederwohl, 2009; Nicholls et al., 2004; Darelius et al., 2014).

Line 170: "… it increases to account for …"

Thanks, we have added the missing "to"

Line 223: "… uses partial cells to represent better the bottom …"

We have changed "better represent the bed topography" to "represent the bed topography better".

Lines 321-322: Doesn't NEMO use a finite volume discretisation?

The NEMO simulations presented here use a finite difference scheme to solve the primitive equations. Please see `https://www.nemo-ocean.eu/doc/node19.html`.

Line 422: Describing the circulation as “western-intensified” seems a little misleading for simulations on a f-plane. Presumably any intensification is a result of water column thickness gradients and, while it may be on the topographic “western” side, any relation to the geographic west will be coincidental.

We agree, hence why western was in quotes for emphasis. We will replace “ “western”-intensified (y>50 km) ” with “boundary-intensified (y>50 km)”. We will also replace the other mention of western in the paper.

Figures 7, 8, 9 and 10: In these figures the ice geometry looks quite different between models. In particular, differences in 7 and 9 are much more pronounced than in figures 2 and 3. Presumably this the difference between a central section and a width-averaged view of the geometry and highlights that the topography is significantly different at the margins. Presumably that is a result of differing interpolation schemes and cut-off values used to infer grounding line and ice edge, but doesn’t it mean that all the models effectively had different distributions ice shelf basal gradient, and hence buoyancy forcing. Given that, differences in model response aren’t really surprising, are they? This is a strong argument for using a simpler geometry, where interpolation schemes would not end up giving you such different geometries. This point is illustrated in the Supplementary material, but could be drawn out more prominently in the main text as it is important for the interpretation of the results.

Thank you for the suggestion, this is important for us to highlight. We have added some text when the melt rate patterns are shown:

> L446 - 449: These differences in melt rate patterns, particularly near the grounding line and side walls, are likely to be at least partly associated with differences in bed topography and ice shelf draft between models that are enhanced near the side walls, grounding line and ice front (Figs. S11, S12).

and also emphasised it where it was previously mentioned:

> L482 - 484: Variation in circulation between models may be associated with the different interpolation and smoothing choices of the topography and ice draft, *particularly near the side walls, grounding line and ice front* (Sect. 3, Figs. S11, S12) and are likely also related to the different melt rate spatial distributions (Figs. 5, 6, 7).

and also added it to the Discussion

> L793 - 798: Variation in melt rate spatial distributions, particularly near the grounding line and side walls, has implications for ice sheet evolution in coupled ocean–ice sheet models to be explored in the complementary MISOMIP1 model analysis (Seroussi et al. *in prep*). Overturning and barotropic streamfunctions also indicate differences in the cavity circulation patterns (Fig. 7- 10). *Both of these spatial distribution differences are likely to be at least partly associated with differences in bed topography and ice shelf draft as a result of model smoothing, particularly near side-walls, the grounding line and ice front, and vertical coordinate choices.*

Lines 451-452: I don’t see how the results in figure 10 resemble observation. I don’t know of any observation that is suggestive of a reversed overturning circulation in the upper water column. Many papers going back to the 1970’s discuss a lower and an upper circulation cell producing Deep (DISW) and Shallow (SISW) forms of Ice Shelf Water, but they all (to my knowledge, at least) suggest that both cells are clockwise (i.e. in at depth and out along the ice shelf base). The reversed circulation looks odd to me and wouldn’t produce DISW and SISW in any case, but rather a single outflow sourced from waters above and below that level in the water column. I know of no observations that support that, and I assume it is an artefact of the unrealistic stratification (commented on above).

We had added the references to highlight that mixed-source ice shelf cavity circulations have been observed: for example, Mode 1 and Mode 3 ice shelves using the Jacobs et al. (1992) framework, or the Berkner mode (Hattermann et al., 2021; Janout et al., 2021). In particular, the Berkner mode refers to an inflow of water at the surface freezing point with a different density than the outflowing Ice Shelf Water (in contrast to the warm surface water of Mode 3 melt that drives clockwise overturning circulation) (Hattermann et al., 2021). This cold inflow will not cause much melting of shallower ice, hence not drive a strong overturning, but it means that the inflow, outflow and their depths (i.e. the overturning streamfunction) will depend more on the ambient stratification and other drivers of the circulation than the local buoyancy input. Observations of circulation beneath ice shelves remain sparse, so perhaps we have not observed these environments sufficiently to discount the realism of the anti-clockwise circulation.

In our experiments, the reversed circulation in Ocean2 is influenced by our idealised sponge wall boundary condition as well as the strong salinity profile and the steep ice base geometry, so it is not locally controlled by the small melt-induced buoyancy fluxes at shallow depths (a parallel to the Berkner mode). To avoid confusion, we have clarified that the reversed overturning cell has not been (to our knowledge) observed before, and is a feature of the model setup, and we also expanded on the existing text:

> L506 - 513: Note that the simulated shallow counterclockwise circulation is likely related to density gradients in the domain that develop in response to the boundary restoring to the salinity-stratified cold profile, in combination with the steep ice base geometry, rather than local buoyancy fluxes (which are small due to the low melting at shallow depths, Fig. 6). The circulation is a feature of our model setup, noting the unrealistically strong stratification, and that reversed overturning cells of this extent have not been observed in Antarctic ice shelves (to our knowledge). However, the counterclockwise circulation may not be completely unrealistic since mixed-source circulation and a surface inflow of shelf water have been observed inside Antarctic cold cavity ice shelves (Hattermann et al., 2021; Janout et al., 2021) and observations of large-scale currents beneath Antarctic ice shelves remain limited.

Line 554: “Many of these differences . . . ”

We have replaced “Differences...” with “Many of these differences . . . ” as suggested.

Figure 13 (and others): If I understand correctly (not guaranteed as the description is not completely clear to me), the vertical coordinate is discretised into 5 m bins and measured from the defined ice shelf surface. So, the absolute depths are consistent between panels. But wouldn’t it be a fairer comparison of the models to plot results as a function of distance from the discretised ice shelf surface used in each model. That way the distance from the ice base would be the consistent coordinate between panels. Isn’t that the key variable?

In the processed model output submitted by each model team, the vertical coordinate is discretised into 5 m bins from a reference depth of 0 m, so, for example, in Fig. 2, the depths are absolute values. In Fig. 13, we shift the vertical coordinate by the given ice draft variable, which usually does not lie on the 5 m bin values. This is the only way we can approximate the distance from the ice draft, as the data on the model grids did not form part of the model submissions. We agree it would be ideal to make the distance from the ice base the consistent coordinate, but unfortunately due to the lack of raw data this is not possible.

We have clarified the point on the 5 m bin discretisation:

> L620 - 622: This remapping produces the jagged features of Fig. 13: model output is remapped onto a discrete grid with 5 m vertical spacing, relative to 0 m depth, whereas the ice draft used in each model can vary continuously.

Lines 677-679: So, does that suggest that the target melt rate in the COM experiments was too high? How was it set?

The target melt rate was originally chosen by Asay-Davis et al. (2016) to be representative of warm-cavity ice shelves such as Pine Island in the late 2000s, when shelf-mean melt rates were commonly estimated at a few × 10 m/yr, with localized maxima exceeding 100 m/yr near the grounding line. Subsequent work still supports shelf-mean values in the ∼20–30 m/yr range for PIIS-like settings.

Was the target melt rate for COM set too high? Not necessarily. The fact that most TYP configurations produce lower melt than COM does not imply that the COM target was unrealistic. Nearly all TYP setups are at coarser resolution, so cavity circulation is underresolved; even with tuning of heat transfer and/or drag coefficients, this tends to produce sluggish flow and therefore lower melt rates.

To make this clear, we have changed the text to:

> L741 - 744: It is generally true that a group's TYP model configuration produces less melt in response to the warm ocean forcing than using the idealised COM protocol (Fig. 16a, b), but this primarily reflects differences in resolution and parameterizations rather than the COM target melt rate being unrealistically high.

With hindsight, it is clear that achieving the COM target requires relatively strong turbulent mixing in the sub-ice-shelf boundary layer. The experimental design prescribed low background vertical diffusivity, so models with high vertical resolution but without explicit parameterizations of sub-ice-shelf boundary-layer mixing tended to produce lower melt. In that sense, the target was ambitious relative to the mixing assumptions of the COM design, even though it remained within the observationally supported range for PIIS-like cavities.

This point is already discussed later in the manuscript in the paragraph beginning,

> L820 - 821: Low melt rates in ROMS and FVCOM can be explained by the high vertical resolution of their terrain-following coordinates, leading to relatively thin upper cell thicknesses.

We modified the last sentence of that paragraph to answer the question more explicitly:

> L832 - 834: At the same time, the results from the COM and TYP experiments highlight the sensitivity of the simulated melt rates and ice-ocean boundary layer structure to the integration of the applied basal melt parameterisation and the model's interior vertical mixing parameterisation, rather than indicating that the COM target melt rate itself was unrealistic.

Reviewer 2

Review of: Results of the second Ice Shelf-Ocean Model Intercomparison Project (ISOMIP+)

Authors: Claire K. Yung et al.

This study reports model output from the ISOMIP+ study, which consisted of 12 different ocean model configurations simulating ocean circulation and ice-shelf melt rates in idealized ice-shelf cavities. The simulations consist of 20-year runs in two different configurations: (i) an initially cold-shelf cavity transitioning to a warm shelf regime due to a far-field sponge-layer forcing and (ii) an initially warm-shelf cavity transitioning to a cold-shelf regime due to a far-field sponge-layer forcing. The warm experiments (warm far-field properties) are mostly equilibrated while the cold experiments appear to still be in a stage of transient adjustment after 20 years. The study reports differences in ice-shelf cavity hydrographic properties, circulation properties, and melt rates. The choice of vertical grid appears to have one of the largest impacts on variations across the models because of the representation of ocean thermal and velocity properties near the ice-ocean interface.

We greatly appreciate the effort that has been undertaken by this group of modelers to run these simulations and to bring the output together for this comparison. This study will make a nice contribution to the literature and will be an important resource in developing the next generation of coupled ocean-ice shelf models. There are two major suggestions, described in more detail below. First, the manuscript would benefit from a more concise summary of where this group of authors perceives that future model development and model choices will have the greatest impact in improving numerical representations of ice shelf-ocean interactions. Second, the lack of information and discussion of the density structure and stratification in the main text is a significant omission in this manuscript.

We recommend major revisions because of the number of comments that are provided below, but I do not anticipate that these will be too hard to address.

*** This is a joint review from a senior and an early career researcher. ***

Thank you very much for your thorough review, and your support of our paper. We appreciate your thoughtful comments that have improved the clarity of our manuscript, and we have added some future-looking discussions.

We added more detail in our mentions of future model development in the Discussion, and also added a discussion on the density stratification in the results.

Major comments

• As an overall comment, this manuscript would benefit from a clearer statement about how these results inform priorities for future model development or model design choices. The degree to which a more synthetic interpretation of the model results could be provided would make this paper more valuable to the scientific community interested in ice-ocean interactions. The group of authors that contributed to this manuscript are the leading experts in this field — it would be great to have a more nuanced (and more opinionated!) discussion of these simulations in the Discussion section.

Thanks for the suggestion. Also based on feedback from Reviewer 1, we have rewritten the last few paragraphs of the Discussion to emphasise the role of both idealised and realistic models in future ice shelf–ocean model development, as well as the different purposes of different complexity ice shelf–ocean model intercomparisons.

> L846 - 882: The variability seen in the ISOMIP+ ensemble may also be influenced by the model protocol. Models appear more similar in the original ISOMIP (Grosfeld et al., 1997; Losch, 2008; Holland et al., 2008; Galton-Fenzi, 2009; Gwyther et al., 2015, 2016; Mathiot et al., 2017), but this inter-model consistency largely reflects the simplicity of that configuration, which omits key processes relevant to Antarctic ice-shelf cavities (e.g. warm CDW inflow, thin turbulent boundary layers, complex topography). ISOMIP+ was designed to stress those processes, though still with a highly idealised geometry, and consequently highlights where models may differ: e.g.

in the boundary layer, associated with vertical coordinates, or due to topography interpolation and smoothing differences at the boundaries exacerbated by the small domain. Other differences in ISOMIP+ may be amplified by the simplified forcing conditions: the open-ocean gyres that differ greatly between models are likely an artifact of the idealised, sponge-forced model setup that is highly sensitive to geometry choices (Zhao et al., 2022). The model variability in ISOMIP+ highlights that conclusions made with one idealised ocean model may not be directly applicable to other ocean models. This may be an important consideration for melt parameterisation development, particularly with machine learning methods. The results demonstrate that the model representation of the ice shelf–ocean boundary layer is an area for future development.

Though there are clear differences between the ice shelf–ocean boundary layer and melt rates across models, there are already some methods to reconcile these. Different models may need different model parameters to achieve the same state, unlike the prescriptive COM protocol here. By calibrating the depth over which temperature, salinity and velocities are sampled in the three-equation melt parameterisation and the distance over which freshwater or a virtual salt flux is distributed, similar melt rates can be achieved with different vertical coordinates and resolution (Gwyther et al., 2020). Melt rates can also converge as the vertical resolution is enhanced (Scott et al., 2023). Models will likely require tuning and development of new melt parameterisations in future Antarctic ice shelf simulations, in both idealised and realistic configurations, to achieve melt rates and ice shelf cavity conditions consistent with *in situ* observations. The integration of melt parameterisations with the model-specific interior vertical mixing schemes, which may depend on vertical coordinates, is also an area for future work.

When simulating realistic geographic domains, model choices are expected to also play a large role between models (Naughten et al., 2018b; Richter et al., 2022; Galton-Fenzi et al., in press). However, ocean models may be constrained to prescribed atmospheric forcings or tuned to achieve an ocean and ice state similar to existing observations, using greater parameter freedom than in ISOMIP+. Realistic ice shelf–ocean model intercomparison projects such as RISE (Galton-Fenzi et al., in press) and MISOMIP2 (De Rydt et al., 2024) are therefore able to validate models and assess biases compared to limited observations, something ISOMIP+ cannot do. These realistic projects will provide the next assessments of our ice sheet-ocean modelling capabilities and guide future model development. As has been done in the past, this model development will likely rely on idealised models of varying complexity: highly idealised for verification, and adding complexity for benchmarking. For example, the next generation of idealised ice shelf–ocean models might consider including features not included in ISOMIP+, such as rough topography, eddies and more realistic ice shelf cavity stratification, and learn from the difficulties of the ISOMIP+ sponge boundary. They may also consider a higher vertical resolution to better resolve the ice shelf–ocean boundary layer. In lieu of observations or an analytical solution in idealised models, the latter of which is an area for future work, attempting to achieve model solution convergence with increased resolutions may better establish a truth for benchmarking of these models. Going forward, we see idealised and realistic experiments as complementary, and expect the community will continue to oscillate between the two in order to both understand mechanisms and test against observations, as we work towards improved ice shelf–ocean model fidelity.

Also based on Reviewer 1's feedback, we made some suggestions for ways to test achieving model agreement with different parameters:

> L841 - 845: Future work could more systematically probe causal links between model choices and consequent model solutions by performing parameter testing experiments or by tuning the TYP experiments to, for example, achieve a certain melt rate (as in COM; only the NEMO and FVCOM models used this approach in the TYP experiments presented here, Fig. 6). These experiments would test whether model states can be made more similar if their parameters have greater freedom to vary, noting that results likely still depend on the complexity of the model configuration used.

- A related comment: At the moment, each simulation is presented as an equally valid realization of ocean circulation in the cavity. It is clear, however, that certain simulations are well outside of the expected behavior. For a general reader, it would be helpful to provide some critical assessment of which models perform more realistically and perhaps more importantly, why certain models seem to be failing. This is not a criticism of any model, but a way to learn what impacts model fidelity. It would be nice to capture this in the Discussion section.

We agree that for some model simulations, the behaviour is abnormal, for example the barotropic circulation in COCO differs from all the other configurations. However, as mentioned in the manuscript (L208), anomalous behaviour of models is likely a consequence of problems with the model configuration (e.g. forcing files, advection scheme not high enough order etc.) rather than issues with the model itself. In some cases, due to the age of the model configuration and results, we cannot determine what the cause of the anomalous model behaviour is. This uncertainty makes determining the issues or choices that affect model fidelity difficult.

We expanded our paragraph in the Discussion (L745) to point out all the models that have anomalous results, and the learnings that we can take from the exercise. Anomalous model behaviour has allowed us to find and fix model configuration errors (e.g. advection scheme in MITgcm) as well as identify numerical schemes that need improvement (such as the unphysically cold temperatures in POP2x). The expanded paragraph reads

> L805 - 819: There are also outliers in certain diagnostics. The COCO setup used in ISOMIP+ has an anomalously strong circulation, possibly related to spurious currents from the implementation of the sponge boundary. The NEMO-UKESM1is setup used here has a known surface cooling error that results in a cooler interior. The MITgcm models used here have anomalously fresh meltwater associated with the advection scheme (Fig. 16, S4), and the transfer coefficients of MITgcm-BAS appear to be inconsistent (Fig. S3). POP2x and MOM6-SIGMA-ZSTAR displayed anomalously cold temperatures in the T–S diagrams, and the POP2x transfer coefficient was far larger than other z-coordinate models, indicating possible numerical issues. Many of these differences are likely associated with the imperfect implementation of the idealised model protocol rather than fundamental issues with the models. However, the ability to compare these models as part of the ISOMIP+ project allowed anomalous model behaviour to be identified, and in some cases, resolved. Outliers worth highlighting are the low melt rates in the ROMS and FVCOM model submissions (Fig. 12), which could not achieve the target melt rate of $30\,\mathrm{m\,yr^{-1}}$ even with large transfer coefficients (Fig. 17a). The $30\,\mathrm{m\,yr^{-1}}$ target was contrived based on a z-level model and may not represent what melt the Ocean0 domain and forcing would create in reality, particularly given the other idealised model assumptions made, such as the low internal vertical mixing and resolution – a limitation of the experimental setup. The ROMS and FVCOM results high-

> light the effect of different vertical coordinates, meltwater forcing sampling and flux distribution schemes on simulating ice shelf basal melting.

• The absence of any information about the structure of the density field, specifically the ocean stratification, is a significant omission in this study. In terms of controls on circulation and near-ice-shelf transport properties, recent work has highlighted the importance of ocean stratification. This is particularly true for the gyre and overturning circulations—both controlled by geostrophic shear. The density will be determined by the salinity field, rather than the temperature field. I realize that salinity information is contained in the supplementary material, but I suggest moving a discussion of ocean/cavity stratification to the main text.

Thank you for raising this. We have added a comment on stratification being an important controller of circulation to the introduction of our results section which presents the transects. Since salinity controls density in our experiments, we moved the salinity transect of Ocean1 to the main text, and also mentioned this when the salinity transects are mentioned (near the temperature transects)

> L405 - 407: Temperature and salinity distributions at year 20 of the Ocean1 and Ocean2 COM simulations at the $y = 40\,\mathrm{km}$ transect show similarities between models (Figs. 2, 3 for temperature and Figs. S7, S8 for salinity, *noting that salinity variations control the density stratification*)
>
> L426- 431: Salinity transects in both experiments approximately indicate the density and stratification (Fig. 4, S8), which are important controllers of ocean circulation. The fresh salinity near the ice shelf–ocean boundary layer of the Ocean1 experiment indicates meltwater stratification, further highlighted by the increased density of the salinity contours near the ice. Stratification of the Ocean2 experiment (Fig. S8) is dominated by the far-field restoring forcing rather than meltwater effects, with relatively flat salinity contours. The density of the initial conditions and far-field forcings is the same for both cold and warm profiles, indicating that gradients of density are only due to meltwater interactions.

We also expanded on the Asay-Davis et al. (2016) protocol's choice to keep the vertical profile of density the same between the experiments:

> L145 - 149: By making use of the experiment's linear equation of state, the cold and warm profiles are designed to have the same density profile (Fig. 6 from Asay-Davis et al., 2016) to reduce convective instabilities. Having the same density profile also means that density variations are solely created by ice shelf meltwater, not by the boundary (Holland, 2017). However, the salinity stratification of the cold profile is stronger than the conditions observed in cold Antarctic ice shelves (e.g. Orsi and Wiederwohl, 2009; Nicholls et al., 2004; Darelius et al., 2014).

The density data provided by model groups is limited to a few temperature and salinity transects as well as the bottom and surface values (from which we can use the linear equation of state to determine density). Analysis of the density field and stratification and its effect on circulation is therefore limited. Since circulation is associated with stratification throughout the domain, the overturning streamfunction as well as $T - S$ diagrams are better integrated indicators of circulation and water mass transformation than our limited density transects. However, in this review response, we attach the density transects and N^2 Brunt-Väisälä frequency transects, which show the stratification along the $y = 40\,\mathrm{km}$ at year 20 for both Ocean1 and Ocean2 experiments (Figs. R3-R6). Many of these features are also visible in the temperature and salinity transects included in the manuscript. We added some text to highlight the stronger stratification of the ice shelf–boundary layer compared to the rest of the domain (see above text).

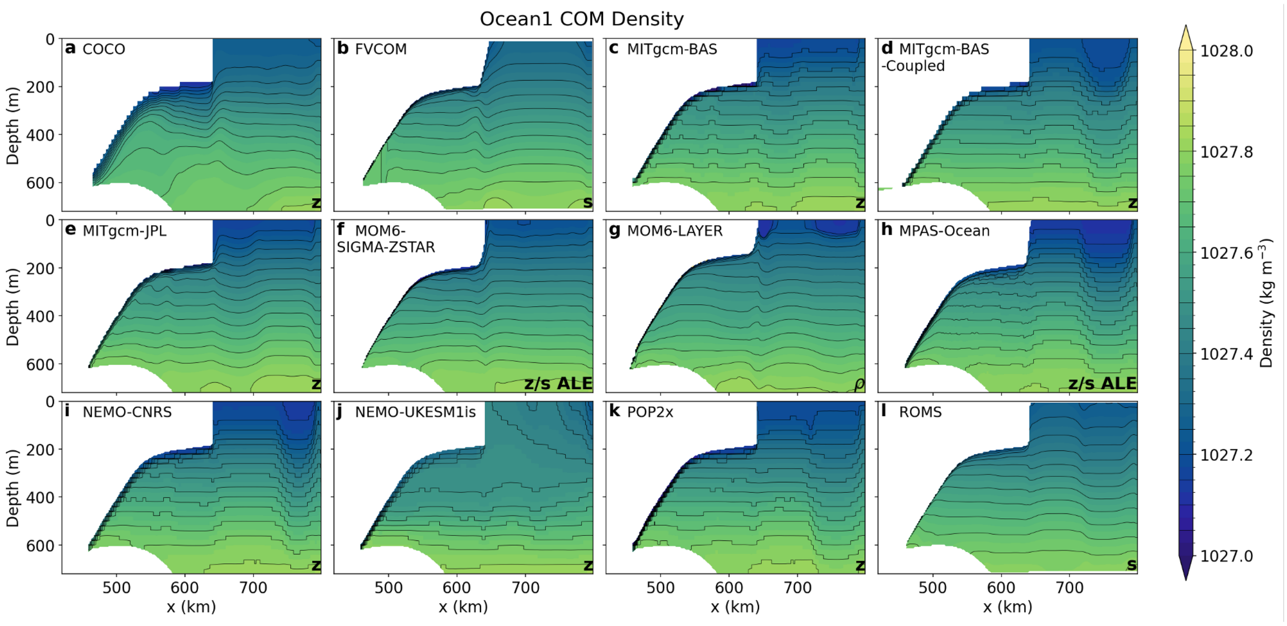

Figure R3: Ocean1 density transect through $y = 40\,\mathrm{km}$, average of year 20

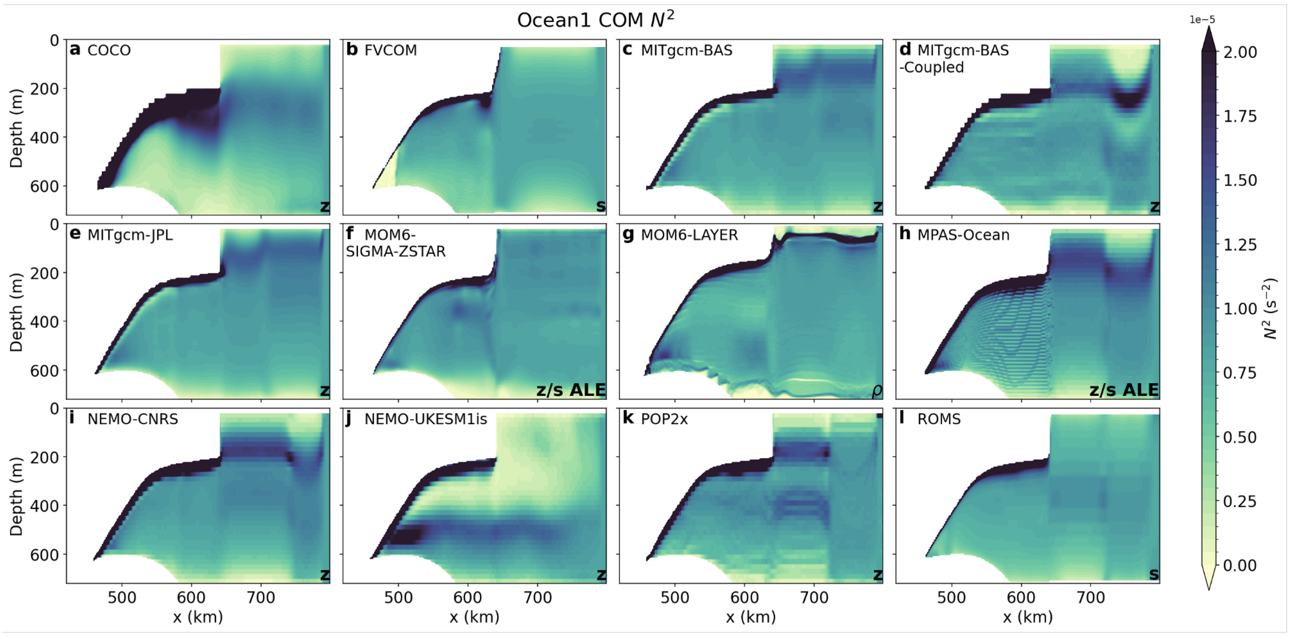

Figure R4: Ocean1 N^2 (Brunt-Väisälä frequency squared) transect through $y = 40\,\mathrm{km}$, average of year 20. Density fields were smoothed by a 20 m rolling average before taking vertical gradients.

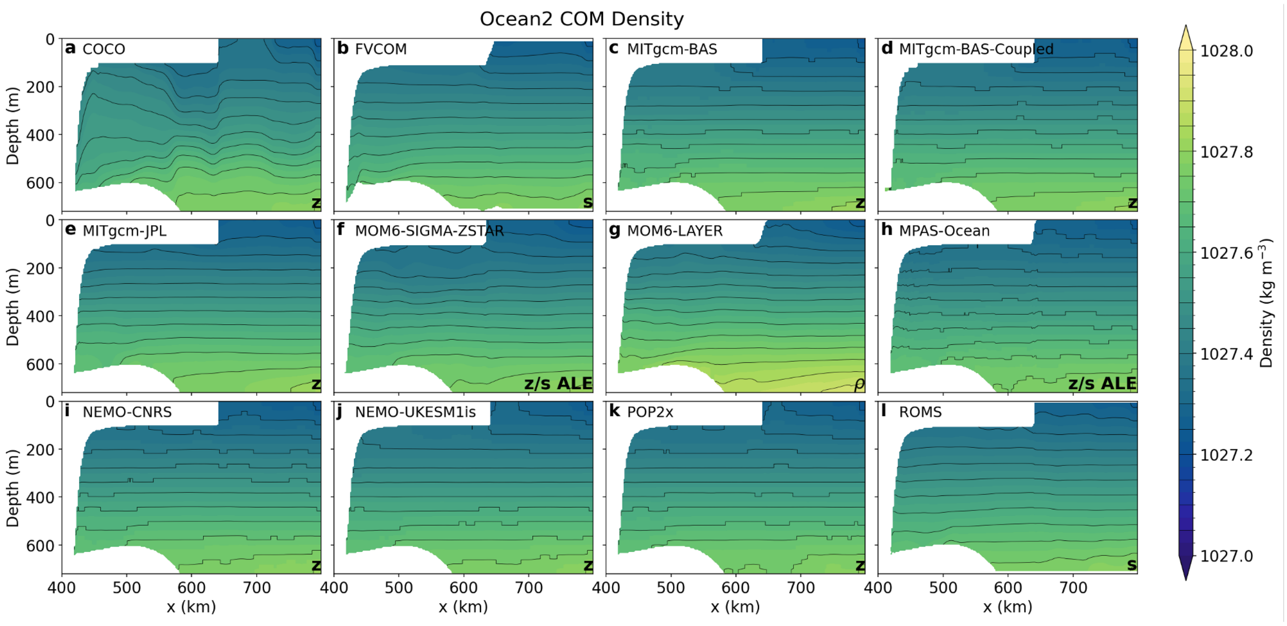

Figure R5: Ocean2 density transect through $y = 40$ km, average of year 20

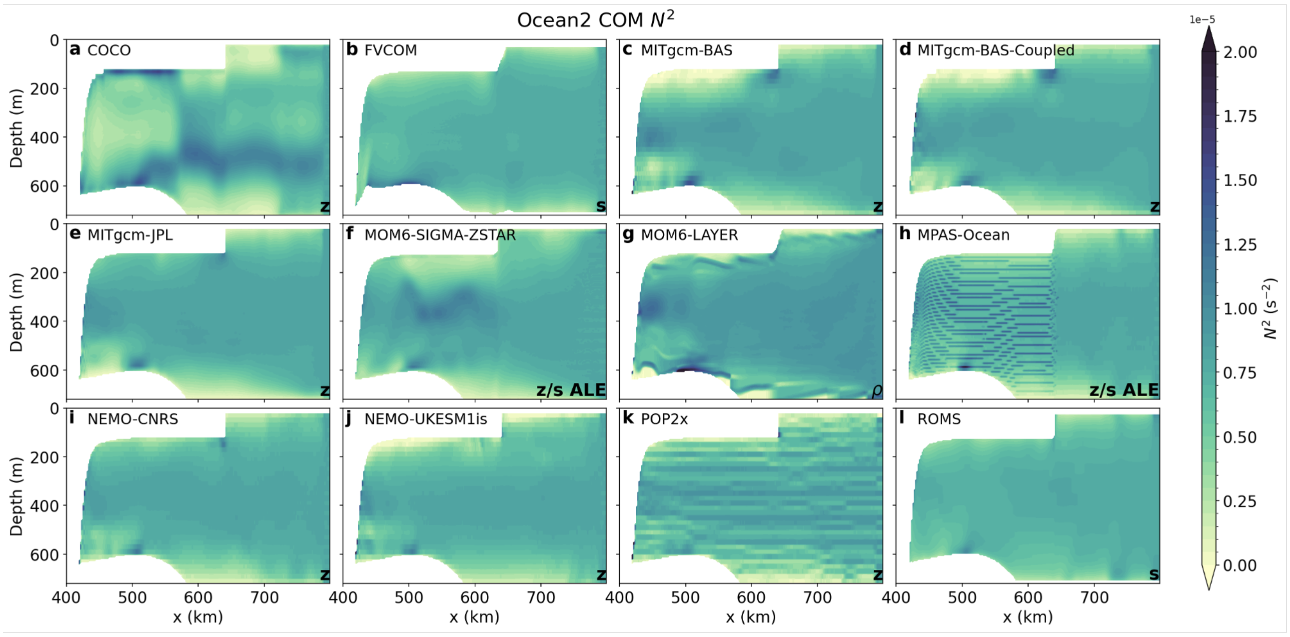

Figure R6: Ocean2 N^2 (Brunt-Väisälä frequency squared) transect through $y = 40$ km, average of year 20. Density fields were smoothed by a 20 m rolling average before taking vertical gradients.

- It would be helpful to have the overturning circulation diagnosed and presented in density coordinates, rather than in depth coordinates (the Eulerian overturning). Using a density framework provides a better assessment of water mass transformation processes in these simulations, which offers more physical insight into controls on the overturning. I appreciate that the output required for this calculation may not be available for all, or any, of the models. If this is not possible, it would helpful to have a more nuanced discussion of what the overturning represents in these models (adiabatic vs. diabatic processes). See the comment below about transient stratification adjustment and inferred vertical velocities.

We agree that a density framework provides better quantification of water mass transformation; however, the submitted model output of overturning streamfunction is only available in depth coordinates, and there is no 3D data available to compute it in density coordinates. It is therefore difficult to make a statement on diabatic/adiabatic processes seen here. In a different study, Yung et al. (2025) presented the overturning streamfunction of two ISOMIP+ models in density coordinates, but it is not directly comparable here as those experiments were run with different transfer coefficients and lower average melt rates. We added a sentence to mention the density vs depth framework in the preamble of section 4.2, and recommend that future model intercomparison experiments include the overturning streamfunction in density coordinates as a required diagnostic, in line with standard practice in oceanography:

> L455 - 460: The overturning streamfunction is presented in x–depth coordinates due to model data availability: using density coordinates may have allowed better quantification of water mass transformation in the ice shelf cavity (Webber et al., 2019), but circulations are qualitatively similar (see, e.g. Fig. 6 in Yung et al., 2025, but note they use smaller transfer coefficients and have lower melt rates than the ISOMIP+ protocol). Future model intercomparisons should consider including the density-coordinate overturning streamfunction as a required diagnostic, and may also consider diagnosing adiabatic and diabatic contributions to heat transport.

- I am concerned that most of the variations across the cold-shelf simulations are due to the fact that the models are at different stages of adjustment to a new equilibrium. This is strongly suggested by the model results as well as the discussion in the manuscript. This is quite different from the warm shelf experiments, where multi-model variations are more likely due to different representations of boundary layers, geometry, vertical coordinate schemes, etc. The authors should think critically about whether the cold-shelf experiments provide insight into how model differences influence cavity properties and ice-shelf melt. While the authors are clear about the transient nature of these experiments, perhaps a shorter paper focused on the warm-shelf experiments would be more appropriate (and still worthy of publication).

We agree that many differences between the cold cavity simulations likely associated with the spin-down process not yet reaching equilibrium. However, there are still some interesting findings associated with the Ocean2 results, for example, including Ocean2 allows us to compare spin-up and spin-down processes with Ocean1, and furthermore, it is interesting that the Ocean2 models spin down differently and at different timescales! Additionally, including the Ocean2 results allows us to present a complete summary of the ISOMIP+ results, including the lessons learnt - e.g. timescales models should run for and the stratification and ice base shape used in idealised models. The Ocean2 results will also be a useful comparison for the MISOMIP coupled ice sheet–ocean experiments (Seroussi et al., in prep), where the Ocean2 experiment is the initial condition for simulating an ice sheet readvance (IceOcean1ra, IceOcean2ra, Asay-Davis et al., 2016).

However, based on your comment, we will be more precise with our description of the state in year 20, which is generally spun-up but not necessarily at steady state since there is still some transient behaviour, particularly in Ocean2. We modified the section titles (e.g. “Steady state circulation”) to “Spun-up” to be more precise about the ocean state, and added some text in the preamble, for example, for Section 4.1:

> L401 - 404: In this section, we present the spun-up temperature and salinity profiles and melt rate spatial distributions at the end of the Ocean1 and Ocean2 COM experiments. We take results from the average of the final year (year 20). By then, Ocean1 melt rates are approximately constant and at steady-state, but Ocean2 is still evolving and therefore still exhibits a small amount of transient adjustment

> (see Sect. 4.3 for further details).

Finally, note that the small temperature scale of Fig. 3 emphasises that the models are not fully spun up; temperatures in Ocean2 transect have maxima of ∼-1.5°C which is significantly colder than the 1°C initial conditions at the deepest part of the domain.

> L412 - 415: In Ocean2, temperatures are more uniform within the domain, matching the uniform -1.9 °C sponge forcing, but MITgcm-BAS-Coupled, MITgcm-JPL and NEMO-CNRS have a warmer interior of the cavity with temperatures reaching -1.6°C *(note the different colourbar to Ocean1, which emphasises these features).*

We will add some text to the discussion to emphasise these lessons learnt:

> L874 - 877: For example, the next generation of idealised ice shelf–ocean models might consider including features not included in ISOMIP+, such as rough topography, eddies and more realistic ice shelf cavity stratification, and learn from the difficulties of the ISOMIP+ sponge boundary.

A note on manuscript length: since we added the salinity transect for Ocean1 into the main text, we moved Fig 17 (the plot of thermal driving, friction velocity and melt rate for the TYP experiments) into the Supplementary to avoid increasing the already large number of figures in the main text.

Minor comments

• Abstract: It is always nice to have a few quantitative statements in the abstract. You might consider putting some values on the range of variability of key parameters.

Thank you for the suggestion, we have now added the range of thermal transfer coefficients to the abstract.

> L7 - 8: Different thermal *transfer coefficient values (ranging from 0.011 to 0.2)* are used for each model in the melting ....

• Line 21: You might consider some earlier citations here as it has been well known for some time that AABW formation impacts the global overturning, e.g. Purkey and Johnson papers, Talley 2013, or earlier studies.

Thanks for the suggestions. The key link we would like to make here is that ice shelves/Antarctic meltwater affect Southern Ocean circulation, which neither of the two studies mentioned focus on. Instead, we added citations to Fogwill et al. (2015), Bronselaer et al. (2018) and Golledge et al. (2019), which are earlier papers that demonstrated the link between ice shelf melt and Southern Ocean circulation.

• Line 30: “discrepancies among different models” consider providing a few examples.

We have rewritten the sentence as

> L32 - 33: However, there are still uncertainties in these simulations associated with model differences (Naughten et al., 2018b), compounded by uncertainties in ice sheet model projections (IPCC, 2021; Seroussi et al., 2020)

• Line 55: “higher spatial resolution” Be quantitative here if possible — by how much did the resolution increase?

Hunter (2006) define the protocol to be in degrees rather than kilometres. However, we can still make an approximate comparison:

> L59 - 61: ISOMIP+ also builds on the first generation of ISOMIP by using a higher spatial resolution *(from ∼3-9 km horizontal resolution and at least 10 vertical layers to ∼2 km and 36 vertical layers Hunter, 2006; Asay-Davis et al., 2016)* and a velocity-dependent basal melt parameterisation (Holland and Jenkins, 1999; Jenkins et al., 2010).

- Line 62: "incorporating the unresolved effect of stratification due to buoyant meltwater," Consider adding additional details here since previous models do represent vertical temperature and salinity profiles (and thus stratification).

Yes, this work developed a parameterisation to account for the unresolved feedback of stratification *on boundary layer turbulence* and therefore melt rates. We will rewrote it as "the unresolved feedback effect of stratification due to buoyant meltwater suppressing boundary layer turbulence and therefore melt rates."

- Introduction: The introduction captures the range of studies that have attempted to model ice-shelf cavities, but it mostly lists these papers. It would be nice to provide some synthesis about key outcomes as well as remaining gaps in modeling capabilities and future priorities (at least as perceived by this group of authors).

We added the following to the end of the ISOMIP+ papers paragraph:

> L76 - 78: Many of these studies, as well as earlier ISOMIP studies, explore sensitivities to resolution and melt parameterisation choices – reconciling parameterised melt with real observed ice shelf melt and cavity regimes remains a challenge for the community.

We have also rewritten the Discussion section (see major responses), hopefully this addresses some of your concerns on the lack of synthesis of future model priorities.

- I realize that these simulations are now ∼20 years old, but the manuscript needs a better justification of starting the warm experiments with a cold shelf and vice versa, especially as the focus of this study is on steady state behavior.

Just to clarify, the simulations were run between 2016 and 2020 (L233) so are all less than 10 years old. Additionally, we disagree that the focus is only on steady-state behaviour. The spun-up year 20 results are presented first but all of Section 4.3 is focused on the transient behaviour of the models, which is indeed one of the main commonalities between models (Fig. 13) as highlighted in the abstract (though we added "during the spin-up and spin-down" to the abstract in L12 to emphasise this). Please also see L213 where we justify the choice to use spin-up and spin-down experiments.

- Figure 1 caption: Ocean 1 and Ocean 2 in the text are referred to warm and cold, respectively, because of the boundary forcing. In the caption, Ocean 1 and Ocean 2 are referred to as a cold and warm, respectively, because of the initial condition. Please make sure these are consistent. Also, the text indicates that the domain is 480 km long, but only 400 km are shown — is the other 80 km the sponge layer? It would be helpful to indicate in the caption that the transects in the right-hand panels were taken at y=40 km.

We have clarified the caption of Fig. 1 as

> Fig 1: Experiment geometry and initial conditions, showing the bathymetry (a), and ice shelf draft for the Ocean0 and Ocean1 (b) and Ocean2 (c) experiments, and cross sections at $y = 40\,\text{km}$ of the temperature and salinity initial conditions for Ocean1 ("*cold initial conditions*", d,e) and Ocean2 ("*warm initial conditions*" f,g). The sponge forcing applied at the positive x boundary is the opposite (cold/warm)

> of the initial conditions, *therefore once models have been run to an equilibrium state, Ocean1 has "warm" conditions and Ocean2 "cold".* Ocean0 uses the warm initial conditions and warm sponge boundary.

In Asay-Davis et al. (2016), the domain is specified as being from $x = 320\,\text{km}$ to $x = 800\,\text{km}$ despite the ice being grounded for $x < 400\,\text{km}$ in both Ocean1 and Ocean2 geometries. Since the model output is on that grid, we would like to keep those numbers for consistency, but we have modified the text to read

> L123 - 125: The domain has size 480 km and 80 km in the x and y directions, respectively, *though part of this domain ($x < 400$ km) contains only grounded ice and is therefore not included in our figures.*

We also added hatching in Fig. 1b and Fig. 1c to show the sponge forcing location (790 km–800 km):

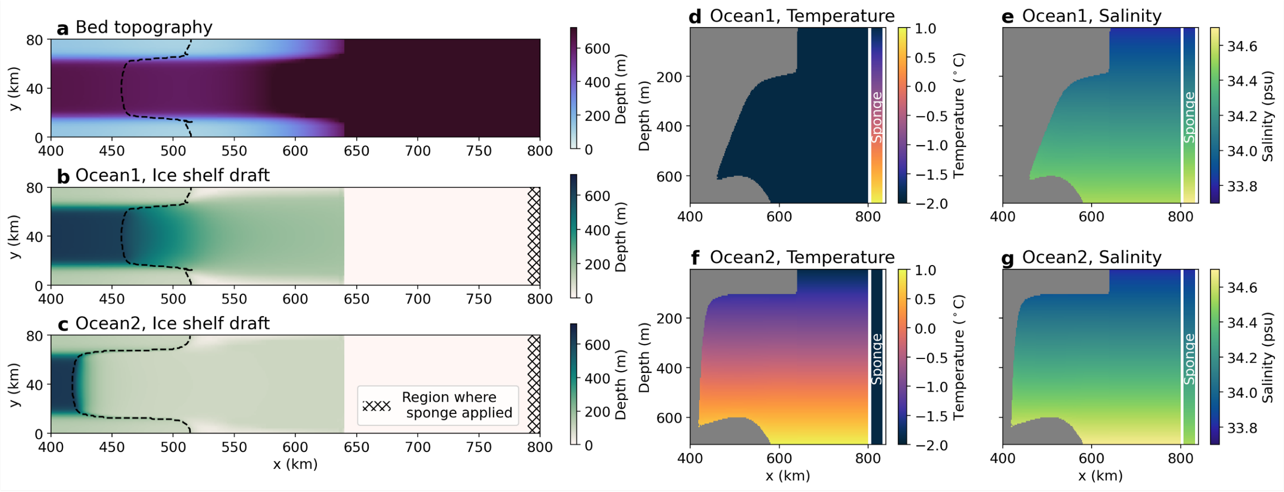

Figure R7: Revised Fig. 1

- Line 126: "By making use of the experiment's linear equation of state, the cold and warm profiles are designed to have the same density profile." This choice should be justified further — the density structure outside of ice-shelf cavities of warm and cold shelves are typically have very different density structures.

The density profile was kept the same between the two experiments in the Asay-Davis et al. (2016) experimental protocol, where the motivation for keeping the density profiles constant is stated as to reduce "convective instabilities resulting from the transitions between COLD and WARM conditions that occur in Ocean1–2" experiments. We agree that the cold salinity profile varies from conditions typically seen in cold Antarctic ice shelves. We added to L126 (now 145) and also added a sentence to highlight the unrealistically strong stratification:

> L145 - 149: By making use of the experiment's linear equation of state, the cold and warm profiles are designed to have the same density profile (Fig. 6 from Asay-Davis et al., 2016) to reduce convective instabilities. Having the same density profile also means that density variations are solely created by ice shelf meltwater, not by the boundary (Holland, 2017). However, the salinity stratification of the cold profile is stronger than the conditions observed in cold Antarctic ice shelves (e.g. Orsi and Wiederwohl, 2009; Nicholls et al., 2004; Darelius et al., 2014).

- Line 158: You might make is clearer there is only a single parameter that is tuned (Γ_T) although presumably the melt rate depends on both Γ_T and Γ_S.

We have rewritten it as

> L204 - 207: For the COM experiments, participants were requested to modify Γ_T *(and consequently* $\Gamma_S = \Gamma_T/35$ *also varies)* to achieve a target area-averaged melt rate at depth ($z < -300$ m) of $30 \pm 2\,\mathrm{m\,yr^{-1}}$. This tuning involved running multiple Ocean0 configurations to sample various Γ_T until the target melt rate was achieved, with larger Γ_T producing more melt via Eqns. 3–4.

- Line 225: How does a "a full step representation" reduce velocity noise? Naively this sounds like a steppier bathymetry and ice-shelf draft.

In COCO, the basal melt rate is sensitive to the vertical resolution of the topmost grid cell under the ice shelf (Gwyther et al., 2020). A Losch (2008)-style sampling layer is not used: tracers and velocities for the melt rate parameterisation are sampled from the top-most cell. If partial cells are used without the Losch (2008)-style scheme, this results in variation in the size of the melt parameterisation sampling depth. Because the basal melt parameterisation in the ISOMIP+ experiment directly links the velocity field and the melt, this sensitivity led to both the basal melt field and the velocity field becoming noisy as the melt rate was computed from velocities averaged over different depths.

To avoid this problem in the ISOMIP+ experiments, the COCO team did not use partial cells and instead adopted the full step representation, which mitigated this sensitivity and thereby reduced the velocity noise, as explained in Gwyther et al. (2020). We added a reference to Gwyther et al. (2020) to the main text at L251 for readers who are interested in the details.

- Line 239: Here and elsewhere in the text, the authors state that certain models were not able to reach the target melt rate. It would be nice to provide some additional information about why this melt rate was not achieved. Did the model crash with large transfer coefficients or did the melt rates saturate?

Melt rates from ROMS and FVCOM saturated at high transfer coefficient values. We added "as melt rates saturated with high transfer coefficient values" to each of the models' descriptions (L267, L394).

- Line 244: "the coupled ice–ocean framework as described in Jordan et al. (2018)" Can you confirm that the ice-shelf/sheet is stationary in this configuration, i.e. there is no grounding line migration? It appears that there is water inland of the grounding zone in Figure 2d for this configuration — this would be worth commenting on.

Yes, the ice shelf is stationary, but there is a small minimum water column thickness that is applied everywhere, which results in the water inland of the grounding zone. This is because the same model configuration was also submitted to the Ocean3-4 and MISOMIP1 experiments, where the ice sheet was then able to move, and this subglacial water layer is what enables grounding line retreat or advance in this coupling system. This choice has minimal effect on melt rates even in a coupled model (e.g. see Fig. 9d, Goldberg et al., 2018). We added further details to where this was mentioned:

> L284 - 286: MITgcm-BAS-Coupled imposes a minimum water column thickness of 0.5 m *for consistency with the Ocean3-4 and MISOMIP1 experiments, where the minimum water column thickness is required (Goldberg et al., 2018), with minimal effect on melt rates.*

Since this layer is not relevant to the cavity flow, we have now masked it out of Figure 2d to avoid further confusion.

- Section 3: While I appreciate that it is important to provide some details of each model used in this inter-comparison, the authors might consider whether key information could be provided more succinctly with a table. For instance, there seems to be a few key differences between models, e.g. where T/S values are diagnosed for estimating melt rates, the spatial scale of the ice-shelf geometry and bathymetry, etc., that have a strong impact on model differences. Summarizing these for the reader would make this section more readable. Also, a summary of what seem to be particularly sensitive or critical parameters at the end of the section would be valuable. The authors may consider moving model details to the supplementary material.

Thank you for the suggestion. It is difficult to categorise some of the model choices succinctly, such as the smoothing methods, in a table. However, we have added the flux distribution depth and meltwater sampling depths to the table.

Table 1: Summary of the model configuration submissions for the ISOMIP+ Ocean0, Ocean1 and Ocean2 experiments. We list the vertical coordinate (z-level, sigma/terrain-following (s-level), isopycnal, and z/s ALE for Arbitrary Eulerian-Lagrangian coordinates with a quasi z–ice shelf-following target coordinate), heat transfer coefficient for COM experiments, the flux mixing thickness (FMT, vertical distance over which melwater was spread), tracer sampling distance (TSD, for the melt parameterisation), meltwater addition method and whether TYP Ocean1 and Ocean2 experiments were also submitted. The method of meltwater addition is either a virtual salt flux or volume flux. In the case of a volume flux, we also specify whether the sea level is constrained to be constant via an adjustment applied to the entire open ocean, applied to just the sponge boundary, or not constrained ("none"). COM salt transfer coefficients were fixed according to $\Gamma_S = \Gamma_T \,/\, 35$.

Model configuration (submitter)	Vertical coordinate	COM heat transfer coefficient Γ_T	FMT	TSD	Meltwater addition (sea level method)	TYP
COCO (Kusahara)	z-level	0.025	Top cell	Top cell	volume flux (open ocean)	
FVCOM (Zhou)	s-level	0.2	Top cell	Top cell	virtual salt flux	✓
MITgcm-BAS (Jordan)	z-level	0.011 / 0.036[a]	20 m	20 m	virtual salt flux	
MITgcm-BAS-Coupled (Jordan)	z-level	0.0135[b]	20 m	20 m	virtual salt flux	
MITgcm-JPL (Nakayama)	z-level	0.0325	20 m	20 m	virtual salt flux	
MOM6-SIGMA-ZSTAR (Marques)	z/s ALE	0.045	20 m	20 m	volume flux (sponge)	
MOM6-LAYER (Marques)	isopycnal	0.1423	Top cell	10 m	volume flux (none)	✓
MPAS-Ocean (Asay-Davis)	z/s ALE	0.0194	Exp. dist.[c]	10 m[d]	volume flux (sponge)	✓
NEMO-CNRS (Jourdain)	z-level	0.026	20 m	20 m	volume flux (open ocean)	✓
NEMO-UKESM1is (Smith)	z-level	0.045	20 m	20 m	volume flux (open ocean)	✓
POP2x (Asay-Davis)	z-level	0.1146	20 m	20 m[d]	virtual salt flux	✓
ROMS (Gwyther)	s-level	0.05	Top cell	Top cell	virtual salt flux	✓

[a] Derived transfer coefficient for Ocean1 simulation is 0.011 and 0.036 for Ocean2, see Sect. 3.3 for more details. [b] Using derived transfer coefficient rather than reported value (Sect. 3.3). [c] See Sect. 3.6. [d] Velocity sampled only in top cell.

We have added the following summary to the beginning of Section 3 to summarise the main differences between models:

> L231 - 233: The vertical coordinate is a key area of model difference – divided into those that use z-level coordinates and those that use other coordinates – and also tends to categorise meltwater distribution and tracer sampling distances (Table 1).

Line 312: “The fluxes are adjusted each month to match the freshwater flux from melting averaged over the previous 3 months.” Please provide some justification for this choice; it was not clear why this was implemented.

Sea level restoring is needed in models that use volume fluxes to add meltwater to avoid sea level rise of hundreds of metres during the simulation period, as mentioned in L133 and Asay-Davis et al. (2016). We have modified the text at L155 to explain this further:

> L155 - 157: To avoid sea level rise over the course of the simulation, sea level may also be restored if melting is implemented as a volume flux using surface mass fluxes in the sponge restoring region; otherwise, there are no open ocean surface fluxes (unless specified in Section 3, see also the Meltwater addition column of Table 2).

For most models with volume fluxes, this is achieved using an evaporative flux at the sponge boundary (Table 2).

For the MPAS-Ocean experiments, we did not wish to add specialized code for maintaining sea level to MPAS-Ocean itself just for the ISOMIP+ experiments, so instead we utilised its evaporative fluxes to maintain sea level. Rather than modifying the evaporative fluxes online, we used a python script to modified them offline (between model restarts) based on melt rates from the model output covering the last 3 simulated months. This didn’t maintain sea level exactly but it was sufficient to prevent dozens of meters of sea-level rise that would have otherwise happened over the coarse of the simulation.

We altered the text as follows:

> L340 - 343: Sea level is approximately maintained, for all but the Ocean0 experiment, by applying negative freshwater fluxes in the northern restoring region. These fluxes are adjusted monthly, using a 3-month running mean of the meltwater flux, which was implemented offline via simple “evaporative” freshwater flux, heat flux and salt flux adjustments rather than modifying MPAS-Ocean code.

- Figure 2: Some of the differences between the temperature distributions are quite subtle. The authors might consider showing a multi-mean and then deviations away from this mean for each model. This might highlight differences between the simulations more clearly. The contours are not described in the caption.

Thank you for the suggestion. Presenting a multi-model mean map is difficult as there are small differences in the boundaries between models due to their vertical coordinate and geometry smoothing, so differences at the boundaries (e.g. ISOBL) may be obscured, and the multi-model mean can obscure step-like features in z-coordinate models. We also do not wish to communicate that the multi-model mean is the “correct” solution. However, we added a description of the contour intervals in the caption.

- Line 395: “where the thermal and haline driving is larger due to the salinity stratification” I did not understand why salinity stratification leads to larger forcing of the ice-shelf. Above, the manuscript discusses how stratification shields the ice shelf from warm water. Do these models capture the effects of entrainment by meltwater plumes?

We removed “haline driving”. And yes, these models should capture entrainment of the ambient ocean by the meltwater (to a certain extent, due to the coarse vertical resolution in many of the models).

- Line 404: “also have cold temperature transects” Can you provide more details here?

Here we are referring to these model configurations not having a warm blob in Ocean2 (as some models do, see Fig. 3). We rewrite this, adding the parentheses, as

> L442 - 445: Other models (e.g. MITgcm-BAS, MOM6-LAYER, NEMO-UKESM1is, POP2x, ROMS) also have cold temperature transects *(i.e. lack the warm temperatures remnant from the spin-down observed in some models, Fig. 3)* and no such freezing, however, the temperature transects do not sample the sidewall region where freezing occurs.

- Line 405: "These variations demonstrate that models can achieve similar cavity-averaged melt rates (the tuning target melt rate below 300m is achieved by all models except ROMS and FVCOM) with very different spatial distributions of melting. These differences in melt rate patterns, particularly near the grounding line and side walls, have implications for ice sheet evolution in coupled ice sheet–ocean models." This is a really interesting result! It would be nice to go into a bit more detail — mechanistically, what is happening at the grounding zone and side walls that are different between models. Also, can you explicitly state why this would matter in more realistic coupled ice sheet-ocean models.

The grounding line and side walls are most likely to be affected by differences between models in topography and ice shelf draft. We have rewritten as

> L446 - 451: These differences in melt rate patterns, *particularly near the grounding line and side walls,* are likely to be at least partly associated with differences in bed topography and ice shelf draft between models (Figs. S11, S12). Differences in spatial distributions of melting have implications for ice sheet evolution in coupled ice sheet–ocean models, *as the ice thickness and therefore the buttressing effect may evolve differently depending on the location of melt.*

- Paragraph starting on line 420: I was somewhat surprised that the barotropic circulation within the cavity was the same magnitude as the overturning circulation. For the continental shelf circulation the barotropic gyres are typically an order of magnitude stronger (see Webber et al. 2019 for example). Is this because the barotropic gyres are weaker in the cavity or is this a result of estimating the overturning circulation in depth coordinates, rather than density coordinates?

Figs. 8, 11 and 13 only show the barotropic streamfunction results within the cavity. Figs. S5, S6 show the gyre circulations outside of the cavity are much stronger (∼1-2 Sv), approximately 5 times larger in magnitude than the barotropic streamfunction in the cavity, which seems to agree, at least to first order, with Webber et al. (2019). The weaker barotropic transport in the cavity is likely related to the ice-front blocking effect (Wåhlin et al., 2020). Unfortunately, we do not have the required model data diagnostics to calculate heat transports for a true comparison. We added the following italicised text:

> L488 - 490: Despite the general similarity in ocean circulation within the ice shelf cavity, the ocean circulation outside the ice shelf cavity shows significant variability among the models, *and is generally stronger in magnitude* (Fig. S5).

- Line 422: "clockwise" — you should also state that these are "cyclonic" gyres.

We have added "cyclonic".

- Line 472: "The area-averaged melt rates over the entire ice shelf for the Ocean1 and Ocean2 experiments demonstrate the dependence of melt rate on temperature (Fig. 11)." The statement should be more precise here — melt rates also depend on the circulation structure.

We have rewritten as

> L532 - 533: The area-averaged melt rates over the entire ice shelf for the Ocean1 and Ocean2 experiments as a function of time demonstrate the dependence of melt rate on ocean conditions and circulation (Fig. 12).

- Figure 7: Why does the overturning circulation extend into the ice shelf in some of these panels?

This is because of "zonal" averaging; near the $y = 0\,\text{km}$ and $y = 80\,\text{km}$ boundaries a smaller ice thickness means x-z transect looks quite different and the ocean is present at different depths to the central transect. We have rewritten the Fig 8 and Fig 10 captions

> Fig 8: Ocean1 COM overturning streamfunctions in depth–x coordinates, averaged over the y direction and over year 20, corresponding to the steady warm state of the cavity. The 0 Sv contour is indicated by a light grey line and the ice front at $x = 640\,\text{km}$ is indicated by a blue line.

as well as added some extra text in the main body after the Ocean1 overturning streamfunction is presented:

> L468 - 470: Varying bed topography and ice geometry in both x and y-direction (Fig. 1) results in the streamfunction, which is computed by averaging velocities over the y-direction, being nonzero in (x, z) coordinates that are not simulated as ocean in the $y = 40\,\text{km}$ transect (e.g. Fig. 2).

- Figure 9: The overturning circulation in the cold regime experiments show a clockwise circulation at depth, supported by ice-shelf melt, and a shallower counter-clockwise circulation. This upper cell must be supported by waters becoming denser in the ice-shelf cavity and it was not clear what process is responsible for this. Is the ocean water freezing back on to the ice shelf in these simulations? This freezing is typically associated with waters becoming supercooled when they rise from depth, but that does not seem to be consistent with the streamfunctions here. Another possibility is that these simulations are not in steady state — in simulations where the density surface are evolving with time, the overturning streamfunction will produce vertical velocities that are not associated with diapycnal transports.

The counterclockwise upper cell in Ocean2 experiments is created by the strong salinity stratification, sponge, and closed domain. The meltwater outflow of the lower cell happens as the meltwater reaches a neutrally buoyant depth, strongly controlled by the salinity stratification. This outflow, and the flow it entrains above and below, is modified at the sponge boundary at $x = 800\,\text{km}$. The sponge boundary is also a wall, and fluid must go somewhere by conservation of volume, so there is a return flow above and below.

Note the same upper cell is seen in models that use the same profile for initial and boundary conditions (e.g. Yung et al. (2025) Fig. 4) so is not likely to be caused by the simulations having not reached equilibrium.

We don't see freezing at these shallower depths associated with this circulation in most models (see melt rate spatial distributions in Fig. 6, also mentioned in the text in L505), suggesting that the upper cell is not controlled by meltwater processes directly: rather it is created by the interaction of the lower cell (which is meltwater-driven), the sponge boundary which sets a strong ambient stratification, and the closed boundaries of the domain.

We have modified the text (also in response to Reviewer 1) to read

> L506 - 510: Note that the simulated shallow counterclockwise circulation is likely related to density gradients in the domain that develop in response to the boundary restoring to the salinity-stratified cold profile, in combination with the steep ice

> base geometry, rather than local buoyancy fluxes (which are small due to the low melting at shallow depths, Fig. 6). The circulation is a feature of our model setup, noting the unrealistically strong stratification and that reversed overturning cells of this extent have not been observed in Antarctic ice shelves (to our knowledge).

- Line 445: "Return flow above and below this depth is created by the modifications made by the restoring forcing to the fluid's buoyancy at the x = 800 km wall boundary." This does not explain what process provides the source of mid-depth outflow from shallower depths (from the upper counter-clockwise cell).

As mentioned, there will be outflow due to buoyancy released by melting at depth in the lower cell, and the depth that this flow settles at will be set by the cavity ambient stratification, which is in turn set by the external ambient stratification in this case by the boundary conditions. At this depth, both upper and lower cells must close. The closure of the upper cell is enabled by conservation of volume, since the boundaries of the domain are all closed walls. We added "and is constrained by conservation of volume" to L500.

- Figure 11: It is nice to show the collapse of the melt rate evolution when normalizing!

Thank you!

- Figure 12: I would suggest removing the COCO simulation from this figure. It is well outside the range of all the other experiments and makes it difficult to assess differences between the other 11 simulations. It would also be helpful to indicate the position of the final state (the time average of the 20th year) in each panel.

We would like to keep the COCO simulation for completeness, but have adjusted the y-axis of the barotropic streamfunction simulations for the other models and will add a note to the caption "In some panels, COCO results fall outside the main range of the other models." We have also added a larger, black-outlined symbol to indicate the average of year 20.

- Line 532: "If the ice shelf cavity flow is driven only by buoyancy, the magnitude of melt would be expected to be proportional to the near-ice velocity, which in turn should approximately scale with overturning circulation strength (e.g. MacAyeal, 1984; Jourdain et al., 2017)." I did not understand this statement. The strength of the overturning is governed by the rate of water mass transformation, which depends on both the magnitude of the buoyancy flux at the ocean-ice shelf interface as well as the density gradient. The velocity field arise to move water across the stratification at a rate to balance the buoyancy-driven transformation. The magnitude of melt would only be proportional to the near-ice velocity if the buoyancy gradient is uniform.

The approximately linear feedback between melt rate and overturning and barotropic streamfunction strength has been shown previously, for example in Jourdain et al. (2017) Fig. 7a,b. We agree it is not perfectly linear, since the stratification and also boundary layer properties respond to melting, but to first order it appears to be a good approximation. We replaced "approximately" with "to first order" and mentioned the impact of meltwater on stratification:

> L592 - 594: If the ice shelf cavity flow is driven only by buoyancy, the magnitude of melt would be expected to be proportional to the near-ice velocity, which in turn should scale *(to first-order)* with overturning circulation strength (e.g. MacAyeal, 1984; Jourdain et al., 2017), *noting that melt rate feedbacks on stratification would modify this relationship.*

- Figure 13: It would be nice to produce this figure as a function of density on the y-axis,

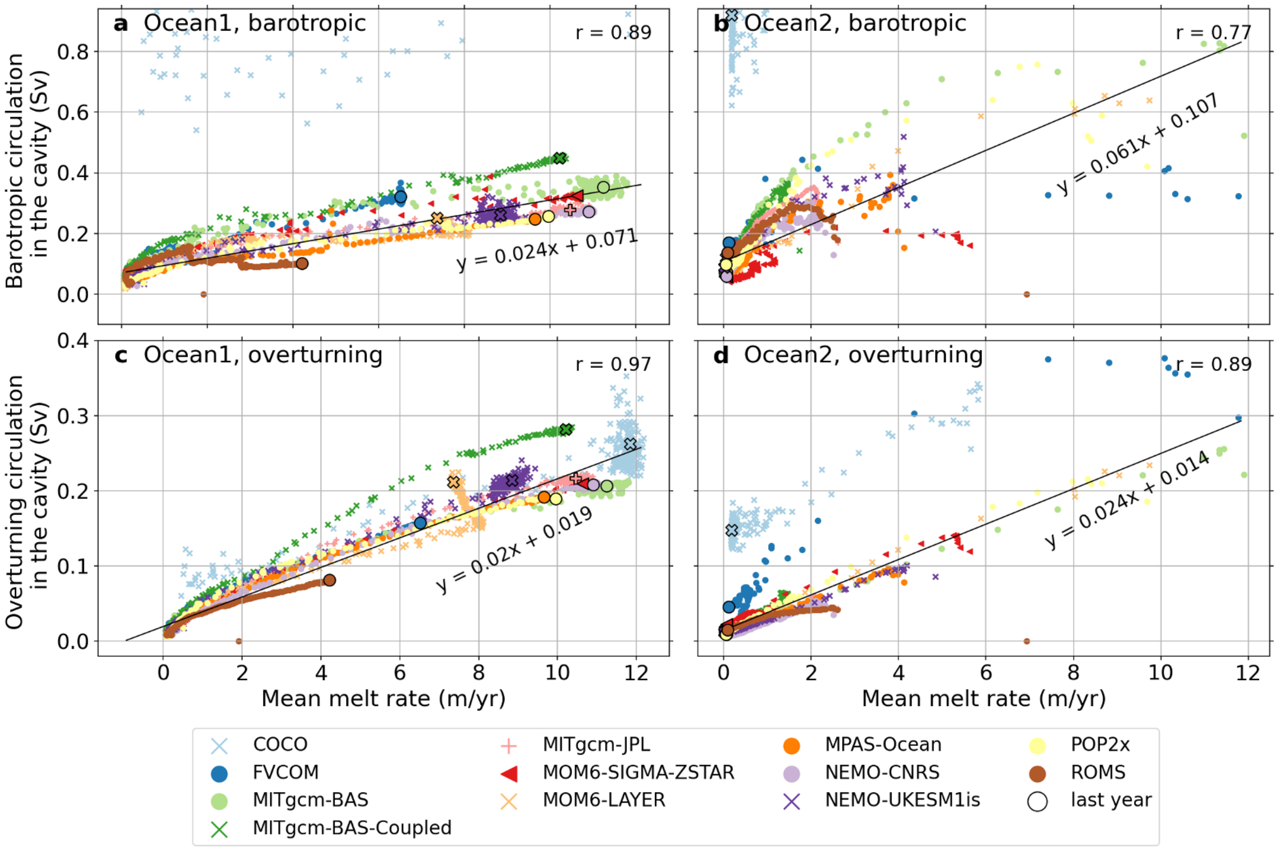

Figure R8: Revised Fig. 12, now 13.

rather than depth. The discussion around Figure 13 would benefit from assessing differences in the stratification between these different models. What does "transect averaged" mean in the caption?

Thanks for the suggestion. However, having the y-axis a function of depth allows us to better highlight the boundary layers of models of different vertical coordinates, which is the main purpose of this figure. Plotting as a function of density might be an idea for future work; we agree that stratification will be important, but note that most models do not resolve boundary layer stratification – melt rate feedbacks due to their sampling of temperature and salinity over a large Losch (2008)-style layer.

We also added a mention of stratification in the boundary layer

> L647 - 648: Meltwater fluxes stably stratify the water column, hence thin, stratified boundary layers have potential feedback effects on melt rate.

"Transect averaged" is supposed to be "transect, averaged ...". We added the missing comma.

- Line 520: "The deviation from the quadratic scaling is consistent with Holland (2017), who show that warm-to-cold transitions (i.e. Ocean2) better match the equilibrium response compared with cold-to-warm transitions (i.e. Ocean1)." This statement was unclear, can you please clarify.

We have added some more detail to this sentence:

> L581 - 583: The deviation from the quadratic scaling is consistent with Holland (2017), who show that warm-to-cold transitions (i.e. Ocean2) better match the

> equilibrium response *(a quadratic melt rate–thermal driving relationship)* compared with cold-to-warm transitions (i.e. Ocean1).

- Line 527: "Additionally, the multi-model mean gradients of the overturning and barotropic streamfunction as a function of melt have the same order of magnitude, indicating barotropic and overturning circulation have similar magnitudes." I was confused by this statement because Figure 12 does not show streamfunction gradients — you might mean change in volume transport with melt rates, but this would have a slope or trend, rather than a magnitude.

Firstly, in case it was confusing, we would like to clarify that "gradients of the overturning and barotropic streamfunction" referred to the gradients of the scatter plot in Fig. 12, rather than spatial gradients of the streamfunction. In this figure, since the two linear fits have intercepts at approximately zero, similar gradients imply similar absolute magnitudes of overturning and barotropic streamfunctions. However, this relationship is already demonstrated in the steady-state plots, so we removed L527 for improved clarity.

- Line 565: "show warmer water at depth" Perhaps be more quantitative here.

Thanks for the suggestion, however, the following sentences provide quantitative information on the depth profiles of temperature, so we feel this opening statement to the paragraph doesn't require extra detail.

- Line 579: "Sharp differences in temperature along the meltwater layer suggest that the z-coordinate resolution is partially responsible." Explain why z-coordinate is responsible — presumably it is because terrain-following coordinates resolve the boundary layer better?

This is because the z-coordinate models have step-like boundaries. We have rewritten as

> L639 - 640: Sharp differences in temperature along the meltwater layer suggest that the z-coordinate resolution and its step-like boundary are partially responsible.

- Line 581: "POP2x also has a very cold boundary layer, likely related to its large melt rate transfer coefficient (Table 2) compared to other z-coordinate models." Shouldn't the melt rate matter more than the value of the transfer coefficient?

Both are related features: since the temperature is cold, a large transfer coefficient is required to obtain the required tuning melt rate. It is likely also related to the sub-freezing point temperatures in Fig. 15. We have rewritten it as

> L633 - 635: POP2x also has a very cold boundary layer, which explains why it requires such a large melt rate transfer coefficient (Table 2) compared to other z-coordinate models to achieve the same target melt rate, and may indicate numerical inaccuracies.

- Line 620: "z-level models are expected to require smaller transfer coefficients to achieve the same tuned melt rate, since the lower vertical resolution near the ice and greater thermal driving sampling and freshwater flux distribution distances result in larger melt rates compared with higher resolution terrain-following configurations (Gwyther et al., 2020)." This sentence was confusing, consider re-phrasing: perhaps "near the ice and greater thermal driving sampling" should be "near the ice implies greater thermal driving sampling"

Thanks for the suggestion, we have rewritten as

> L683 - 687: z-level models are expected to require smaller transfer coefficients to achieve the same tuned melt rate, since the lower vertical resolution near the ice implies greater thermal driving sampling and freshwater flux distribution distances.

> z-level models therefore tend to have larger melt rates compared with higher resolution terrain-following configurations when the same transfer coefficient is used (Gwyther et al., 2020).

- Line 645: "and a significant fraction of the available heat for melting is advected out of the cavity by the buoyancy-driven overturning." This is an example where the manuscript would benefit from a clearer statement of the circulations that are diagnosed. I would have expected the barotropic, or gyre, circulations to carry heat into and back out of the cavity. The buoyancy-driven circulation, on the other hand, would be associated with heat loss (melting) that converts water from one density class to another. However, if density surfaces are tilted, there could also be a sheared, baroclinic, circulation that is also adiabatic and contributes to the inefficiency of the available heat to melt the ice shelf.

Thanks for the comment. Unfortunately we lack the diagnostics to properly separate the diabatic and adiabatic contributions to heat transport. We will replace "by the buoyant-driven overturning" to "by the cavity circulation". We will recommend future intercomparison studies consider adding diagnostics to quantify this in the preamble of Section 4, where we also recommend adding density coordinate streamfunctions

> L458 - 460: Future model intercomparisons should consider including the density-coordinate overturning streamfunction as a required diagnostic, and may also consider diagnosing adiabatic and diabatic contributions to heat transport.

- Line 669: "The T–S space results indicate that water mass analysis is effective for model verification." State why the T-S plots were useful for model verification — verification of what?

As stated earlier in the section, the T–S analyses enabled a problem with the MITgcm advection scheme to be uncovered, as well as unphysical heat loss in POP2x and MOM6-SIGMA-ZSTAR. Verification typically refers to checking that the model numerics are correct and that the model results reflect the physical equations they are supposed to.

- Section 4.5: In a manuscript that is already long, I would suggest removing some of the details of the TYP experiment (or moving it to supplementary) and shifting the focus of this section to specifically addressing what additional insight was gained from these suite of experiments. The last paragraph attempts to provide a summary, but there is no attempt at addressing why these simulations differed (admittedly hard!).

Thank you for the comment. We would like to keep the TYP experiments in the paper as there are still lessons to be learnt from these experiments. Whilst we cannot say too much about individual parameters in the TYP experiments, the main conclusion is that the TYP experiments demonstrate additional sensitivity to model choices on top of differences in the model used (COM). They also show how different the COM parameters are to what is typically used by a variety of model groups (e.g. lower melt rates in almost all the experiments, since most groups did not tune their TYP experiments).

We moved Fig. 17 to the Supplementary to reduce the length of this section, and condensed the remaining text into ∼1 page of text, mostly covering Fig. 16.

We added some text to the Discussion highlighting these learnings for future comparison projects, following additional suggestions from Reviewer 1:

> L841 - 845: Future work could more systematically probe causal links between model choices and consequent model solutions by performing parameter testing experiments or by tuning the TYP experiments to, for example, achieve a certain melt rate (as in COM; only the NEMO and FVCOM models used this approach

> in the TYP experiments presented here, Fig. 17). These experiments would test whether model states can be made more similar if their parameters are freer to vary, noting that results likely still depend on the complexity of the model configuration used.

- The Discussion does a nice job of highlighting key aspects of the model that led to inter-model differences, such as the importance of vertical coordinates and vertical resolution near the ice-ocean boundary. It would be great to have a short paragraph on how this would influence future model development priorities.

Thanks for the suggestion. We have added some text in the Discussion discussing suggestions for future work (see response to Major Comment 1).

Technical comments/suggestions

- Line 50: It would be clearer to break this into two sentences.

We have done this as follows.

> L54 - 58: The domain used in these experiments was designed to be representative of small-sized, laterally confined ice shelves that experience buttressing, such as Pine Island Glacier Ice Shelf. These ice shelves are thought to be particularly vulnerable to rapid retreat and potential contributors to large amounts of sea level rise contained in the grounded ice sheet regions they buttress (Rignot et al., 2014; Favier et al., 2014; Christianson et al., 2016; Reed et al., 2024).

- Line 68: A verb is missing after "Zhou et al. (2024) . . . "

We added "use" here.

- Line 88: "drivers" should be "drivers of melt"

We edited as suggested.

- Line 122: "vary uniformly with depth" I think the authors mean, "vary with depth but hare horizontally uniform."

We replaced with "vary linearly with depth".

- Table 1: ρ_{ref} should be a reference density

We edited the table description to read "Reference density for linear EOS"

- Line 189: "states" should be "ocean states"

We edited as suggested.

- Line 232: "advected" should be "horizontally advected"

We edited as suggested.

- Line 331: "spread vertically" — I assume means "spread uniformly over the upper 20 m"?

Yes, we edited as suggested.

- Line 348: "topography": Here and throughout, please be consistent — "bed topography," "ice shelf topography," "ice draft," and "bathymetry" are all used so sometimes it is not clear what is being referred to.

We removed mentions of “bathymetry” and are now more specific with our language regarding bed topography and ice shelf draft.

- Line 367: Suggest modify “difference and drivers of melt” to “differences of melt along with its drivers”

We have rewritten as “differences in melt between models and their drivers”

- x-label in Figure 11c,d: This could have a clearer description, e.g. “Time since the half-maximum melt rate (yr)”

We edited the figure label as suggested

- Line 513: “melt rates” should be area-averaged melt rates”

We edited as suggested

- Line 567: “Upslope” — suggest “seaward”

Upslope highlights the fact that the boundary layer is tilted, so we prefer to keep it as upslope.

- Line 617: “transfer coefficients” should be “thermal transfer coefficients”

We edited as suggested

- Line 692: Add compared to COM after less in Ocean 2”

We edited as suggested

- Line 723: “similar ocean profiles” should be “similar hydrographic profiles”

We edited as suggested

- Line 791: “tuning and development of the melt parameterisation” You might be clear here whether tuning/development of existing parameterizations is sufficient or whether new parameterizations are required.

We rewrote it as “tuning and development of new melt parameterisations”

References

Asay-Davis, X. S., Cornford, S. L., Durand, G., Galton-Fenzi, B. K., Gladstone, R. M., Gudmundsson, G. H., Hattermann, T., Holland, D. M., Holland, D., Holland, P. R., et al.: Experimental design for three interrelated marine ice sheet and ocean model intercomparison projects: MISMIP v. 3 (MISMIP+), ISOMIP v. 2 (ISOMIP+) and MISOMIP v. 1 (MISOMIP1), Geoscientific Model Development, 9, 2471–2497, https://doi.org/10.5194/gmd-9-2471-2016, 2016.

Bett, D. T., Bradley, A. T., Williams, C. R., Holland, P. R., Arthern, R. J., and Goldberg, D. N.: Coupled ice–ocean interactions during future retreat of West Antarctic ice streams in the Amundsen Sea sector, The Cryosphere, 18, 2653–2675, https://doi.org/10.5194/tc-18-2653-2024, 2024.

Bronselaer, B., Winton, M., Griffies, S. M., Hurlin, W. J., Rodgers, K. B., Sergienko, O. V., Stouffer, R. J., and Russell, J. L.: Change in future climate due to Antarctic meltwater, Nature, 564, 53–58, https://doi.org/10.1038/s41586-018-0712-z, 2018.

Burgard, C., Jourdain, N. C., Reese, R., Jenkins, A., and Mathiot, P.: An assessment of basal melt parameterisations for Antarctic ice shelves, The Cryosphere, 16, 4931–4975, https://doi.org/10.5194/tc-16-4931-2022, 2022.

Christianson, K., Bushuk, M., Dutrieux, P., Parizek, B. R., Joughin, I. R., Alley, R. B., Shean, D. E., Abrahamsen, E. P., Anandakrishnan, S., Heywood, K. J., et al.: Sensitivity of Pine Island Glacier to observed ocean forcing, Geophysical Research Letters, 43, 10–817, https://doi.org/10.1002/2016GL070500., 2016.

Darelius, E., Makinson, K., Daae, K., Fer, I., Holland, P. R., and Nicholls, K. W.: Hydrography and circulation in the Filchner depression, Weddell Sea, Antarctica, Journal of Geophysical Research: Oceans, 119, 5797–5814, https://doi.org/10.1002/2014JC010225, 2014.

De Rydt, J. and Naughten, K.: Geometric amplification and suppression of ice-shelf basal melt in West Antarctica, The Cryosphere, 18, 1863–1888, https://doi.org/10.5194/tc-18-1863-2024, 2024.

De Rydt, J., Jourdain, N. C., Nakayama, Y., van Caspel, M., Timmermann, R., Mathiot, P., Asay-Davis, X. S., Seroussi, H., Dutrieux, P., Galton-Fenzi, B., Holland, D., and Reese, R.: Experimental design for the Marine Ice Sheet–Ocean Model Intercomparison Project – phase 2 (MISOMIP2), Geoscientific Model Development, 17, 7105–7139, https://doi.org/10.5194/gmd-17-7105-2024, 2024.

Favier, L., Durand, G., Cornford, S. L., Gudmundsson, G. H., Gagliardini, O., Gillet-Chaulet, F., Zwinger, T., Payne, A., and Le Brocq, A. M.: Retreat of Pine Island Glacier controlled by marine ice-sheet instability, Nature Climate Change, 4, 117–121, https://doi.org/10.1038/nclimate2094, 2014.

Fogwill, C., Phipps, S., Turney, C., and Golledge, N.: Sensitivity of the Southern Ocean to enhanced regional Antarctic ice sheet meltwater input, Earth's Future, 3, 317–329, https://doi.org/10.1002/2015EF000306, 2015.

Galton-Fenzi, B., Fricker, H. A., Bassis, J. N., Crawford, A. J., Gomez, N., and Schoof, C.: The Antarctic Ice Sheet and sea level: contemporary changes and future projections, in: Antarctica and the Earth System, edited by Meredith, M., Melbourne-Thomas, J., Raphael, M., and Garabato, A. N., Taylor & Francis Group, https://doi.org/10.4324/9781003406471-7, 2025.

Galton-Fenzi, B. K.: Modelling ice-shelf/ocean interaction, Ph.D. thesis, University of Tasmania, https://doi.org/10.25959/23233199.v1, 2009.

Galton-Fenzi, B. K., Porter-Smith, R., Cook, S., Cougnon, E., Gwyther, D. E., Huneke, W. G. C., Rosevear, M. G., Asay-Davis, X., Boeira Dias, F., Dinniman, M. S., Holland, D., Kusahara, K., Naughten, K. A., Nicholls, K. W., Pelletier, C., Richter, O., Seroussi, H. L., and Timmermann, R.: Realistic ice-shelf/ocean state estimates (RISE) of Antarctic basal melting and drivers, The Cryosphere, 2025, 1–27, https://doi.org/10.5194/egusphere-2024-4047, in press.

Goldberg, D., Snow, K., Holland, P., Jordan, J., Campin, J.-M., Heimbach, P., Arthern, R., and Jenkins, A.: Representing grounding line migration in synchronous coupling between a marine ice sheet model and a z-coordinate ocean model, Ocean Modelling, 125, 45–60, https://doi.org/10.1016/j.ocemod.2018.03.005, 2018.

Golledge, N. R., Keller, E. D., Gomez, N., Naughten, K. A., Bernales, J., Trusel, L. D., and Edwards, T. L.: Global environmental consequences of twenty-first-century ice-sheet melt, Nature, 566, 65–72, https://doi.org/10.1038/s41586-019-0889-9, 2019.

Grosfeld, K., Gerdes, R., and Determann, J.: Thermohaline circulation and interaction between ice shelf cavities and the adjacent open ocean, Journal of Geophysical Research: Oceans, 102, 15 595–15 610, https://doi.org/10.1029/97JC00891, 1997.

Gwyther, D. E., Galton-Fenzi, B. K., Dinniman, M. S., Roberts, J. L., and Hunter, J. R.: The Effect of Basal Friction on Melting and Freezing in Ice Shelf–Ocean Models, Ocean Modelling, 95, 38–52, https://doi.org/10.1016/j.ocemod.2015.09.004, 2015.

Gwyther, D. E., Cougnon, E. A., Galton-Fenzi, B. K., Roberts, J. L., Hunter, J. R., and Dinniman, M. S.: Modelling the response of ice shelf basal melting to different ocean cavity environmental regimes, Annals of Glaciology, 57, 131–141, https://doi.org/10.1017/aog.2016.31, 2016.

Gwyther, D. E., Kusahara, K., Asay-Davis, X. S., Dinniman, M. S., and Galton-Fenzi, B. K.: Vertical processes and resolution impact ice shelf basal melting: A multi-model study, Ocean Modelling, 147, 101 569, https://doi.org/10.1016/j.ocemod.2020.101569, 2020.

Hattermann, T., Nicholls, K. W., Hellmer, H. H., Davis, P. E., Janout, M. A., Østerhus, S., Schlosser, E., Rohardt, G., and Kanzow, T.: Observed interannual changes beneath Filchner-Ronne Ice Shelf linked to large-scale atmospheric circulation, Nature Communications, 12, 2961, https://doi.org/10.1038/s41467-021-23131-x, 2021.

Holland, D. M. and Jenkins, A.: Modeling thermodynamic ice–ocean interactions at the base of an ice shelf, Journal of physical oceanography, 29, 1787–1800, https://doi.org/10.1175/1520-0485(1999)029⟨1787:MTIOIA⟩2.0.CO;2, 1999.

Holland, P. R.: The transient response of ice shelf melting to ocean change, Journal of Physical Oceanography, 47, 2101–2114, https://doi.org/10.1175/JPO-D-17-0071.1, 2017.

Holland, P. R., Jenkins, A., and Holland, D. M.: The response of ice shelf basal melting to variations in ocean temperature, Journal of Climate, 21, 2558–2572, https://doi.org/10.1175/2007JCLI1909.1, 2008.

Hunter, J.: Specification for test models of ice shelf cavities, in: Technical Report June, Antarctic Climate & Ecosystems Cooperative Research Centre, 2006.

IPCC: Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change, Cambridge University Press, Cambridge, UK and New York, NY, USA, https://doi.org/10.1017/9781009157896, 2021.

Jacobs, S. S., Helmer, H., Doake, C. S., Jenkins, A., and Frolich, R. M.: Melting of ice shelves and the mass balance of Antarctica, Journal of Glaciology, 38, 375–387, https://doi.org/10.3189/S0022143000002252, 1992.

Janout, M. A., Hellmer, H. H., Hattermann, T., Huhn, O., Sültenfuss, J., Østerhus, S., Stulic, L., Ryan, S., Schröder, M., and Kanzow, T.: FRIS revisited in 2018: On the circulation and water masses at the Filchner and Ronne ice shelves in the southern Weddell Sea, Journal of Geophysical Research: Oceans, 126, e2021JC017 269, https://doi.org/10.1029/2021JC017269, 2021.

Jenkins, A., Nicholls, K. W., and Corr, H. F.: Observation and parameterization of ablation at the base of Ronne Ice Shelf, Antarctica, Journal of Physical Oceanography, 40, 2298–2312, https://doi.org/10.1175/2010JPO4317.1, 2010.

Jourdain, N. C., Mathiot, P., Merino, N., Durand, G., Le Sommer, J., Spence, P., Dutrieux, P., and Madec, G.: Ocean circulation and sea-ice thinning induced by melting ice shelves in the

Amundsen Sea, Journal of Geophysical Research: Oceans, 122, 2550–2573, https://doi.org/10.1002/2016JC012509, 2017.

Jourdain, N. C., Asay-Davis, X., Hattermann, T., Straneo, F., Seroussi, H., Little, C. M., and Nowicki, S.: A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections, The Cryosphere, 14, 3111–3134, https://doi.org/10.5194/tc-14-3111-2020, 2020.

Jourdain, N. C., Mathiot, P., Burgard, C., Caillet, J., and Kittel, C.: Ice shelf basal melt rates in the Amundsen Sea at the end of the 21st century, Geophysical Research Letters, 49, e2022GL100 629, 2022.

Kusahara, K., Tatebe, H., Hajima, T., Saito, F., and Kawamiya, M.: Antarctic sea ice holds the fate of antarctic ice-shelf basal melting in a warming climate, Journal of Climate, 36, 713–743, https://doi.org/10.1175/JCLI-D-22-0079.1, 2023.

Losch, M.: Modeling ice shelf cavities in a z coordinate ocean general circulation model, Journal of Geophysical Research: Oceans, 113, https://doi.org/10.1029/2007JC004368, 2008.

MacAyeal, D. R.: Thermohaline circulation below the Ross Ice Shelf: A consequence of tidally induced vertical mixing and basal melting, Journal of Geophysical Research: Oceans, 89, 597–606, https://doi.org/10.1029/JC089iC01p00597, 1984.

Mathiot, P. and Jourdain, N. C.: Southern Ocean warming and Antarctic ice shelf melting in conditions plausible by late 23rd century in a high-end scenario, Ocean Science, 19, 1595–1615, https://doi.org/10.5194/os-19-1595-2023, 2023.

Mathiot, P., Jenkins, A., Harris, C., and Madec, G.: Explicit representation and parametrised impacts of under ice shelf seas in the z coordinate ocean model NEMO 3.6, Geoscientific Model Development, 10, 2849–2874, https://doi.org/10.5194/gmd-10-2849-2017, 2017.

Naughten, K. A., Meissner, K. J., Galton-Fenzi, B. K., England, M. H., Timmermann, R., and Hellmer, H. H.: Future projections of Antarctic ice shelf melting based on CMIP5 scenarios, Journal of Climate, 31, 5243–5261, https://doi.org/10.1175/JCLI-D-17-0854.1, 2018a.

Naughten, K. A., Meissner, K. J., Galton-Fenzi, B. K., England, M. H., Timmermann, R., Hellmer, H. H., Hattermann, T., and Debernard, J. B.: Intercomparison of Antarctic ice-shelf, ocean, and sea-ice interactions simulated by MetROMS-iceshelf and FESOM 1.4, Geoscientific Model Development, 11, 1257–1292, https://doi.org/10.5194/gmd-11-1257-2018, 2018b.

Naughten, K. A., Holland, P. R., and De Rydt, J.: Unavoidable future increase in West Antarctic ice-shelf melting over the twenty-first century, Nature Climate Change, 13, 1222–1228, https://doi.org/10.1038/s41558-023-01818-x, 2023.

Nicholls, K. W., Makinson, K., and Østerhus, S.: Circulation and water masses beneath the northern Ronne Ice Shelf, Antarctica, Journal of Geophysical Research: Oceans, 109, https://doi.org/10.1029/2004JC002302, 2004.

Orsi, A. H. and Wiederwohl, C. L.: A recount of Ross Sea waters, Deep Sea Research Part II: Topical Studies in Oceanography, 56, 778–795, https://doi.org/10.1016/j.dsr2.2008.10.033, 2009.

Reed, B., Green, J. M., Jenkins, A., and Gudmundsson, G. H.: Recent irreversible retreat phase of Pine Island Glacier, Nature Climate Change, 14, 75–81, https://doi.org/10.1038/s41558-023-01887-y, 2024.

Richter, O., Gwyther, D. E., Galton-Fenzi, B. K., and Naughten, K. A.: The Whole Antarctic Ocean Model (WAOM V1. 0): Development and Evaluation, Geoscientific Model Development, 15, 617–647, https://doi.org/10.5194/gmd-15-617-2022, 2022.

Rignot, E., Mouginot, J., Morlighem, M., Seroussi, H., and Scheuchl, B.: Widespread, rapid grounding line retreat of Pine Island, Thwaites, Smith, and Kohler glaciers, West Antarctica, from 1992 to 2011, Geophysical Research Letters, 41, 3502–3509, https://doi.org/10.1002/2014GL060140, 2014.

Scott, W. I., Kramer, S. C., Holland, P. R., Nicholls, K. W., Siegert, M. J., and Piggott, M. D.: Towards a fully unstructured ocean model for ice shelf cavity environments: Model development and verification using the Firedrake finite element framework, Ocean Modelling, 182, 102 178, https://doi.org/10.1016/j.ocemod.2023.102178, 2023.

Seroussi, H., Nowicki, S., Payne, A. J., Goelzer, H., Lipscomb, W. H., Abe-Ouchi, A., Agosta, C., Albrecht, T., Asay-Davis, X., Barthel, A., Calov, R., Cullather, R., Dumas, C., Galton-Fenzi, B. K., Gladstone, R., Golledge, N. R., Gregory, J. M., Greve, R., Hattermann, T., Hoffman, M. J., Humbert, A., Huybrechts, P., Jourdain, N. C., Kleiner, T., Larour, E., Leguy, G. R., Lowry, D. P., Little, C. M., Morlighem, M., Pattyn, F., Pelle, T., Price, S. F., Quiquet, A., Reese, R., Schlegel, N.-J., Shepherd, A., Simon, E., Smith, R. S., Straneo, F., Sun, S., Trusel, L. D., Van Breedam, J., van de Wal, R. S. W., Winkelmann, R., Zhao, C., Zhang, T., and Zwinger, T.: ISMIP6 Antarctica: a multi-model ensemble of the Antarctic ice sheet evolution over the 21st century, The Cryosphere, 14, 3033–3070, https://doi.org/10.5194/tc-14-3033-2020, 2020.

Siahaan, A., Smith, R. S., Holland, P. R., Jenkins, A., Gregory, J. M., Lee, V., Mathiot, P., Payne, A. J., Ridley, J. K., and Jones, C. G.: The Antarctic contribution to 21st-century sea-level rise predicted by the UK Earth System Model with an interactive ice sheet, The Cryosphere, 16, 4053–4086, https://doi.org/10.5194/tc-16-4053-2022, 2022.

Timmermann, R. and Hellmer, H. H.: Southern Ocean warming and increased ice shelf basal melting in the twenty-first and twenty-second centuries based on coupled ice-ocean finite-element modelling, Ocean Dynamics, 63, 1011–1026, https://doi.org/10.1007/s10236-013-0642-0, 2013.

Wåhlin, A. K., Steiger, N., Darelius, E., Assmann, K. M., Glessmer, M. S., Ha, H. K., Herraiz-Borreguero, L., Heuzé, C., Jenkins, A., Kim, T.-W., et al.: Ice front blocking of ocean heat transport to an Antarctic ice shelf, Nature, 578, 568–571, https://doi.org/10.1038/s41586-020-2014-5, 2020.

Webber, B. G., Heywood, K. J., Stevens, D. P., and Assmann, K. M.: The impact of overturning and horizontal circulation in Pine Island Trough on ice shelf melt in the eastern Amundsen Sea, Journal of Physical Oceanography, 49, 63–83, https://doi.org/10.1175/JPO-D-17-0213.1, 2019.

Yung, C. K., Rosevear, M. G., Morrison, A. K., Hogg, A. M., and Nakayama, Y.: Stratified suppression of turbulence in an ice shelf
basal melt parameterisation, The Cryosphere, 19, 5827–5861, https://doi.org/10.5194/tc-19-5827-2025, 2025.

Zhao, C., Gladstone, R., Galton-Fenzi, B. K., Gwyther, D., and Hattermann, T.: Evaluation of an emergent feature of sub-shelf melt oscillations from an idealized coupled ice sheet–ocean model using FISOC (v1. 1)–ROMSIceShelf (v1. 0)–Elmer/Ice (v9. 0), Geoscientific Model Development, 15, 5421–5439, https://doi.org/10.5194/gmd-15-5421-2022, 2022.

Zhou, Q. and Hattermann, T.: Modeling ice shelf cavities in the unstructured-grid, Finite Volume Community Ocean Model: Implementation and effects of resolving small-scale topography, Ocean Modelling, 146, 101 536, https://doi.org/10.1016/j.ocemod.2019.101536, 2020.