Response to Referee Comments

May 7, 2025

The document is color coded in the following way:
Referee comments are in **blue**.
Our answers are in **black**.
Citations from the revised manuscript are in **red**.

Anonymous Referee #1

The authors have mostly done a good job at responding to my review. I now have one major comment, and a few more minor ones:

We thank the reviewer for their thorough review, and address the comments below:

Major comment:

I was surprised to see the authors' analysis that the CICE6 flux calculation itself was the cause of the coastal freshening in ROMS, not changes in ice growth or melt. Later in their response the authors say they have not looked into the MetROMS coupling calculations to see if they should be updated. I think this should be an urgent priority, as something like a subtle unit change (e.g. scaled by ice concentration or not) could have a large effect over time. Best practice is to review the coupling any time there is a major version change in a component, and I recommend this is checked before the manuscript goes any further.

The earlier answer might have been misleading. When we said that *"We have not really looked into how the conversion of CICE's freshwater and salinity flux is handled when converted to the ROMS's salinity flux."*, we meant that we have not tried to re-tune this, but we have checked that nothing should have changed regarding the CICE outputs according to CICE documentation.

If the coupling is correct, a brief discussion of the implications would be warranted: given that the community has spent many years tuning ocean models coupled to CICE5, some re-tuning will be necessary in the future to deal with the new flux behaviour in CICE6, and of course this is not trivial.

We have re-examined our coupling code and could not find any errors there. However, upon further examination of the CICE code, we found a bug regarding the fluxes to the ocean from frazil ice formation in the Icepack submodule (version Icepack 1.3.1) used by CICE 6.3.1, released 02/2022. According to the Icepack Github issue #390 https://github.com/CICE-Consortium/Icepack/issues/390, this bug was found in May 2022 and fixed by the end of 2023.

Based on this information, we plan to address this problem in the next version of the MetROMS-UHel model. We consider that trying to resolve the problem now is out of the scope of this manuscript. CICE 6.3.1 and MetROMS-UHel 1.0 have already been used in other research, and we consider that an evaluation of the current version of the model is still valuable and useful for the community.

We have added a mention of this bug in the metadata of the released model code in zenodo:

Icepack 1.3.1 in CICE 6.3.1 has a bug when using ktherm = 2 and update_ocn_f = True, affecting the salinity flux from frazil ice formation (`https://github.com/CICE-Consortium/Icepack/issues/390`). We suggest taking this into account before using this version of the model.

And discussed this problem in the revised manuscript on rows 484 to 487:

The problem seems to originate from a bug in the Icepack 1.3.1 code (submodule of CICE 6.3.1), regarding calculations of salinity fluxes from frazil ice formation (CICE-Consortium, 2022), which is strongest at the coast. The bug has been resolved in later versions of CICE, and we plan to address this issue in the next version of MetROMS-UHel.

Minor comments:

Figure B5: Even in the initial state, the AABW cell is extremely weak and does not extend as deep as we believe it should (suggesting the dense water is not cascading all the way down the slope) - compare for example to Figure 2a of Zhang et al. 2016 (doi:10.1002/2016JC011790). The authors should note this initial bias in the text. I am also struggling to see how this cell is stronger in the CICE6 runs, as the authors claim - it's maybe a touch stronger in the upper ocean but if anything weaker at depth. I also don't see how the CICE6 runs experience more weakening than CICE5 in the anomaly panels - weakening would show up as positive anomalies, but (at least at depth) these seem more positive for CICE5. Maybe a different colour scale/contours, plotting in density space, or highlighting the regions of interest would help?

Thank you for this comment. We note the initially weak AABW cell in the text and have rewritten the text related to Figure B5 (lines 442–446). Essentially, we admit that there are no systematic differences in the initial AABW cells, and since they are already quite weak from the beginning, the CICE6 AABW cells can not weaken further despite the freshening of coastal waters.

Table 2: Why is everything compared to Rignot 2013 with the + and -, including Adusumilli 2020, which are also observations (and more up to date)? I think it would be more correct to compare only the modelled melt rate to the full range of both datasets (Adusumulli would need some kind of uncertainty quantification, even just a standard deviation over time would help), and consider it biased (with + or -) only if it falls outside the range of both. This takes into account uncertainty in observational estimates, and does not favour one dataset over the other without justification.

This was done as we did not have comparable error estimates for the Susheel Adusumilli et al. (2020). Upon the reviewers request, we have now instead used the melt water flux estimates from 1994–2018 Adusumilli et al. (2020) supplementary Table 1 for the 25 ice shelves used in the comparison (`https://static-content.springer.com/esm/art%3A10.1038%2Fs41561-020-0616-z/MediaObjects/41561_2020_616_MOESM1_ESM.pdf`). The values for the melt in the supplementary table differ somewhat from those acquired from the Susheel Adusumilli et al. (2020), mostly in the Australian sector, and probably due to how the edges of the shelves are defined, but not enough to affect the conclusions drawn in our study.

Table 2 and the corresponding text in Lines 530 to 557 have been updated with these new observational values.

Lines 74-79: How are the CICE6 updates not "climate changing" if they affect fluxes to the ocean, and have been shown in this and other studies to change the behaviour of the coupled ice-ocean system? Does this phrase only consider ice-atmosphere fluxes?

The CICE developers have stated that the update from CICE5 to CICE6 should not be "climate changing" in standalone mode, i.e. when not coupled to an ocean model. This statement does not take the fluxes into account. The meaning of the phrase "climate changing" is described in Roberts et al. (2018), where they state that:

"Understanding whether or not non-BFB changes in CICE code may also alter the climate of the model can be non-trivial. By 'climate changing', we mean significant changes in sea-ice thickness, h, over a substantial fraction of the ice pack within a defined number of annual cycles. h integrates changes in sea-ice growth, melt, drift and deformation, and therefore the time series hi of ice thickness, weighted by ice concentration, documents evolution of simulated ice mass and underpins our quality control (QC) procedure (i is a time index)."

We have made this clearer by adding the following sentence to the lines 77 to 79:

The developer defines 'climate changing' as 'significant changes in sea-ice thickness, h, over a substantial fraction of the ice pack within a defined number of annual cycles' (Roberts et al., 2018).

Anonymous Referee #2

General comments

I thank the authors for all their efforts in response to my comments, especially in adding more details in several places on how specific changes in the forcing and ice model code lead to differences in the model output (new section 4.1.4 and sections 4.2.1, and 4.3). I still think it would have been useful to compare the ice production to observational estimates, especially since the authors have now added a figure (Fig. 7) showing the model ice growth, but I can see their point that "it would require considerable work to be addressed properly" (reply to reviewers) and am happy that at least the model production is now added.

I think this manuscript does meet its objective of explaining how the upgrades impact the model simulation and this will be quite useful to some who study the Southern Ocean community. I have several new specific comments and suggestions below, but all of these are pretty minor, and I do not think this manuscript needs very much work before it is suitable for publication.

We are happy that the reviewer is satisfied with our efforts. We address all the minor specific comments below:

Specific comments

Line 12: Since there are areas where the modeled melt rates are greater than observations (e.g. Amery), I suggest changing "melt rates are underestimated" to something like "melt rates are generally underestimated".

Thank you for the comment, this has been changed as suggested.

Lines 37-38: Since satellite ice concentration has been available in the Antarctic for quite a while, I suggest a slight modification of "estimates of sea ice properties" to something like "estimates of sea ice properties beyond ice concentration".

We have updated these lines as suggested.

Line 76: I don't understand what is meant by 'climate changing' when referring to model code updates. Do the authors mean the changes do not impact mean climate states simulated by the model?

The 'climate changing' is citing the developers of CICE, who in Roberts et al. (2018) define the term as:

"Understanding whether or not non-BFB changes in CICE code may also alter the climate of the model can be non-trivial. By 'climate changing', we mean significant changes in sea-ice thickness, h, over a substantial fraction of the ice pack within a defined number of annual cycles. h integrates changes in sea-ice growth, melt, drift and deformation, and therefore the time series hi of ice thickness, weighted by ice concentration, documents evolution of simulated ice mass and underpins our quality control (QC) procedure (i is a time index)."

We have made this clearer by adding the following sentence to the Lines 77 to 79:

The developer defines 'climate changing' as 'significant changes in sea-ice thickness, h, over a substantial fraction of the ice pack within a defined number of annual cycles' (Roberts et al., 2018).

Line 134: See comments below on Appendix A, but I'm not sure "Similar decreasing trends" is accurate as the decreases here are significantly stronger than in Naughten et al. 2018b.

We have updated Lines 135 to 137 to be more accurate:

The deep ocean does not reach equilibrium and some model drift can be seen in the interior ocean, for example, in the ACC transport, measured at the Drake Passage (Appendix A), which decreases. A similar, but less negative trend of the ACC was found in the MetROMS-Iceshelf runs by Naughten et al. (2018).

Line 405: What is actually increasing in “except for an increase in both updates”?

This means that the sea ice velocities at the coast increase in both updates. Thank you for pointing out the error. This sentence now reads:

Updating from CICE5 to CICE6 or replacing ERAI forcing with ERA5 does not result in substantial changes, except for an increase of ice velocities near the coast in both updates, with the largest increase in the C6E5 run (Fig. 8).

Figure 10 caption: Suggest changing “from psu” to “from model output (psu)”.

This is a good suggestion, and we have updated it as suggested.

Line 434: ISW is generated under the ice shelves, but can be found well outside the ice shelf cavities. Suggest a slight rewrite of “because it is located under the ice shelves where the high pressure lowers the freezing point” to something like “because it is generated at the base of ice shelves where the freezing point of seawater is below that at the surface due to depression of the freezing point with increased pressure”.

Thank you for the suggestion. This sentence has been changed to:

This water mass can be this cold without freezing because it is generated at the base of ice shelves, where the high pressure due to depth lowers the freezing point.

Lines 439-446 and Figure B5: The text says B5 shows the streamfunction north of 60S between 3000 and 5000m, but the figure itself has axes labels for 80S to 40S and 5000m to the surface. Probably more important, the big differences in the streamfunction (assuming the x-axis label is correct) are near the model northern boundary. Because of this, and knowing that the model ACC is slowing down for some reason that may indicate significant changes in Southern Ocean interior watermasses (maybe spurious diapycnal mixing in the interior, as mentioned on lines 521-522 and Naughten et al., 2018b), I would be wary in attributing the differences in the streamfunction to differences in dense water export from the Antarctic continental shelf. I know the streamfunction was computed in direct response to a review comment about examining changes in the shelf water export, but it might be better to just examine the overturning right near Antarctica instead of over the entire model domain.

Thank you for pointing out this streamfunction issue. We replotted the streamfunctions in the updated Figure B5, where their latitudinal extent is limited between 80° S and 60° S to better see their features and differences close to the continental shelf. We have rewritten the text related to Figure B5 (lines 442–446).

Line 539: Suggest specifying that it’s the melt rates that reach equilibrium in 1996 (i.e. “Our model run basal melt rates” instead of “Our model runs”).

Thank you, we have updated this as suggested

Line 543: Suggest changing “decrease in the flux” to “decrease in the melt”.

Updated according to suggestion

Line 556: Suggest changing “speculates that the underestimation of the Australian region is due” to something like “also underestimated melt in the Australian region and speculate that it is due”.

Good suggestion. We have updated this according to the reviewers comment.

Table 2: Should the errors across the individual ice shelves in Rignot et al. be summed in quadrature to give the error for a region instead of just a simple sum (e.g. for the Australian section 198.3 +/- 20 ($sqrt(10^2 + 15^2 + 8^2 + 3^2$) instead of +/- 36 (10+15+8+3))?

Yes, they should. Thank you for pointing out the error. This has been fixed, and Table 2 and the corresponding text have also been updated following the comment from Reviewer 1.

Line 569: Suggest changing "under the Bellingshausen" to "in the Bellingshausen".

Changed as suggested

Appendix A: I know a possible cause for this trend in the ACC transport is discussed in lines 521-522, but do the authors want to add something here? Also, the decreases in all four simulations presented here are much greater than the 0.28 Sv/yr in MetROMS found in Naughten et al. 2018b. Do the authors have any speculation as to why these models have a greater decline?

We do not have any good speculation on why these models have a greater decline. We were surprised to discover it, as the two large changes that we know of between Naughten's model runs and ours are that our model runs do not have a sponge layer and the use of different rheology in CICE, with Naughten et al. (2018) using EAP and us using EVP. We do not see how these differences would result in a greater decline. There might be some other differences we are not aware of, as we do not have available the exact run setup used by Naughten, and can not duplicate her runs because of this.

We would prefer not to add anything more to the manuscript here, as a more thorough speculation would require further analysis into the reasons for the decline.

0.1 Technical corrections

Thank you for the thorough list of technical comments.

Line 107: "Ice Sheets" should be "Ice Shelves".

Fixed.

Line 145: I think "helps resolving" should be "helps resolve" or "helps with resolving".

Fixed.

Line 148: I think "as tides are not accounted for" can be removed from the sentence.

Fixed.

Lines 195-196: Subject verb agreement "differences . . . is".

You are correct. Replaced with "differences ... are"

Line 248: I think "Such pattern" should be "Such a pattern".

Fixed.

Line 251: Typo, "C5E6" should be "C6E5".

Fixed.

Line 375: Typo, "Fig. 3 and 3" should be "Fig. 3 and 6".

You are correct. Fixed.

Line 426: "resulting significant" should be "resulting in significant".

Fixed.

Line 480: "11i" should be "11k".

Fixed.

Line 515: "12e" should be "12f".

Fixed

Line 541: Subject verb agreement "loss . . . are".

Fixed to "loss ... is".

Line 578: Typo, “inthe” should be “in the”.

Fixed.

Line 582: Typo, “asses” should be “assess”.

Fixed.

Line 606: Typo, “arebased” should be “are based”.

Fixed.

Appendix B: I think units need to be added to the table.

Units have been added to table caption.

Figure B1 caption: “it’s” should be “its”.

Fixed.

References

Adusumilli, S., Fricker, H. A., Medley, B., Padman, L., and Siegfried, M. R.: Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves, Nature Geoscience, 13, 616–620, https://doi.org/10.1038/s41561-020-0616-z, publisher: Nature Publishing Group, 2020.

CICE-Consortium: update_ocn_f problem · Issue #390 · CICE-Consortium/Icepack, `https://github.com/CICE-Consortium/Icepack/issues/390`, 2022.

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, publisher: Copernicus GmbH, 2018.

Roberts, A. F., Hunke, E. C., Allard, R., Bailey, D. A., Craig, A. P., Lemieux, J.-F., and Turner, M. D.: Quality control for community-based sea-ice model development, Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 376, 20170 344, https://doi.org/10.1098/rsta.2017.0344, publisher: Royal Society, 2018.

Susheel Adusumilli, Fricker, H. A., Medley, B. C., Padman, L., and Siegfried, M. R.: Data from: Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves. UC San Diego Library Digital Collections., https://doi.org/https://doi.org/10.6075/J04Q7SHT, 2020.