Response to Referee Comments

March 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

"Impacts of CICE sea ice model and ERA atmosphere on an Antarctic MetROMS ocean model, MetROMS-UHel-v1.0" by Äijälä and colleagues evaluates a circumpolar Southern Ocean model including sea ice and ice shelf cavities. Compared to a previously published version of the model, both the atmospheric forcing and sea ice component have been updated. The impacts of each change in isolation are evaluated, as well as both changes together. Evaluation focuses on sea ice properties, with ocean hydrography and ice shelf melting given a secondary focus. In general the updates to the model are beneficial for the representation of sea ice, but problematic for ocean hydrography, particularly a freshening of the continental shelves.

General comments:

In general this is a solid and thorough evaluation paper including some clever analysis of sea ice model performance. I believe it is suitable for GMD after moderate revisions.

We would like to thank the reviewer for their helpful and constructive comments. We address your inquiries below in the best way possible.

In particular, there are a few missing pieces which would help to fill in the oceanographic gaps in the story:

1. Some quantification of ACC strength, ideally a figure showing transport through Drake Passage. The text mentions it slows down, which is concerning, but does not quantify either the initial bias or the degree of drift.

Thank you for the suggestion, we agree with Reviewer #1 that the decreasing trend of the Drake Passage Transport (DPT) is concerning. An assessment of the DPT has been added to the manuscript in Appendix A, including Fig. 1 (Fig. A1 in the revised manuscript) showing the DPT for all model runs including the linear trends. In the Appendix, on lines 605–617, a brief description of the DPT measurements and the model output results regarding ACC strength have been included.

2. A figure of the overturning streamfunction in each model configuration. I suspect that the coastal freshening induced by the update to CICE6 is shutting down dense water export from the shelf. Quantifying the strength of the lower cell would confirm this.

We have added a figure of the overturning streamfunction from 1992, 2005 and 2018 (Fig. 2) to the Appendix B as Figure B5 and added a paragraph discussing this figure to Sect. 4.2.1 lines 439–446.

A possible consequence of this salinity bias is that the freshening of coastal waters induced by the update to CICE6 disrupts the export of dense water from the shelf, potentially shutting it down. Figure B5 presents the streamfunction values north of 60° S, between 3000 and 5000 m depth. The lower

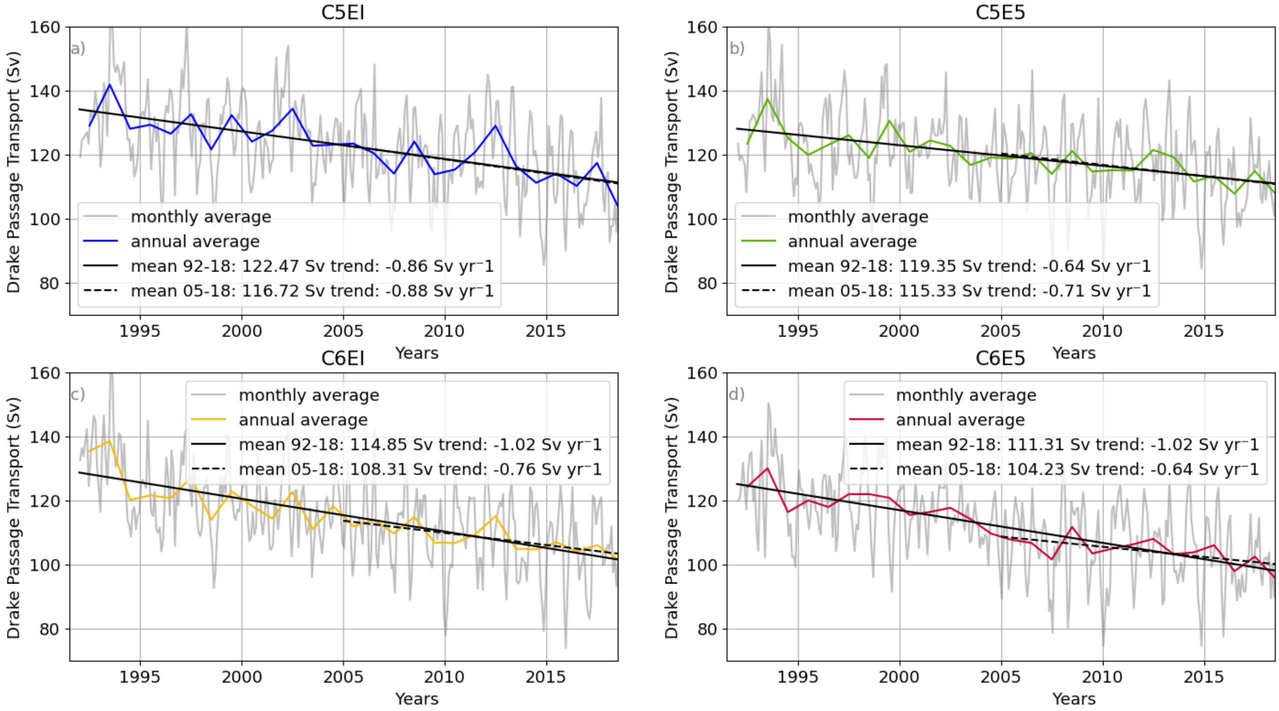

Figure 1: Fig. A1 in the manuscript: Drake Passage Transport (DPT) for all model runs. Gray lines show monthly averages, while the coloured lines show the annual averages for each model. Linear fits for the whole period (1992–2018) are shown in solid black lines; linear fits for the last half of the analysed period (2005–2018) are shown in black dashed lines

overturning cell, identified by negative values south of 50° S near Antarctica, initially exhibits a higher intensity in CICE6 runs compared to CICE5 in January 1992 (Fig. B5a, d, g, j). From 1992 onward, this cell weakens across all simulations, indicating a slowdown in dense water export. Between 1992 and 2005, the weakening is significantly more pronounced in CICE6 than in CICE5, as evidenced by the stronger anomaly in the C6EI/C6E5 2005–1992 difference (Fig. B5h, k) compared to C5EI/C5E5 (Fig. B5b, e). This trend persists in the 2018–1992 comparison (Fig. B5c, f, i, l), suggesting a lasting impact of the CICE6 update on deep water export.

3. Some analysis of the sea ice formation rates in each experiment, as this process is crucial for setting the shelf water masses.

Thank you for the suggestions, we plotted both seasonal and annual growth and melt. The annual average growth and melt, as well as the difference of these, can be seen in Fig. 3, and it has been added as Fig. 7 to the manuscript. The figure results are discussed in a new subsection (Sect. 4.1.4), lines 341–379.

4. A figure (bar chart?) showing ice shelf mass loss integrated over each main ice shelf or region, compared to observations. This would make it much easier to gauge the regional dependence in model bias. From the existing figures it is very difficult to parse the magnitude of the bias or model sensitivity in the small, high-melt cavities in the Amundsen Sea, as well as the large, cold cavities which experience both melting and refreezing.

We have added Table 1 to Sect. 4.3 and Fig. 4 to Appendix B of the revised manuscript as Table 2 and Fig. B7, respectively. We use the same regional division as Naughten et al. (2018b), which defines and analyses eight regions comprising 25 ice shelves. For observations, we present values derived from both Susheel Adusumilli et al. (2020) and Rignot et al. (2013). We have reworked much of the Sect. 4.3, lines 532–580, to incorporate this table and answer specific questions of both reviewers.

The manuscript also errs too much on the side of simple description, with some missed opportunities

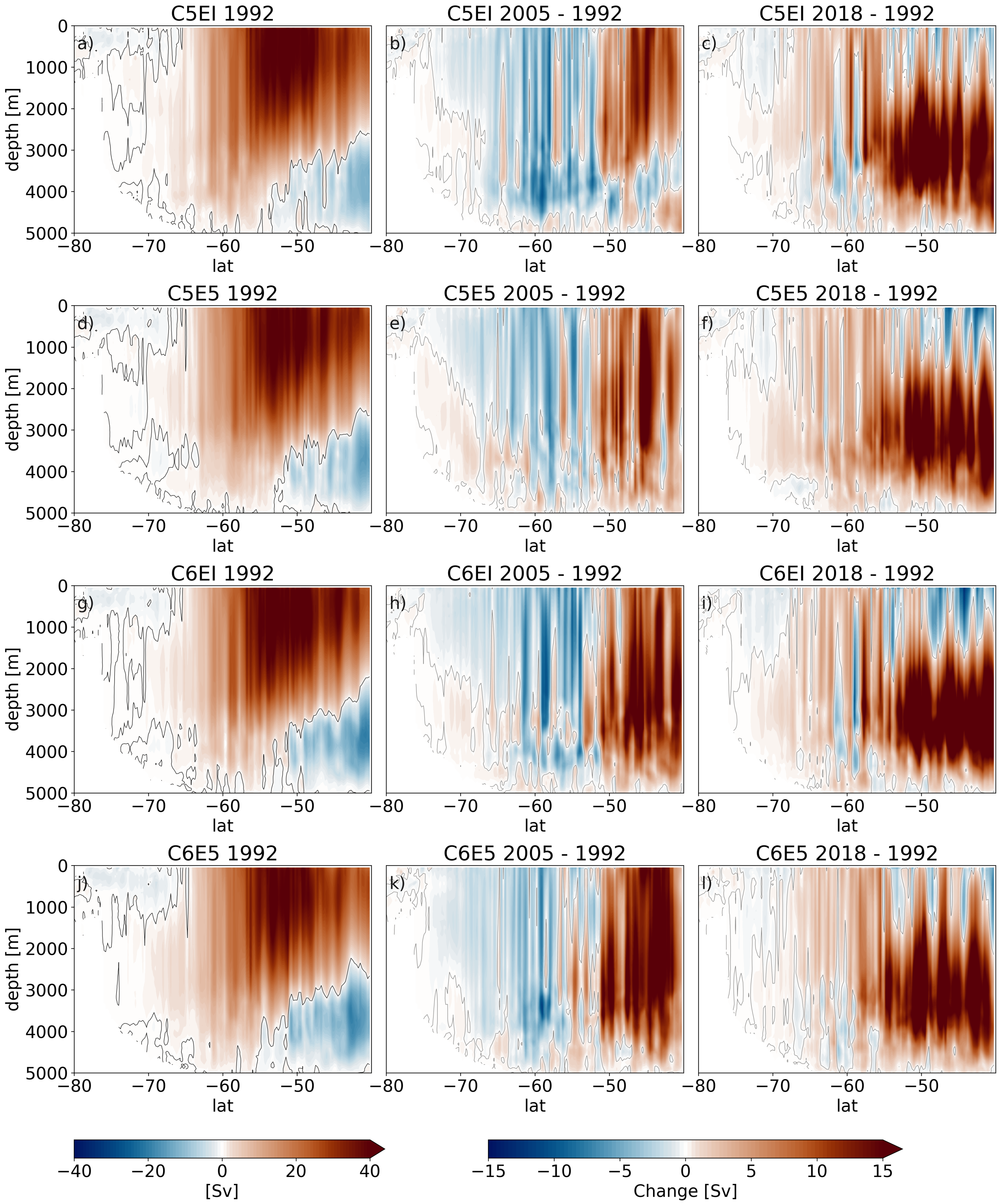

Figure 2: Fig. B5 in the manuscript: Overturning streamfunction for January 1992 (a,d,g,j), and change from January 1992 to January 2005 (b,e,h,k) and 2018 (c,f,i,l) for all model runs.

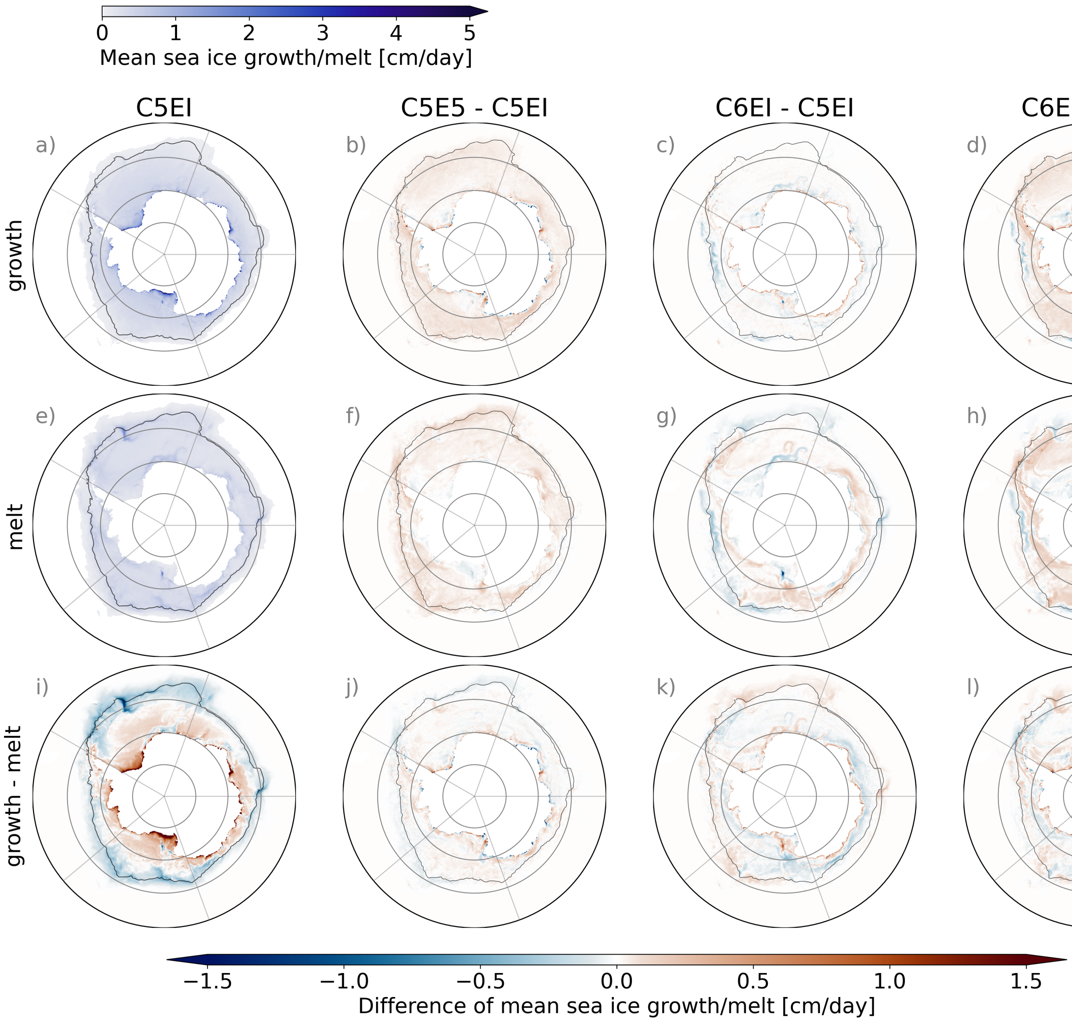

Figure 3: Fig. 7 in the manuscript: Mean sea ice growth (frazil ice growth+congelation ice growth+snow ice formation) and melt (top ice melt + basal ice melt + lateral ice melt) for C5EI (a, e) and difference between the other models and C5EI (b–c, f–h) during 1992–2018. The last row (i–l) shows the difference between the growth and melt above, for C5EI (i) and the model differences (j–k).

for attribution (i.e. which physical processes are behind the model bias visualised?) Of course it would be excessive to fully analyse the causes of all model biases in one manuscript, but at least some discussion of the likely possibilities is warranted. In my specific comments below I have highlighted examples where this is particularly needed.

Thank you, we agree, please see our response to the specific comments below.

Specific Comments:

Line 5: It's worth mentioning more explicitly in the abstract that ice shelf cavities are included, as this is not generally true of Southern Ocean models.

We have added this mention to the abstract, in Line 6–7:

The two versions of MetROMS evaluated in this study use a version of the regional ROMS ocean model including ice shelf cavities.

Region	C5EI	C5E5	C6EI	C6E5	Adusumilli et al.	Rignot et al.
Filchner-Ronne	64.8 (-)	66.7 (-)	61.8 (-)	63.5 (-)	65.5 (-)	155.4 ± 45
Eastern Weddell region	85.3	70.9	94.1	83.8	154.3 (+)	66.3 ± 55
Amery	92.7 (+)	90.4 (+)	104.6 (+)	95.5 (+)	50.5	35.5 ± 23
Australian sector	33.9 (-)	30.9 (-)	36.8 (-)	34.4 (-)	114.8 (-)	198.3 ± 36
Ross Sea	79.7	71.3	78.0	69.2	98.8	70.1 ± 39
Amundsen Sea	83.7 (-)	82.1 (-)	105.6 (-)	95.3 (-)	307.6 (-)	388.8 ± 33
Bellingshausen Sea	66.1 (-)	61.5 (-)	59.7 (-)	50.4 (-)	169.1	187.2 ± 59
Larsen Ice Shelves	20.7	17.4	11.4	9.5	100.5	22.1 ± 81
Total Antarctica	577.1 (-)	537.4 (-)	599.0 (-)	545.1 (-)	1209.9	1325.0 ± 235

Table 1: Table 2 in the manuscript: The average ice-shelf basal mass loss (Gt yr^{-1}) for Antarctica is divided into 8 regions, following Naughten et al. (2018b). These regions encompass 25 ice shelves, as shown in Fig. B7. Model runs represent 1996–2018 averages, while Susheel Adusumilli et al. (2020) dataset provides an average melt rate for 2010–2018. The melt rate is compared to Rignot et al. (2013) as acquired from Naughten et al. (2018b). The $(-)/(+)$ notation indicates values falling outside the range provided by Rignot et al. (2013). Notably, the 2010–2018 estimate of the 1209.9 Gt yr^{-1} estimated by Susheel Adusumilli et al. (2020) differs slightly from the steady state value of 1100±60 Gt yr^{-1} for 1994–2018 reported by Adusumilli et al. (2020).

Line 37: You could make the argument that modelling Antarctic sea ice is also challenging because historically, most development and tuning has been focused on Arctic conditions, which are not entirely transferable to Antarctica.

Thank you, this is a good argument. We have updated the text in Lines 47–48:

Model development and tuning have historically been focused on Arctic sea ice, and such efforts are not entirely transferable to Antarctica, making the modeling of Antarctic sea ice more challenging.

Line 68: What specific changes have been made to the CICE physics parameterisations? If we knew which processes in particular had been updated or added, it could make attribution of the model behaviour easier later. Were any of the bug fixes critical?

The transition from CICE5.1.2 to CICE6.0 was a major refactoring of the code and a few bug fixes. However, with the introduction of the quality control test (QC) as documented in Roberts et al. (2018) these bug fixes and refactoring were not considered 'climate changing', but for example a change in the salinity and fresh water flux calculations states in the release notes (Elizabeth Hunke, 2018) that it might change results in ocean coupled simulations. Additional changes from CICE6.0 to CICE6.3 were also not climate changing and most of these impacted the non-default physics, such as the floe size distribution.

This is now mentioned shortly in the text at Lines 74–79, that now reads:

CICE has been completely reworked between versions 5 and 6, with major restructuring and refactoring of the code, updated physics parametrization and bug fixes. These changes were not considered 'climate changing' in standalone mode, and most changes affected non-default physics. CICE6 has been shown to improve the results of Arctic sea ice compared to CICE5 and CICE4 in the standalone mode (Wang et al., 2020). However, some code changes, such as the ones related to salinity and fresh water flux calculations, might affect ocean coupled simulations.

Line 71: Naughten et al. 2018, which the baseline MetROMS-Iceshelf simulation seems to follow, used elastic-anisotropic-plastic rheology rather than EVP. What was the rationale for changing this and what was the impact? Are there any other major changes from the original model? I note that the total ice shelf basal mass loss was higher in Naughten et al. than any of the four simulations presented here, and I'm curious as to why.

Based on a visual inspection of a short test simulation, we concluded that the EVP rheology more

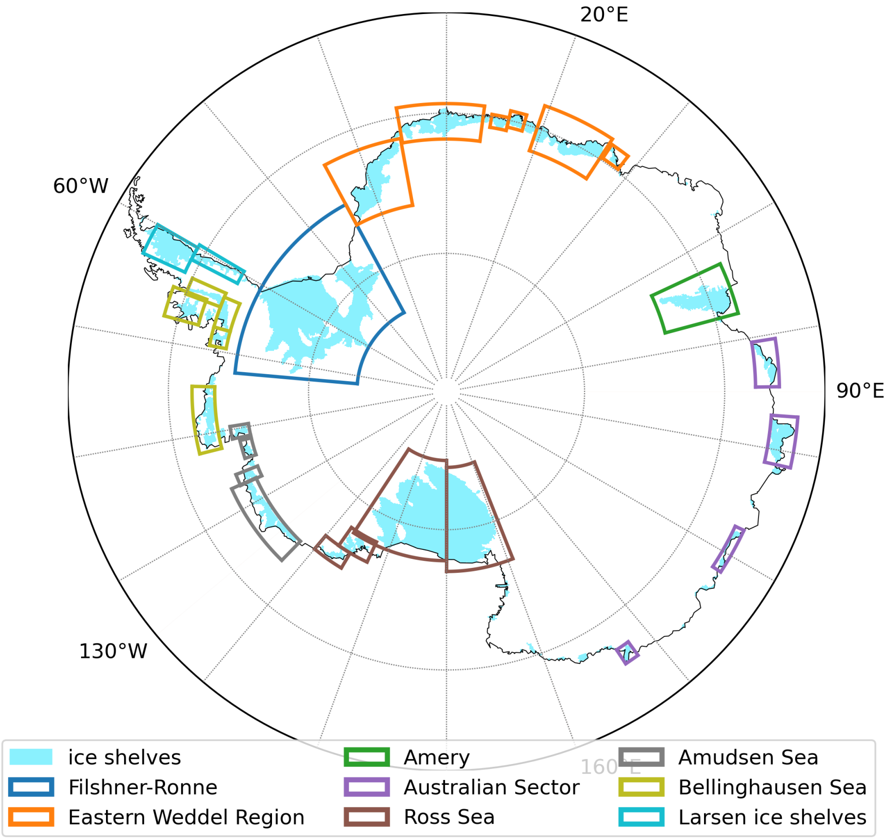

Figure 4: Fig. B7 in the manuscript:Figure of the bounding boxes used for the ice shelves for the comparison in Table 2

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realistically reproduced sea ice dynamics than the EAP rheology. Moreover, EVP is the CICE6 default rheology.

When starting this work, we acquired a version of the circumpolar setup for MetROMS-Iceshelf from the University of Tasmania. We are not sure if this is exactly the same version as that of Naughten et al. (2018b). To our knowledge, other significant differences do not exist. However, minor differences between the MetROMS-Iceshelf and our simulations include different values of the CICE parameter "the e-folding scale of ridged ice", for which we use the default value of `mu_rdg=3`, instead of `mu_rdg=5` as in the MetROMS-Iceshelf. A short MetROMS-Iceshelf spinup simulation, which was not included in Naughten et al. (2018b), had a sponge layer at the northern boundary, that we did not have.

We agree that looking into the reasons of the differences in the basal mass loss would be interesting. We have, however, not looked into why Naughten et al. (2018b) has a higher basal mass loss, and we consider such assessment outside the scope of this work.

Figure 1b: The two different blue shadings are hard to distinguish, could you use two more different colours?

Thank you for the suggestion. We have updated the figure as shown here (Fig. 5).

Line 128: As with CICE6, the improvements made in ERA5 are summarised as “development in model physics”, which could mean any number of things. Which specific processes have been improved?

The ERA reanalyses uses the 4D-Var data assimilation system IFS (Integrated Forecasting System). The model version of ERA-Interim is 31r2 from 2006 while ERA5 uses 41r2 from 2016, so the modelling system has seen multiple improvements over ten years. In the documentation of physical processes of IFS Cycle 41r2 (ECMWF, 2016) almost all subsections report changes since 2006. For example, in the Radiation scheme, the cloud scheme and the surface parameterisation have changed. Additionally, the

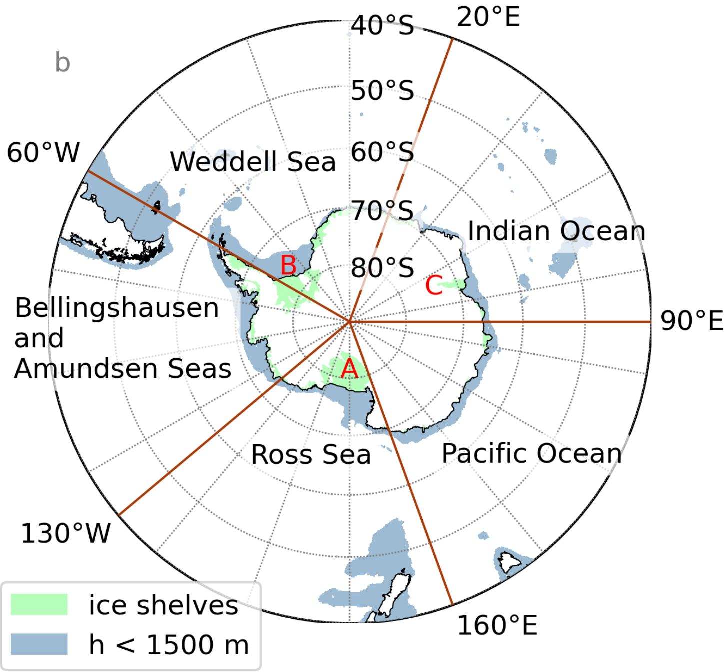


Figure 5: Colours have been updated in Fig. 1b of the manuscript. Labels A, B, and C indicate the Ross, Filchner-Ronne, and Amery Ice Shelves, respectively.

horizontal resolution between ERA-Interim and ERA5 has increased from around 80 km to 31 km, the data assimilation has been improved remarkably, and ERA5 includes a 10-member ensemble providing uncertainty estimates (Hans Hersbach et al., 2019).

Line 137–140 of the manuscript have been updated to summarise this information:

ERA5 is a considerable upgrade from ERAI with a finer horizontal resolution of 31 km compared to the ERAI 80 km. The underlying IFC (Integrated Forecasting System) modelling system has seen multiple improvements over ten years, with development in model physics, numerics, and data assimilation (Hersbach and de Rosnay, 2018).

Line 134: Note that this salinity restoration is only at the surface.

We have added a mention that this is the surface to make it clearer in the manuscript on line 149–150:

The same salinity restoration scheme is applied to the surface of ROMS in all the model runs, as previously used in MetROMS-Iceshelf by Naughten et al. (2018b), where it was used to prevent deep convection in the Weddell Sea.

Line 139: Why is the salinity restoring necessary? It's an unfortunate limitation on the model and the sort of experiments for which it is suitable. Given the updates to ERA and CICE, have you tried switching it off?

Thank you for the question. According to Naughten et al. (2018b) the salinity restoring was needed to prevent deep convection in the Weddell Sea in MetROMS-Iceshelf, so we decided to keep it on for all model simulations. We have not tried switching off the salinity restoration scheme. Future plans include trying to switch it off or changing it for MetROMS-UHel to take into account the sea ice, but this work is outside of the scope of the current manuscript.

This has been clarified in lines 149–150 cited above.

Line 139: ABW should be AABW.

Corrected, thank you.

Line 141: Why is there zero zonal velocity at the northern boundary? This seems equivalent to a no-slip condition, i.e. treating 30S as a solid wall. I wonder if this is contributing to slowing down the ACC, as noted later. Or, is it sufficiently clear of the northern front of the ACC?

Here we just tried to follow what was made in the previous MetROMS-Iceshelf setup in the input files we acquired. Naughten et al. (2017) states the choice was made to "prevent waveguide artifacts". We have not performed tests about this, and chose to follow the original configuration.

Line 185: Are the biases in spring (too much ice offshore and too little close to the coast) linked to the strong sea ice drift shown later (i.e. excessive export of ice)? Or are they the beginnings of the summertime low bias, driven by thermodynamic melting?

We think that the excessive export plays a major role, but the effect of thermodynamics also plays a secondary role. The effects of dynamic and thermodynamic processes are often interlinked and hard to separate. However, looking at the spring sea ice concentration in Fig. 2 of the manuscript, and the sea ice drift from winter in Fig. 8) (Fig. 8 in the revised manuscript), we can see that the drift usually points to the areas of overestimated sea ice concentration. Additionally, from Fig. 6 we can see that, in spring, the pack ice area is underestimated, while the marginal ice zone is overestimated. The sea ice is therefore too spread out, probably due to the excess transport.

We have added a short mention on this link to the drift chapter Sect. 4.1.5, lines 397–398:

The observed large speeds could explain the biases seen in the sea ice concentration in spring (OND), discussed in Sect. 4.1.1.

Line 198: Why might the Pacific and Indian Ocean sectors show the largest underestimation? They are quite a different regime to the other sectors, oceanographically.

We think this might have to do with the ocean hydrography, especially in the Pacific sector, where the warm CDW comes further south in the model runs than where we see it in EN4, and for the bottom of the shelf in the Schmidtko et al. (2014) comparison in Fig. B6.

The updated sentence in Lines 213–215 now reads:

Throughout the year, the largest underestimations happen in the Pacific and Indian Ocean sectors, where especially in the Pacific sector the shelf water seems to be too warm (Fig. B6), and the warm CDW seems to get too far south (not shown).

Figure 4: It would be interesting to fit linear trends to the observations and models, perhaps piecewise breaking around 2014.

Thank you for the suggestion. Fig. 7 shows the time series of sea ice area including linear trends. However, in order to avoid adding a crowded figure to the manuscript, we have decided to include Table 2 (as Table B1 in the revised manuscript) with the means, trends and correlation coefficients (as suggested by Referee 2). Corresponding text referring to the figure has been updated to discuss these values in lines 241–253.

Line 310: Do the CICE6 simulations generally have younger sea ice (there should be an age tracer to analyse)? This would make sense, together with less ridging, thinner coastal ice, stronger export, and possibly stronger sea ice formation (see my general comment).

Unfortunately, the ice age tracer was not saved as an output in the model simulations, so we are not able to answer this question.

Figure 7: It's really hard to see the differences between simulations. Could you plot anomalies of the ice speed without the vectors, perhaps in supplementary?

We have updated the figure (Fig. 8) with differences between simulations as Fig. 8 in the revised manuscript, and added a reference to it in the text.

Line 338: I don't understand what is meant by "simple ocean boundary". Do you mean the sea ice-ocean interface? What is simple about it?

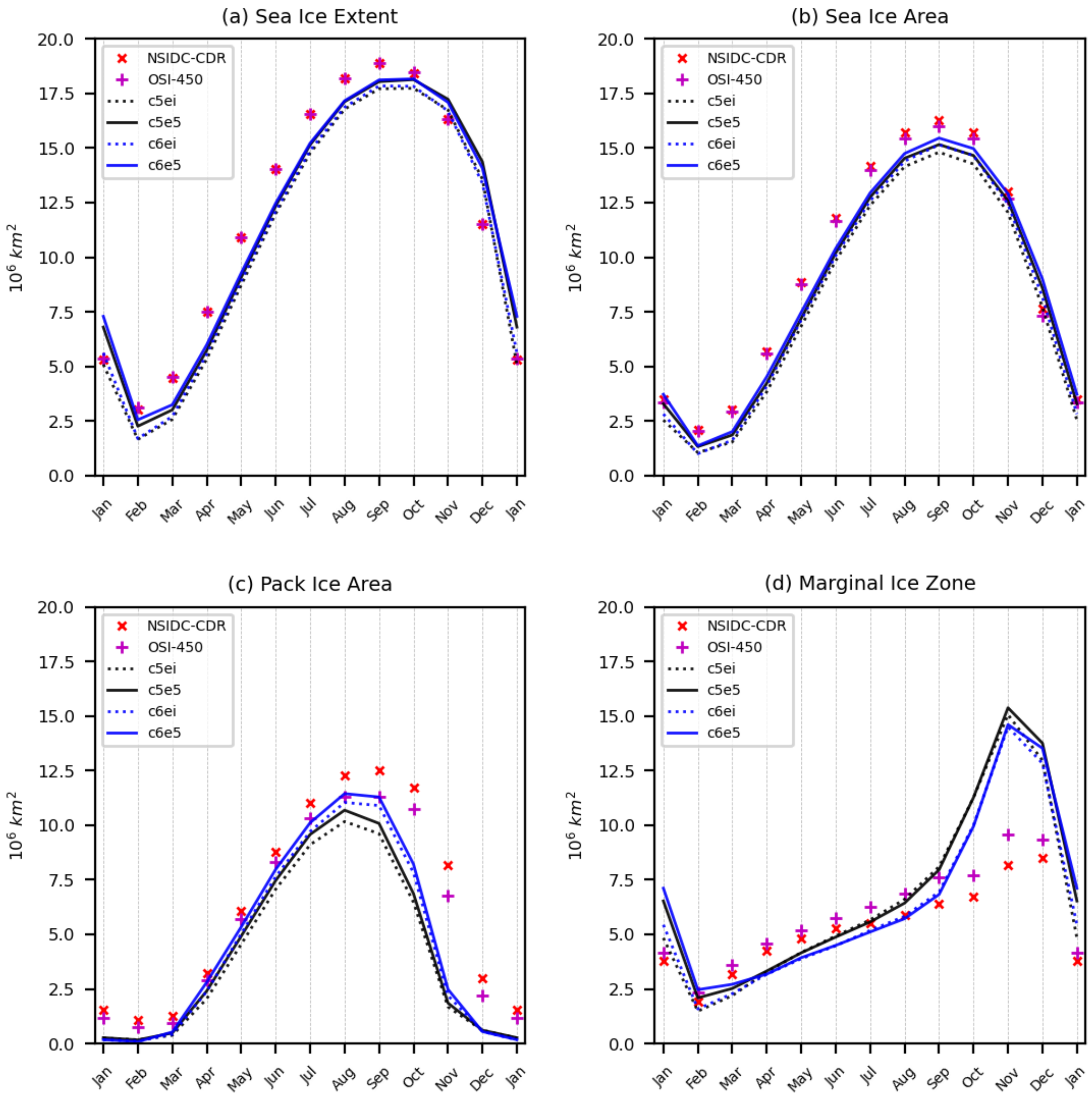


Figure 6: Seasonal cycles of a) sea ice extent, b) sea ice area, c) back ice area and d) marginal ice zone

Here we refer to the boundary between land and ocean, not ice and ocean. As our resolution at the coast on the continental shelf is 8–10 km resolution, with some smoothing of the topography and land/sea mask, the model lacks features like grounded icebergs and other coastal features that in real life would anchor the sea ice, decreasing the amount of drift. We have striven to make this clearer in the text (Lines 399–404):

Furthermore, the relatively low resolution of the ocean-land boundary at the coast and the lack of grounded icebergs could also contribute to the overestimation. A higher resolution ocean-land boundary including icebergs would, potentially, cause slower average motion of the sea ice and longer surviving ice in summer (Naughten et al., 2018b). The low resolution ocean-land boundary might also be a reason for the underestimation of the sea ice, especially in the summer, as it has been shown that sea ice transport is an important process during melt season (Goosse et al., 2023).

Line 358: How appropriate is a comparison to EN4 on the continental shelf (let alone the missing ice shelf cavities)? Does it include enough reliable observations on the shelf?

We acknowledge that the EN4 is based on sparse data in the Antarctic ocean. However, we decided that sparse data to compare to is better than no data at all. The average temperature and salinity error standard deviations and observational weights can be seen in Fig. 9. The lack of sea ice cavities in EN4

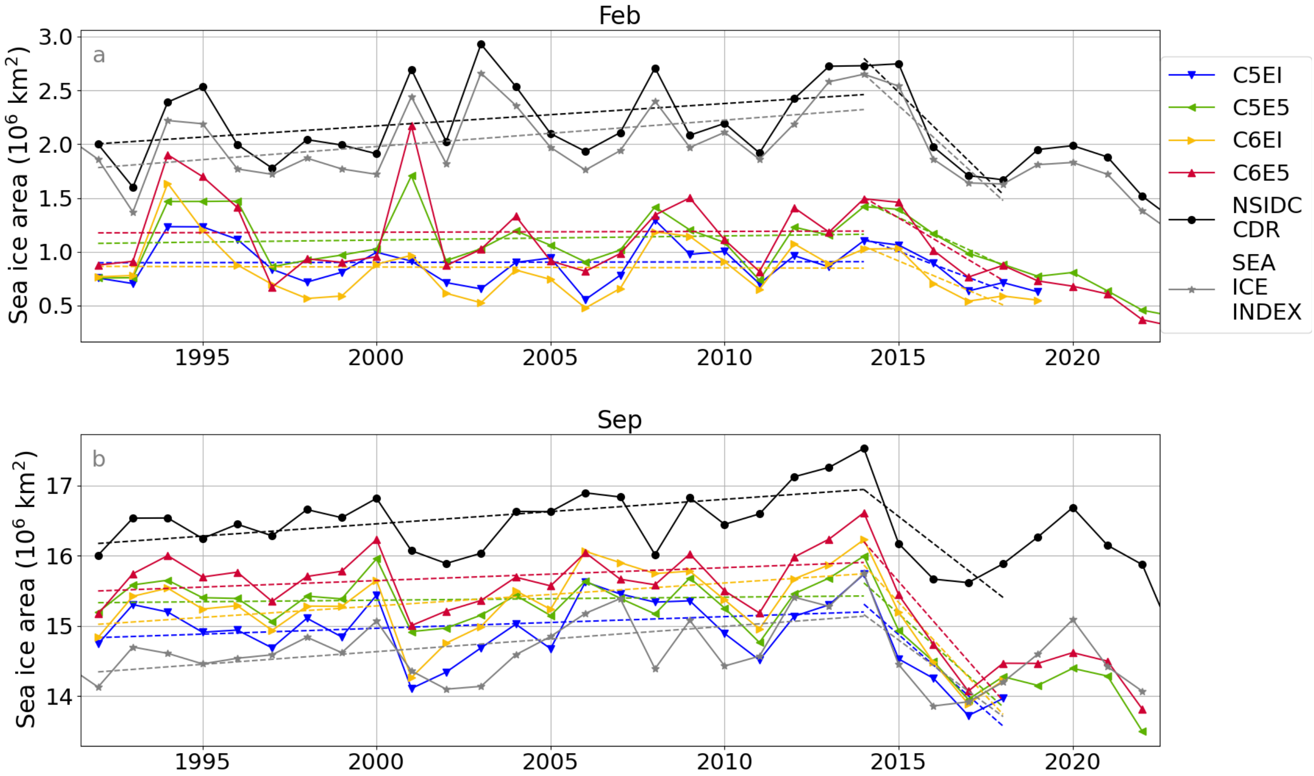


Figure 7: Timeseries over 1992–2022 of (a) February and (b) September monthly average sea ice area [10^6 km^2], with 1992–2014 and 2014–2018 trend lines for the four different model runs (blue, green, yellow, red) and Sea Ice Index (grey), as well as NSIDC CDR sea ice concentration (black). The mean and slope of the trend lines can be found in Table 2

has been taken into account, for example, in Fig. 10 of the manuscript, where the MetROMS zonal means are calculated on the same grid, leaving out the ice shelf cavities.

We have made this clearer in lines 425–427:

It is important to keep in mind, when interpreting the results, that observational data from the Southern Ocean are sparse resulting significant uncertainties in the EN4 data.

Figure 9: This figure is uncomfortably similar to Figure 4 of Naughten et al. 2018, down to the placement of the labels and the nonlinear colour bar. Was the same code used? If so, no attribution is given. I am not sure of the journal's policies on this.

The code is based on the code for Fig. 4 of (Naughten et al., 2018a), but is not identical. Attribution to this has been given in the code in Äijälä and Nie (2024), and we have added a clearer mention of attribution to the metadata of the Zenodo publication as well as to the acknowledgments of the manuscript.

Line 370: The freshening of HSSW in CICE6 is concerning. Hopefully some further tuning of the new sea ice model parameters could alleviate this. Why do you think it occurred? Presumably there's been a decrease in ice formation and/or a local increase in melting — either way, what could cause this? I don't understand how this agrees with the other sea ice variables suggesting higher formation (see my above comment on line 310) or deeper mixed layers in the key formation regions (analysed later).

We agree that this is concerning and hope that tuning the sea ice parameters or modifying the surface salinity restoration will alleviate this in future efforts.

We made a shorter test run with C5E5 and C6E5, where we saved as output the CICE and ROMS parameters for freshwater flux and salinity flux, which are passed to ROMS, and used in ROMS to calculate the salinity flux. We looked into the first year of the spinup, where the sea ice have had the

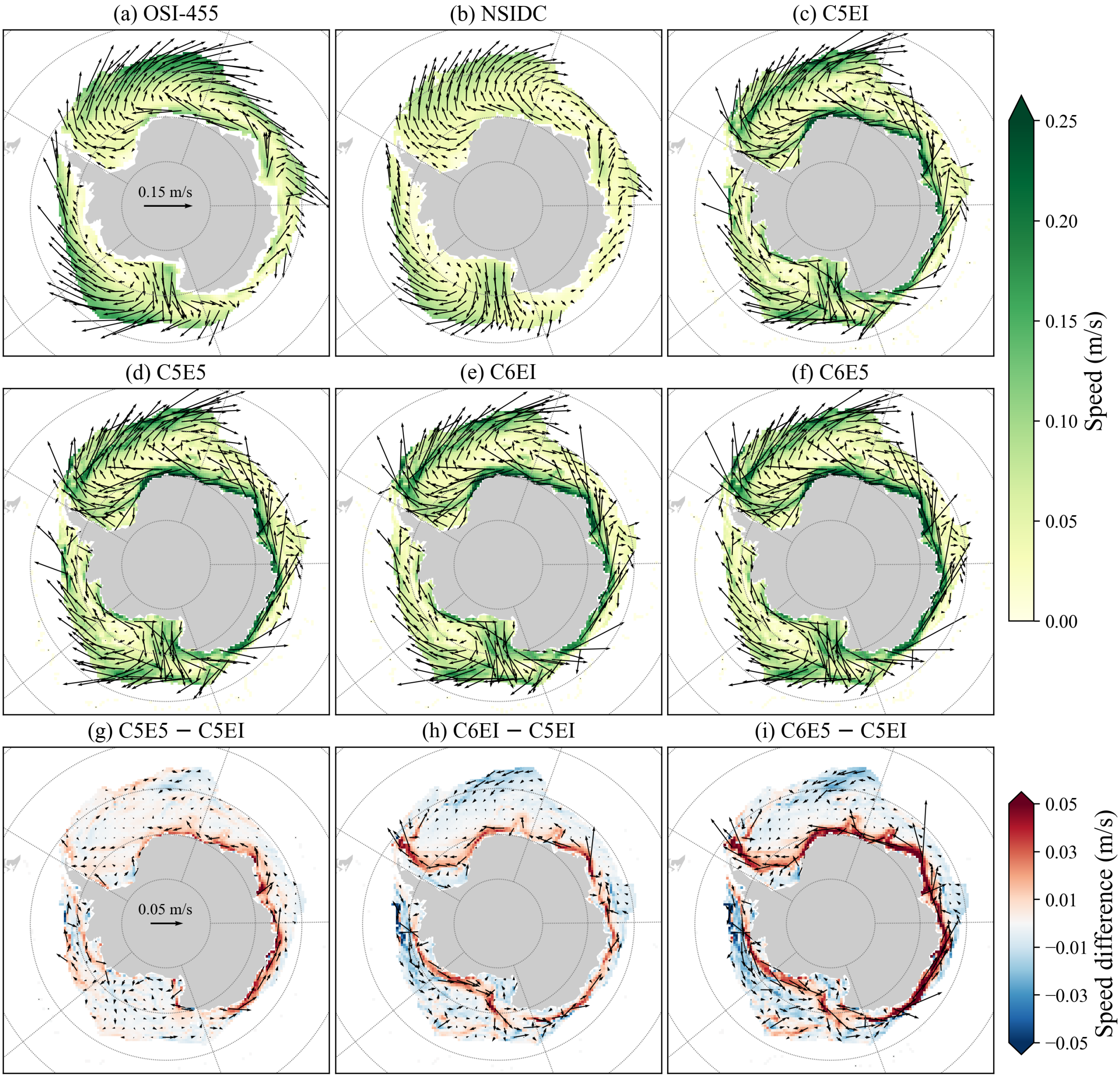


Figure 8: Fig. 8 in the manuscript: Mean sea ice velocities during winter (JAS) over the period 1992–2018, overlaid on the speed from (a) OSI-455, (b) NSIDC ice drift and (c–f) model outputs, and differences between sea ice velocities between model runs (g–i). Only grid cells with climatological winter (JAS) sea ice concentration larger than 15 % are plotted.

Variable	1992-2014 Mean	1992-2014 Slope	2014-2018 Mean	2014-2018 Slope	1992-2018 correlation to NSIDC
			February		
C5EI	0.90	0.000	0.88	-0.121	0.490
C5E5	1.12	0.004	1.17	-0.148	0.683
C6EI	0.86	-0.001	0.78	-0.136	0.471
C6E5	1.18	0.001	1.12	-0.193	0.655
Ice Index	2.05	0.025	2.06	-0.294	0.980
NSIDC	2.23	0.021	2.17	-0.316	-
			September		
C5EI	15.02	0.017	14.44	-0.435	0.801
C5E5	15.38	0.004	14.73	-0.442	0.804
C6EI	15.38	0.033	14.80	-0.532	0.840
C6E5	15.70	0.018	15.07	-0.566	0.868
Ice Index	14.74	0.036	14.44	-0.363	0.960
NSIDC	16.56	0.035	16.18	-0.384	-

Table 2: Table B1 in the manuscript: Mean and slope values for timeseries for sea ice area in February and September (Fig. 4): 1992-2014 and 2014-2018 as well as the Pearson correlation calculated against NSIDC for 1992-2018

least time to evolve differently in the models.

The average salinity flux in ROMS for the first year of the spinup was positive at much of the coast, and mostly slightly negative otherwise. The fluxes are smaller in CICE6 than in CICE5, except close to the maximum ice edge and in parts of the Weddell Sea. The difference is clearly largest at the coast.

In CICE, two fluxes from ice to ocean are passed to ROMS, the freshwater flux (fresh_ai) and the salinity flux (fsalt_ai). Both of these have very similar spatial patterns. When ice is formed, both water and salt are removed from the ocean and the fluxes are negative, and when sea ice melts, freshwater and salt are released to the ocean and the fluxes are positive. These fluxes are on average larger in CICE6 than in CICE5, especially at the coast, similar to the ROMS salinity flux, so that when the ROMS CICE6-CICE5 is negative the CICE salt and freshwater flux CICE6-CICE5 is positive (Fig. 10a).

We looked into the difference in coastal ice growth between CICE6 and CICE5 to see if the difference in the fluxes could be explained by ice growth differences, but as can be seen in Fig. 10b, the difference in ice growth is not consistently positive or negative at the coast. CICE6 has on average a larger ice growth, but there are also areas where CICE5 ice growth is larger, while its fluxes are consistently larger.

This pattern of difference in fluxes correlates much better with the overall ice growth, so that the flux difference increases as ice grows (Fig. 10). This indicates that the flux difference related to the amount of salinity/freshwater flux due to the ice growth, not due to the difference in ice growth between the CICE models.

The text in Lines 482–487 has now been made clearer:

This bias can be attributed to the salt flux from CICE to ROMS which is, on average, smaller in CICE6 than in CICE5. The largest differences can be found at the coast and when the salinity flux is positive, i.e. from the ice towards the ocean, where the ice growth is largest (Fig. 7). The difference does not correlate with the change in ice growth, and seems to be an effect of a change in the flux calculation in CICE (not shown). Because the ice formation rate does not seem to be the cause for the freshening problem, it is likely related to how the CICE's salinity and freshwater fluxes are handled and converted to ROMS salinity flux.

Line 384: The deep waters offshore will basically just be initial conditions so early in the simulation; this should be made more explicit in the note of caution.

We have made this clearer in the text (Lines 458–459):

Deep waters take a long time to spinup due to longer residence times, as discussed in Sect. 3.1, and will therefore be the same or very similar to the initial conditions, and should be interpreted with caution.

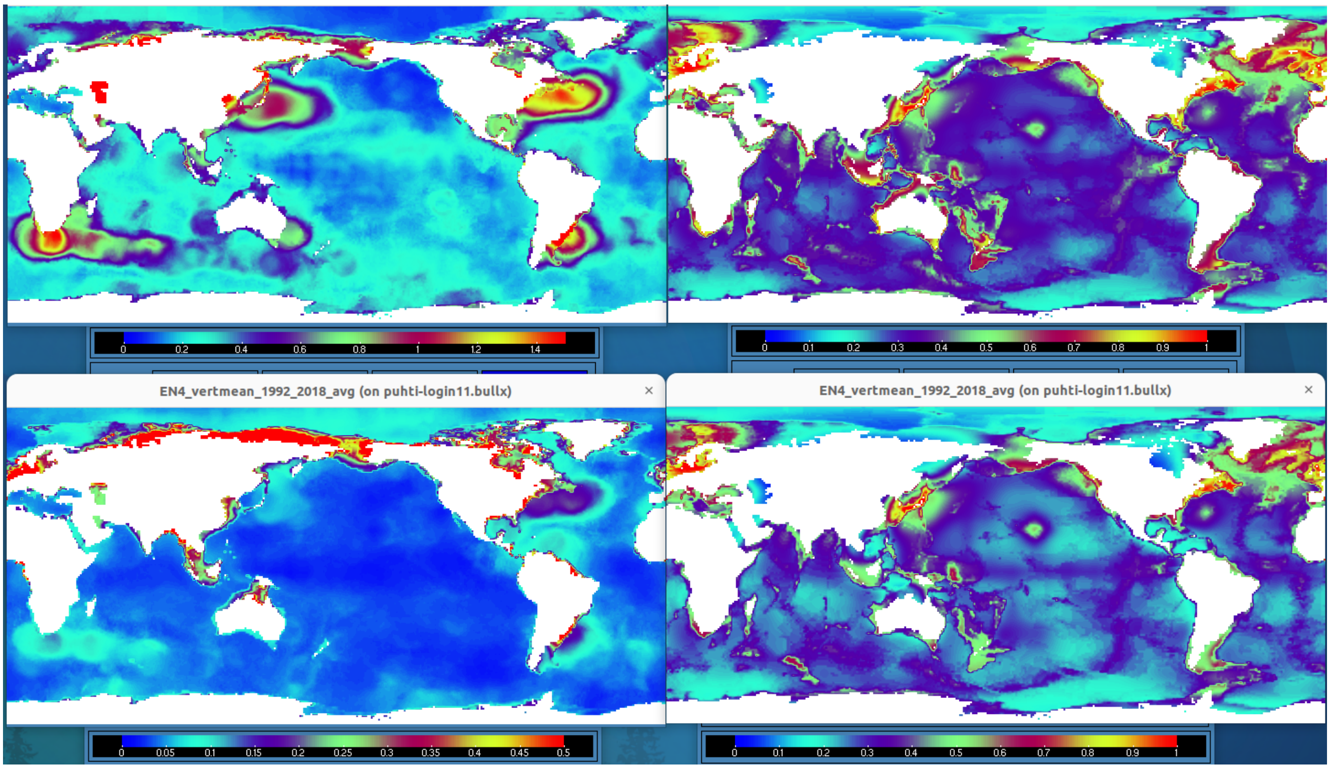


Figure 9: EN4 1992–2018 and depth level averaged uncertainty. Top left shows temperature error standard deviation [K], top right shows temperature observation weights, bottom left shows salinity error standard deviation and bottom right shows salinity observation weights.

Line 398: How is the model surface more saline than EN4 when surface salinity is nudged to observations? Is the nudging really weak, or do the two datasets (WOA and EN4) disagree?

A bit of both. The nudging is quite weak, but the nudging calculated from WOA is also, on average, more saline south of 60° S (Fig. 11). In C5EI the salinity flux from nudging is mostly negative, except in the northern parts of the domain, while in C6E5 we have areas in the south with nudging being on average positive, but there, some of the surface is less saline than EN4, not more saline.

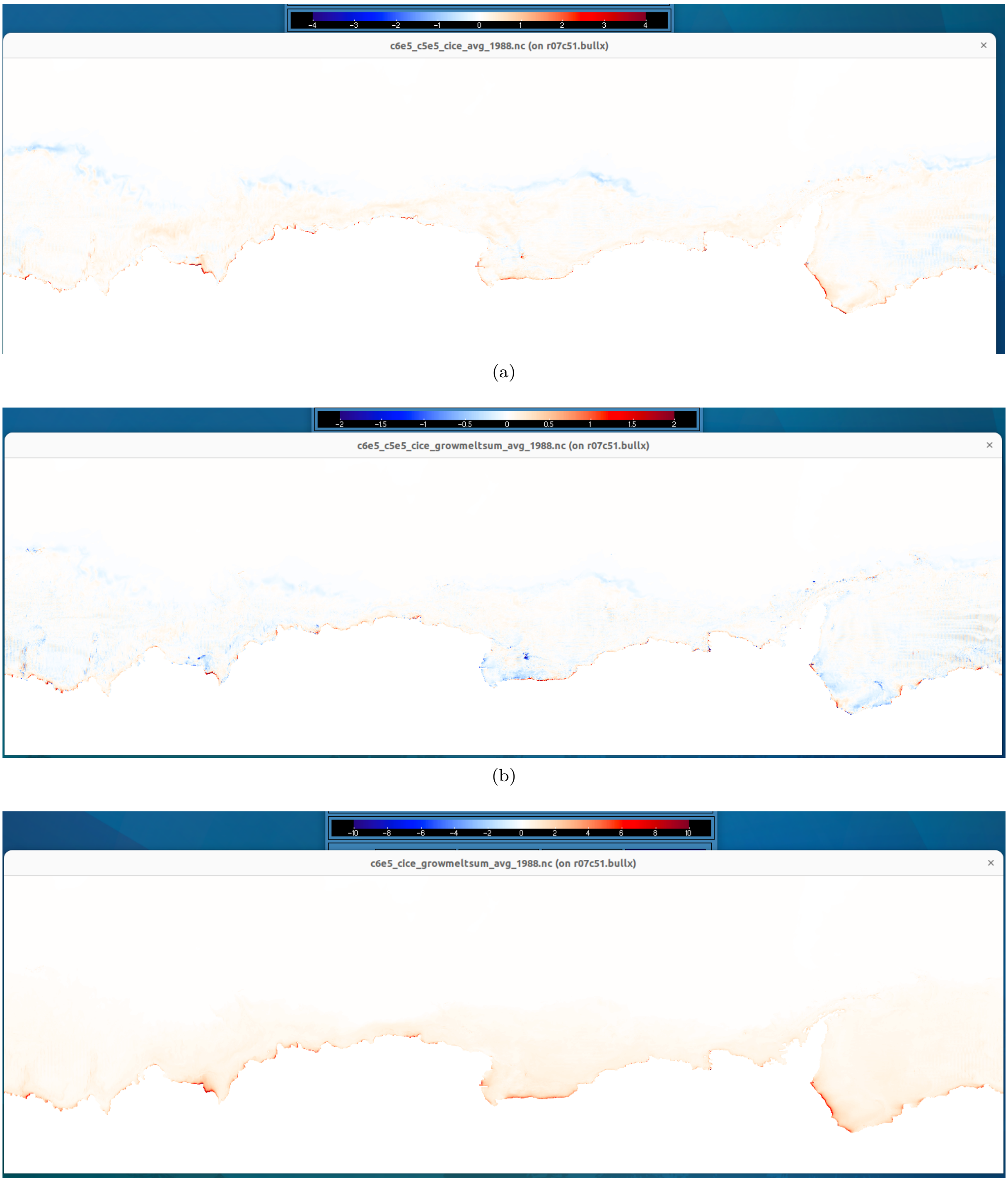
Line 407: A better way to mitigate the freshening problem would be to tune up the CICE6 sea ice formation rates. Salinity restoration should be a last resort, especially on the continental shelves.

This is a good point. However, the ice formation rates do not seem to be the cause for the freshening problem (see answer for comment for Line 370). Nevertheless, it would be good to find a way to handle the effect that the change in salinity fluxes have, to see if something in the coupling or the CICE settings needs to be changed here. 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.

Some updates have been made to the text in this regard, as mentioned above. additionally lines 487–490 reads:

Looking into this would be a good place to start when tuning the model in the future. The salinity restoration scheme could also be updated. The current salinity restoration scheme is only applied when the ocean is deeper than 1500 m (Sect. 3.2) and does not take sea ice into account. Testing different salinity restoration schemes is beyond the scope of this work.

Figure 13: Presumably the colour scale is saturated, as observed melt rates in the Amundsen and Bellingshausen sectors are much higher than 4 m/y. The colour scale should indicate this with triangle caps.



(a)

(b)

(c)

Figure 10: (a) One-year average of fresh_ai difference between C6E5 and C5E5, (b) one-year average of difference of sea ice growth (snoice+frazil+congel) between C6E5 and C5E5 and (c) one-year average sea ice growth in C6E5.

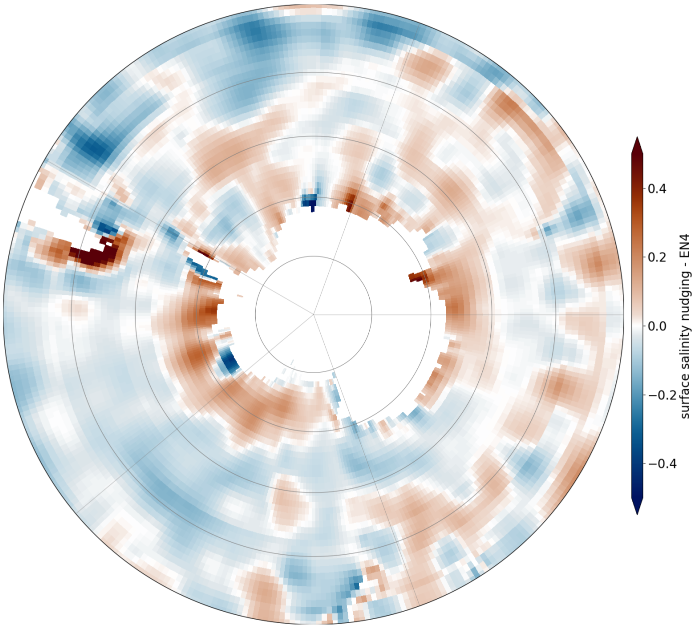


Figure 11: Difference between the average of the sea surface salinity nudging climatology based on WOD and EN4 for the period 1992–2018

Yes, you are correct; the colour scale is saturated. Thanks for pointing this out. We have fixed the colour scale in Fig. 13 and in other figures which had the same problem.

Line 464: The wording implies that the biases in the BellAm sector are secondary, but I suspect they are the main driver of the low total mass loss. The cavities are small, so hard to see in Fig. 13, but their mass loss is huge. This would be easier to see in the bar chart I suggested.

You are correct, from the added table (Table 1) we see that the largest negative bias is in the Amundsen and Bellinghausen Seas together with the Australian sector. Of these, the Amundsen Sea has the largest underestimation compared to Susheel Adusumilli et al. (2020), both in absolute and percentage terms. We have mentioned this in the reworked text on lines 551–557:

The largest underestimations, compared to both observational datasets, occur in the Australian region as well as the Amundsen and Bellingshausen Sea regions (Table 2). In these regions, all the models show values far below the range given by Rignot et al. (2013), except in the Wilkins Ice Shelf in the Bellingshausen Sea, where uncertainty is large. In the BellAm sector, this is likely due to the warm shelf area being too cold in the model runs (Fig. B6b), while this is not the case for the Australian region, which is warmer than observations in the model runs (Fig. B6Bb). Naughten et al. (2018b) speculates that the underestimation of the Australian region is due to a lack of HSSW.

Line 367: Does the Amundsen Sea have the highest melt increase in percentage terms, or only absolute terms? An extra 2 m/y is not a big change for warm cavities like these, compared to cold cavities with much lower initial melt rates.

The Amundsen Sea region has the highest increase in melt both in absolute and percentage terms. This is clearer in the new Table 1.

Line 471: It looks like there is also a loss of refreezing beneath the Ross and Filchner-Ronne Ice Shelves. This would make sense if fresher HSSW is dampening the magnitude of the ice pump.

There seems to be some loss of refreezing in the middle parts of the Filchner-Ronne Ice Shelf, but it seems to be increasing in the western part of the Ronne Ice Shelf. Under the Ross Ice Shelf similar patterns are harder to see. When calculating separately the mass change from grid cells with positive and negative melt rates (Table 3), we see that in the Filchner-Ronne Ice Shelf the negative sum actually increases, meaning there is more refreezing in the long-term average in CICE6. Under the Ross Ice Shelf we see a small decrease, but the change is small.

	FRIS massloss	FRIS refreeze	Ross massloss	Ross refreeze
C5EI	75.73	-10.94	64.60	-2.74
C5E5	77.84	-11.12	58.11	-2.83
C6EI	73.13	-11.33	62.27	-2.60
C6E5	75.20	-11.67	54.54	-2.71

Table 3: Average mass loss over the period 1996–2018 for the Filchner-Ronne Ice Shelf (FRIS) and Ross Ice Shelf separated into positive (mass loss) and negative (refreeze) sums [Gt/yr]

Line 473: I disagree that tides and spatial resolution are the main drivers of low simulated melt rates. I strongly suspect the main driver is the cold bias in the Amundsen Sea (Figure A4), and it's arguable whether or not spatial resolution contributes to this. It's definitely possible to simulate enough CDW transport onshore in a quarter-degree C-grid model; see for example Mathiot et al. 2017 (doi:10.5194/gmd-10-2849-2017). The Nakayama study about topographic flow used a model equivalent to an Arakawa A-grid, whereas a C-grid as used for ROMS is a lot more forgiving. But even if the topography is adequately resolved, the heat from CDW could be wiped out if there is convection on the Amundsen Sea continental shelf, as sometimes happens with ERA forcing even at much higher resolution (eg Bett et al. 2020, doi:10.1029/2020JC016305). Tides could matter for the Filchner-Ronne Ice Shelf, but this is not a big contributor to the Antarctic total.

We agree with the Referee. The cold bias in the Amundsen Sea seems to be indeed one of the main drivers of the low melt rates, with ice shelves in the Amundsen Sea, Bellinghausen Sea and the Antarctic region being the areas with strongest underestimation compared to the observations (Table 1). Sect. 4.3 of the manuscript have been rewritten while adding the table to the manuscript as Table 2.

Line 523: It's a bit of a red flag that ChatGPT was used to write some of the code. Was this just for things like figure layout, or did it actually handle the data analysis? If so, was there sufficient human oversight to make sure it didn't introduce any bugs? I am not sure of the journal's policies on this matter.

ChatGPT was used to generate code snippet from pseudocode like descriptions for some of the scripts. It did not by itself handle any data analysis and all code acquired from it was checked and tested by the corresponding author.

The journal states in its submission guidelines that "Should you have used AI tools to generate (parts of) your manuscript, please describe the usage either in the Methods section or the Acknowledgments", and this is what we have striven to do when stating it was used for some code generation.

1 Anonymous Referee #2

General comments:

In this study, the authors describe an updated version of the MetROMS ocean/sea ice/ice shelf regional Southern Ocean circulation model. Four simulations were run: a base case with the original setup, a simulation where just the atmospheric forcing is updated from the ERA-Interim reanalysis to ERA5, a simulation where just the sea ice model is updated from CICE5 to CICE6, and a simulation where both the atmosphere and sea ice model are updated. The hope is that these changes improved the model simulation of Antarctic sea ice and upper ocean hydrography. Results from the different simulations were compared against each other and observations for sea ice concentration, sea ice area, sea ice edge, sea ice volume (although this is not compared to observations), sea ice drift, ocean temperature and salinity, surface mixed layer depth, and ice shelf basal melt.

I thought the manuscript was clear and easy to understand and generally met its objective of explaining how the upgrades impact the model simulation of different aspects of the Southern Ocean. This setup (ROMS ocean model and CICE6 sea ice model) is a good tool and I'm glad the authors have updated it and made it available to everyone and are using GMD to tell the community what they have done. I felt the specific quantities being compared did a good job of showing the general impact of the upgrades for simulating the physics of the Southern Ocean (although I do have specific suggested additions below).

However, there is not very much on what specific changes in either the forcing or the ice model code led to these differences in the model simulation (there is a little in lines 235–240 about biases in cloud cover and temperatures impacting summer sea ice). I certainly understand how a thorough examination of the causes is well beyond the scope of what the authors are trying to do here. However, are there any large changes between the forcing or ice code versions that are likely to lead to any of the changes seen in the results? For example, does the significant increase in the resolution from ERAI to ERA5 (thus better resolving steep coastal orography) lead to generally stronger winds along the coast, thus impacting ice motion near the coast (e.g. lines 340-341) and perhaps ice production and mixed layer depths over the continental shelf?

We thank the Referee #2 for their comments. We appreciate the Reviewer prompting us to examine and discuss the causes of the differences in the simulations, as we think this can significantly improve the manuscript. Below, we address these inquiries in the best way possible.

One of the major differences between ERA5 and ERA-Interim is the changes in the resolution. While ERA-Interim has a horizontal resolution of 80 km, ERA5 represents a significant improvement with its 31 km resolution. This improved resolution is particularly relevant to resolve small-scale processes at the coast (Fig. 12). For the sea ice, the difference in wind seems to play a relevant role, with slower winds at the open ocean, and stronger winds close to the coast to around 65° S, and then again areas of slower winds at the coast, probably due to a change of topography. These patterns are well correlated to an increased sea ice drift close to the coast, and a decrease of sea ice formation at the coast. We can also see clear differences in most of the other forcing fields, but they are not as easy to connect to the changes seen in the model runs without a deeper analysis. We have added Fig. 12 to the Appendix as Figure B1. Further discussion about these aspects has been included throughout the text, when answering to specific comments of both Referees. Specifically, a sentence in Lines 144–146 in Sect. 3.2:

The largest differences between the ERA5 and ERAI forcing (Fig. B1) are observed mainly at the coast, where the increased resolution helps resolving small-scale processes, for example for the wind, that see large changes at the coast.

The documentation of the sea ice code indicates that no changes to the basic physics should be 'climate changing' in standalone mode, but for example, a change in salinity flux calculation might affect coupled models (see answer to Referee 1 specific comment Line 68). This change in salinity fluxes seems to be behind the freshening in the ocean. Other than that, it is not easy to pinpoint what the reason behind the seen changes are as all processes affect each others in the coupled system.

We think the Referee is correct with their thoughts that stronger wind along the coast might be a reason to the increased mixed layer, and have added the following text to the MLD Sect. 4.2.2 in Lines 508–509:

The observed pattern of MLD increase on the continental shelf and decrease in the open ocean can probably also be linked to changes in wind fields between ERAI and ERA5 (Fig. B1e,f).

I also felt that there could have been more to tie the presented individual changes to each other. For example, for the basal melt there is a discussion of what may cause the systemic underestimation for all the models compared to observations, but there is no discussion of what differences in the modeled hydrography between the simulations would lead to the shown differences in basal melt between the different simulations. Changing the ice model generally leads to greater ice shelf basal melt (especially over the Amundsen, Fig. 13d), which could be related to the temperatures at depth over the continental shelf generally being warmer (Fig. 10e), and the temperature difference between C6EI and C5EI might even be greater over the deep Amundsen shelf (hard to tell as there is no plan view of the temperature difference) as the winter mixed layer depths are significantly shallower there with the ice model change (Fig. 12f). Updating the winds leads to generally lower ice shelf basal melt, which can be related to lower

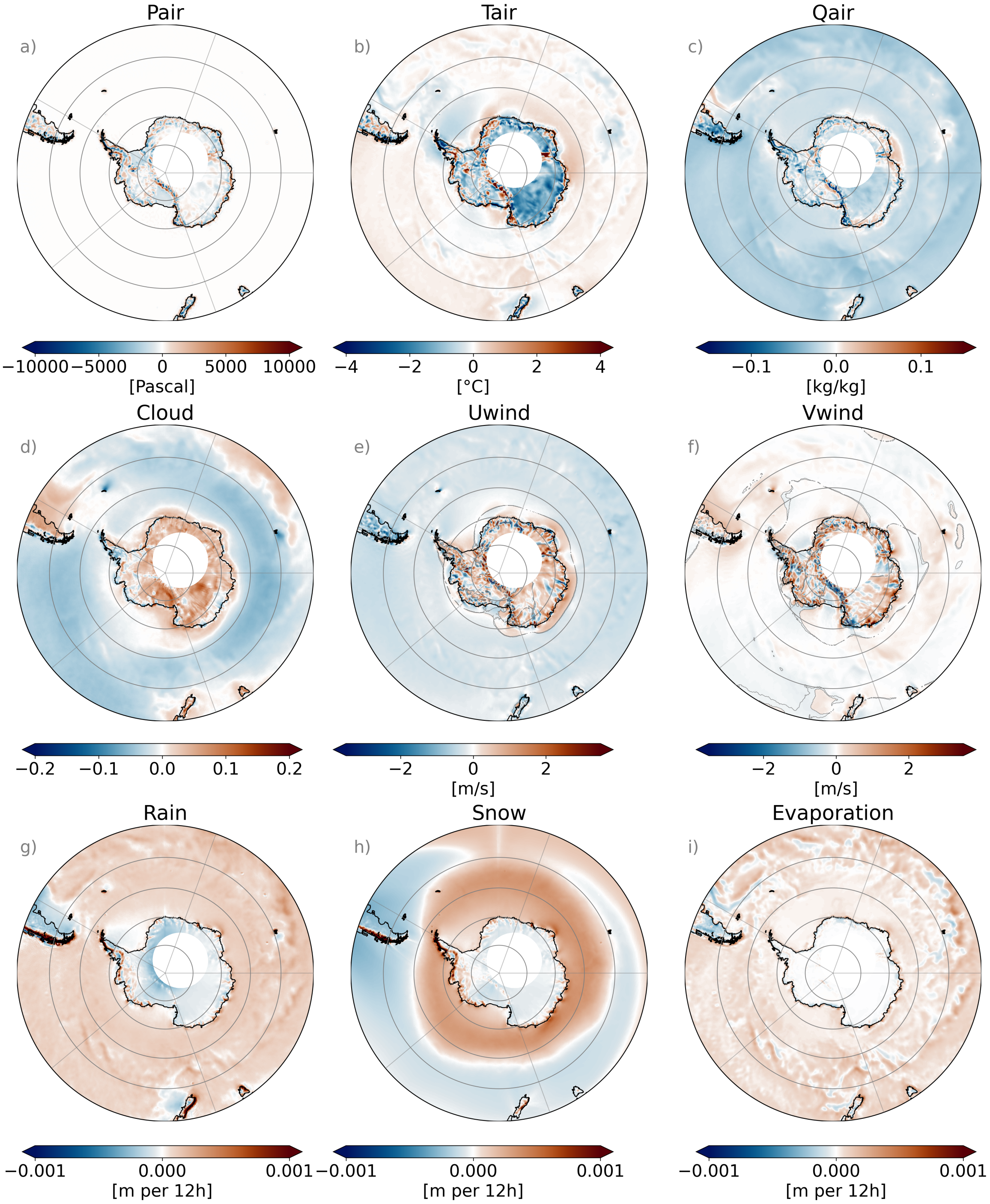


Figure 12: Figure B1 of the manuscript Differences between ERA5 and ERAI forcing fields calculated from 1992-2018 average. For Uwind and Vwind the plots (e,f) shows the change of the strength of the wind component in it's direction calculated as $wind\_diff = (wind\_era5/abs(wind\_era5)) * (wind\_era5 - wind\_eraI)$, and the gray lines indicates the areas where the direction of the wind vector flips into the opposite direction.

temperatures at depth (Fig. 10d) over the continental shelf.

Thank you for the comment. We have added more discussion throughout the text to connect the changes to each other, many of these changes are cited in other answers. However, we have not done a in-depth analysis on the changes under the ice-shelves as that would be out of the scope of this manuscript, but you are correct, the temperature under the ice shelves in the Amundsen Sea as well as under the Amery Ice Shelf increase, leading to the increased basal melt. We have added this discussion and more to the melt rate section (Sect. 4.3).

I have some other specific comments and suggestions below, but most of these are minor and should be easily dealt with by the authors. I think this upgrade is important, but I also think this description of the impacts needs a bit of work before it will be fully useful for the intended audience.

We thank again the reviewer for these suggestions. Please see our point-by-point response below addressing the reviewer's comments.

Specific comments:

Lines 39-41: Is it worth adding something here that while satellite based estimates of thickness are now becoming available in the Arctic, these efforts are not nearly as well advanced in the Antarctic since it is harder to estimate freeboard? I know this is briefly mentioned at the beginning of section 4.1.3, but since this is another example of a difference between southern and northern sea ice, should it be mentioned here?

Thank you for the comment. We have added a mention in Lines 37–39 about the disparity between Arctic and Antarctic satellite-based estimates:

And while satellite-based estimates of sea ice properties, such as sea ice thickness, are becoming available in the Arctic, these efforts are not nearly as well advanced in the Antarctic.

A sentence on the complexity of snow and sea ice conditions was also included in Lines 44–46:

The Southern Ocean receives more snowfall than anywhere else in the world (Lawrence et al., 2024), and the snow distribution on the sea ice is more complex than in the Arctic due to warmer winter temperatures and frequent snow flooding (Willatt et al., 2010).

And the difficulty of estimating freeboard in satellite measurements was included at the beginning of Section 4.1.3 such that Lines 300-302 read as follows:

Sea ice thickness observations in the Southern Ocean are scarce and have large uncertainties (Holland et al., 2014; Uotila et al., 2019; Xu et al., 2021), largely due to the complexity in the snow cover, which makes it challenging to measure freeboard of the sea ice from satellites and affecting satellite-based estimates of ice thickness (Giles et al., 2008).

Lines 70-71: I think it would be helpful to any potential users to explicitly list somewhere (maybe a supplemental table?) the instances in which CICE default values are not being used.

Thank you for your comment. The CICE6 input file, ice.in has been provided as part of the model code in Äijälä and Uotila (2024) in the "example_input" folder. This file shows exactly what input parameters were used, and we will not add a table of the parameters to the manuscript, as the manuscript is alreaady quite long.

Lines 86-87: Is the freeze-melt potential from ROMS integrated over the top 5 m of the ocean model for both versions of CICE as in Naughten et al. 2017? If so, since there could be multiple ROMS layers in the top 5 m, is supercooling in the layers below the surface layer also handled as described in Naughten et al. 2017?

Yes, the freeze-melt potential is handled in the same way. The following text has been added at lines 97–98 to clarify:

The freeze–melt potential, following (Naughten et al., 2017), is integrated over the top 5 m of the ocean model.

Line 123: I really feel the ACC transport for the different experiments should be given.

Thank you for the comment, this was also a concern raised by Reviewer #1. Following this advice, we have added a section about DPT in Appendix A of the revised manuscript, where a short discussion is given. Appendix A includes Fig. 1 (Fig. A1 in the revised manuscript), which is now referenced in Line 134.

Section 3.2: I know it is mentioned in section 4.3, but I think it should be explicitly mentioned somewhere in this section that there is no tidal forcing.

We added a mention about the lack of tidal forcing in Section 3.2 (Lines 147–148):

The model does not have tidal forcing as tides are not accounted for.

Lines 150-152: Is there also a sponge region near the northern boundary with increased viscosity and diffusivity as in Naughten et al. 2017?

We did not use a sponge region near the northern boundary.

Line 162: Suggest changing "increases the concentration" to "generally increases the concentration" since there are areas where the ice concentration decreases with the updates.

We followed this suggestion, thank you.

Lines 221-222 and Figure 4: I think it would be helpful to the reader to include a numerical comparison (correlation or skill score maybe?) between each of the different modeled sea ice area time series and the observations.

We have added a table to the Appendix B. Table B1 includes correlations, means and slopes following this suggestion and that of Referee #1.

References to this table with some numerical comparisons have been added through the text referring to the figure in lines 241–253.

Lines 307-316: One other metric that I think would help in determining why there are increases in concentration but decreases in volume near the coast when updating the ice code is the thermodynamic sea ice production, which could be compared to observations (e.g. Nihashi and Ohshima, 2015; Nakata et al., 2019) and help determine if volume decreases near the coast are due more to decreased production or changes in ice movement.

We have added a figure on growth and melt following the suggestion from the general comment 3 of Referee #1 (Fig. 7), but we consider that making a comparison between our model runs and the suggested polynyas ice growth publication is out of the scope of this paper, as it would require considerable work to be addressed properly.

We have however mentioned this phenomena in rows 360-361 in the new Section 4.1.4:

This supports the argument, presented in Sect. 4.1.3, that the decrease in volume is connected to the change in ridged and level ice area fractions (Fig. B4).

Figure 7 caption: Is the "climatological sea ice concentration" the climatology during winter (JAS) or over the entire year?

It is the climatology during winter (JAS). The legend in Fig. 7, Fig. 8 in the revised manuscript, has been updated (Fig. 8).

Line 346: "Robust" seems like a strong word for the level of increases shown in Fig. 8 (e.g. from 0.457 to 0.466 in the Indian Ocean sector or 0.483 to 0.493 in the BellAm sector) between C5EI and C5E5.

Thank you for your comment. We have remove the word "robust" from the sentence now in Line 411.

Figure 9: I believe the ROMS model output is not absolute salinity, so my guess is that the authors converted the output to absolute salinity. Apologies if I missed it, but should that be explicitly mentioned

somewhere?

You are correct, the model output is given as practical salinity and has been converted to absolute salinity in the analysis using the TEOS-10 standard. This information has been included in the captions of Fig. 9 and Fig. 10 of the manuscript.

Lines 405-407: This is more reason why I think it would be helpful to show the sea ice production from the different runs.

The change in salinity flux is not due to a change in sea ice growth, but seems to be due to a change in CICE instead. See answer to Referee # 1. comment for line Line 370.

Lines 428-429: It is hard to tell from the figures here that there is "a small improvement of the summer MLD". Can the authors make a table of overall RMSE between the MLD of each of the different runs and Sallée (or add a column to Fig. 11 with C6E5 – Sallée)?

We have added the suggested column to the Fig. 11, now Fig. 12.

Line 441: Suggest adding "excess" between "possible" and "deep water formation" since there should be deep water formation in these areas even if the MLDs there were perfect.

The word "excess" has been added as suggested.

Line 457: The 578 Gt/yr for C5EI is a fair bit lower than the 642 Gt/yr in Naughten et al. 2018b. Do the authors have ideas on what is causing the difference, other than the different (1996-2018 vs. 2002-2016) time periods?

The C5EI is indeed lower than that in Naughten et al. (2018b), but we have not looked into this difference in detail. We do not have the exact setup used in Naughten et al. (2018b), nor the output data for that run, so further analyzing this difference, or the reasons behind them is challenging and outside the scope of this work. However, we notice that the difference is not only due to the use of different time periods, as calculating the C5EI mass loss in 2002-2016 gives us a mean of 583Gt/yr.

Looking at the mass loss comparison between different regions made at the request of Referee #1 (Table 1), as well as the shelfwise table it is based on, and comparing it to Table 1 in Naughten et al. (2018b), the biggest absolute differences seem to be in Amundsen Sea where Naughten's melt rate is 125.1 Gt/yr and our C5EI run is 83.7 Gt/yr. Here, all our mass losses for the separate ice shelves are smaller. We also see clearly smaller values in our run for Bellinghausen Sea and eastern Weddell region, while our run has a clearly higher mass loss in the Filchner-Ronne ice shelf. We have not looked into the reasons behind these differences, and feel that to be out of the scope of the manuscript.

An additional note: Due to small corrections in the code, the value of 578 Gt/yr should actually be 577 Gt/yr, due to rounding differences.

Line 461: Suggest adding "model" between "The" and "refreezing" to differentiate the melting in this sentence from the preceding one.

The word "model" has been added as suggested, thank you.

Lines 472-485: This discussion on why all the models have too little melting is interesting, but most of this is already discussed in Naughten et al., 2018b (their sections 4.3 and 4.3.6), which should be referenced here. Also, while it's certainly important to mention this large difference, since this paper is mostly about the upgrades, shouldn't there be something on why there is a difference in the melting with the atmospheric forcing and ice model updates?

Thank you for the comment. We have rewritten most of the melt rate section (Sect. 4.3), added clearer citations to Naughten et al. (2018b), and added discussion on reasons for the changes.

Technical corrections:

Lines 6-7: I don't think the sentence beginning "Both CICE sea ice models. . . " is necessary.

Thank you for your comment. This sentence has been modified to:

’The two versions of MetROMS evaluated in this study use a version of the regional ROMS ocean model including ice shelf cavities.

Line 8: “increase” should be “increases”.

This has been fixed.

Line 10: Should “ocean mix layer” be “ocean mixed layer”?

Thank you for pointing this out, we have fixed this.

Line 96: Should the Filchner-Ronne, Amery, and Ross ice shelves be labelled somewhere (Fig 1b)?

We have added labels to Fig. 1b of the manuscript, as seen in Fig. 5 of this document.

Line 208: “being C6E5” should be “with C6E5 being”.

Thank you for pointing this out, the sentence has been corrected.

Figure 6 caption: The units should be “([model volume]/[C5EI volume])”.

Thank you for pointing out the error. The caption has been corrected.

Line 312: Typo, “A3)a-d” should be “A3a-d”.

This has been fixed.

Line 358: Should the “;” be “ and”?

Thank you for pointing this out, this has been corrected.

Line 370: “them clearly fresher” should be “them are clearly fresher” or “them clearly are fresher”.

Thank you, this has been corrected.

Line 475: The “.” After “basal melting” is not necessary.

The full stop has been removed.

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