Resolution dependence of interlinked Southern Ocean biases in global coupled HadGEM3 models.

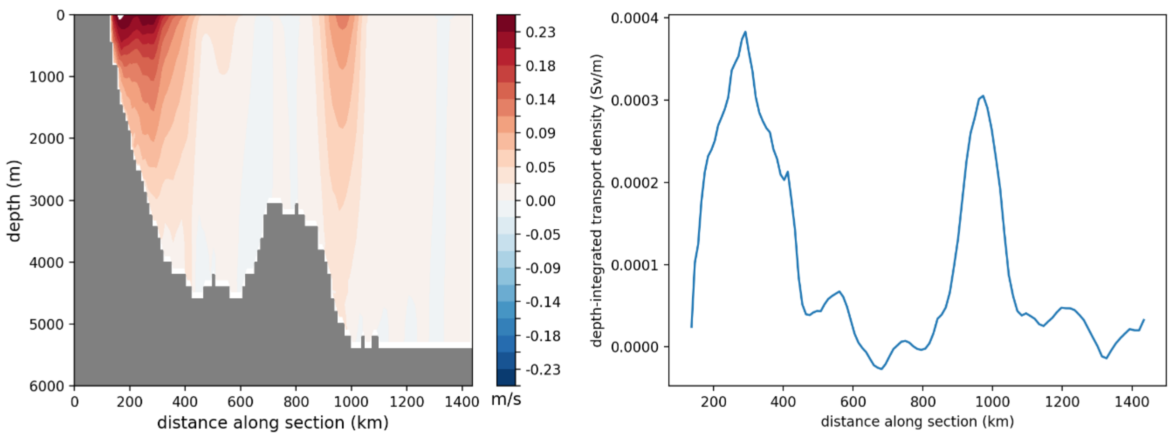
Authors' response to reviewers and list of changes to revised manuscript

We would like to thank both reviewers for taking the time to provide detailed and constructive reviews.

Here we respond to the general comments of both reviewers first and take the more detailed comments later.

General comments

1. Separation of the recirculating gyre transport and ASC transport. Reviewer 1 makes the point that the dynamics of the gyre and the slope current are distinct and suggests that we provide figures for the ASC transport separately by calculating the transport over the continental slope. In the region of the gyres it is difficult to separate the ASC from the gyre, since the recirculating gyre transport impinges on the continental slope and models at these resolutions do not form very distinct jets – see for example the plots below showing cross sections of currents and depth-integrated transport densities from the MM model in the Weddell gyre.



In order to give some indication of the circumpolar flow distinct from the recirculating flow, we have followed the reviewer's suggestion and provided plots of the depth-mean currents in the top 500m (Fig 2 revised manuscript) to complement the plots of the streamfunction in Fig 1. We have modified the text in L105 and L277 (revised manuscript) to refer to this figure. We have also provided an extra timeseries plot in Fig 7 (revised manuscript) showing the timeseries of the counterflow at the southern boundary in the Drake Passage (as defined in Fig 2) compared to the observations of Meijers et al (2016).

2. Use of EN4 1950-1954 climatology for assessment. Both reviewers make the point that observations in the Southern Ocean for this period are extremely sparse, and therefore advise caution in the conclusions drawn by comparing the model with the EN4 climatology for this period. The EN4 analysis uses a climatology for the period 1970-2000 as a background climatology to which the solution relaxes in the absence of observations (Good et al, 2013). Given the lack of observations in the Southern Ocean, the 1950-1954 climatology is likely to be very close to this background climatology.

   We have added a footnote (to L91 of the revised manuscript) to this effect.

3. Length of integration and experimental design. Both reviewers point out that the integrations are short in the context of climate studies, and reviewer 1 questions the statement that *"the early spin up of the model can be useful in diagnosing model biases, since at this stage the model has not drifted too far from initial conditions"* because the initial state of the ocean is out of balance with the greenhouse gas forcing being used and there will be an adjustment associated with this. These experiments follow the HighResMIP protocol (Haarsma et al., 2016), in particular the *spinup-1950* and *control-1950* experiments, in which the model is initialised with "1950s" initial conditions and spun up with 1950s greenhouse gas forcing. As stated by Roberts et al (2019), the nominal spin up for these integrations is very short (30 years) due to computational constraints. As noted in point 2 above the "1950s" initial conditions are likely more similar to a later state of the ocean, so there is an imbalance between initial conditions and the forcing, but the imbalance will not be as great as for a pre-industrial spin up integration. Still it is clear that the model is adjusting in the first decades of the spin up and nowhere near an equilibrated state. However from the timeseries in Fig 6 it seems that there is a fast initial adjustment over the first 2-3 decades after which the three models reach very different states, and these differences in many cases persist for multi-centennial timescales. (We have only shown the first 100 years in Fig 6 because of the length of the sensitivity experiments)**.** In particular, the biases in the MM model appear to be very persistent, so we believe we can learn something about them by studying the initial adjustment period.

   We have rewritten the opening paragraph of Section 2.2 (L87-95) to emphasise that we are following the HighResMIP protocol and argue that we can learn something about the impact of resolution on the model biases from these relatively short integrations in the spirit of Roberts et al 2019.

4. Dense water formation and export. Reviewer 1 suggests some analysis of dense water formation and export from the shelf and AABW export to shed light on the water mass biases on the shelf and possible spurious mixing in the interior. We agree that the models' representation of dense water formation and export could well be relevant to the formation of the biases studied in the paper. From the cross sections in Fig 7 it seems clear that the export of dense water from the shelf in the Weddell Sea is only captured by the 1/12˚ model. We plan to do a systematic study of the dense water formation in the different models looking at the water mass transformation on the shelf, the export over the shelf break and the time evolution of the reservoir of AABW, but we would prefer to present this in another paper.

   We have discussed the potential importance of the models' representation of dense water formation and export in the final paragraph of the Conclusions (L401-410 revised manuscript) and stated that it will be the subject of future work.

5. Explanation for isopycnal slumping in the open ocean. Reviewer 2 says that the slumping of isopycnals across the ACC in the medium resolution model is counter intuitive and that this is not addressed very prominently in the paper, with a tentative explanation only appearing in the last paragraph of the Conclusions.

   We agree and we have followed the reviewer's suggestion of emphasising the counter-intuitive nature of the result in Section 4.2 of the revised manuscript. We have renamed this section to emphasise the discussion on slumping isopycnals. The discussion of the possible link to spurious mixing and the loss of AABW has been moved from the Conclusions to this section.

6. Inclusion of partial slip and scale-aware Gent-McWilliams in 1/12˚ model. Reviewer 2 asks if we tested partial slip or scale-aware Gent-McWilliams in the 1/12˚ model. We have included the combination of partial slip and scale-aware GM in our standard 1/12˚ model configuration, but due to computational expense we only have a clean test of the impact in a forced ocean-ice configuration. In this test, the combined effect of partial slip and scale-aware GM is still positive in the sense that it increases the ACC strength and reduces the gyres, but the impact is not as great as it is for the 1/4˚ model. This might be expected because the increased resolution at 1/12˚ means that the GM coefficient will be non-zero over a smaller area than for the 1/4˚.

We have mentioned these results with the 1/12° model in the Conclusions (L390-393 of the revised manuscript).

Detailed comments reviewer 1

L16: "Antarctic Overturning Circulation". I think stating "between the near surface and deep ocean via the formation of Dense Shelf Water (DSW), Antarctic Bottom Water (AABW), and mid-depth ocean via mode and intermediate water formation" would work better here. If you are linking to the Southern Ocean being critical to the climate system - also mentioning the mode & int water formation is important as this is where all the heat and carbon is going.

We have made the suggested change.

L23: Capitalize C D and W in "Circumpolar Deep Water".

Done.

L24: Also, there are a lot of warm biases in 1-degree CMIP-class models (Beadling et al., 2020), not sure there is a definitive link to high resolution models being warmer? For example, in some high-res simulations we actually see warm biases improve with finer nominal grid spacing.

Agreed – the statement about the resolution dependence may have been a Met Office centric point of view. We have changed this to simply state that warm biases in the Southern Ocean are a common problem in CMIP models (L25 revised manuscript).

L25: It would be useful to cite Hallberg 2013 here which shows this limitation very well. Hallberg, R. (2013). Using a resolution function to regulate parameterizations of oceanic mesoscale eddy effects. Ocean Modelling, 72, 92–103. https://doi.org/10.1016/j.ocemod.2013.08.007.

We have added a reference to Hallberg (2013) together with the reference to Hewitt (2022) in L28 revised manuscript.

L50-53: This paragraph is probably not necessary.

We think it is useful to have a very brief overview of the structure of the paper and have left it in the revised manuscript, but are happy to remove this if the editor prefers.

L56-57: These two sentences appear as separate from the paragraph below. I suggest combining these into the paragraph below.

We have combined these two paragraphs.

L60: “and partial cells (Barnier et al., 2006; Adcroft et al., 1997) allowed next to topography.” -> “and partial cells at the ocean bottom to better represent bathymetry (Barnier et al., 2006; Adcroft et al., 1997).”

That reads better. We have made that change.

L67: “Cavities under ice shelves are closed and the output of basal melt water at the ice shelf front parametrised as described in Mathiot et al. (2017).” This is interesting, so this model represents “ice shelf melt”? Is this just based on some threshold of solid precip over the Antarctic continent? Could you elaborate on this? I assume this does not imply there are realistic melt rates?

The distribution of melt water input around the continent is based on Rignot et al (2013), but for these experiments the overall magnitude of melt water plus iceberg calving is scaled to equal the total precipitation over the Antarctica at each timestep, ie. An assumption that the total mass of ice over the continent is constant. We have expanded the description in the text to explain this (L71-74 revised manuscript).

L83-84: I suggest to briefly elaborate on the term “model drift” here for readers that are not familiar.

We have expanded this sentence to make it more explanatory – L87-89 revised manuscript.

L84-85: “However the early spin up of the model can be useful in diagnosing model biases, since at this stage the model has not drifted too far from initial conditions.” ß I am not sure I agree with this statement, assuming the ocean is starting from a present-day climatology (say WOA13 or WOA18) and a pre-industrial atmosphere, this early stage is an unrealistic climate state and an assessment of realism (i.e., biases relative to observed) is better made once the model has been able to achieve its own equilibrium (or better yet,reached that equilibrium and forced with observed climate forcings; i.e., the historical simulation). I tend to think of the spin-up stage as the adjustment stage that we don’t want to consider when doing assessments against observations. I suggest rewording this or expanding on your reasoning here.

See response under General Commets point 3 above. We have rewritten this paragraph to make our approach clearer.

L93-101: As you note, the gyres and ASC transports merge into one another particularly in the Weddell, so it is hard to discern these from one another in the current figures. I would suggest an additional plot of the upper 1000 m speed (or upper 500 m speed) to see the differences in the strength and location of the ASC. L93-101: As you note, the gyres and ASC transports merge into one another particularly in the Weddell, so it is hard to discern these from one another in the current figures. I would suggest an

additional plot of the upper 1000 m speed (or upper 500 m speed) to see the differences in the strength and location of the ASC.

See response to General Comments point 1. We have provided maps of the depth-integrated currents over the top 500m in Figure 2 of the revised manuscript to complement the plots of the streamfunction in Fig 1 and we have updated the text in L105 and L277 (revised manuscript) to refer to the new figure.

L102: “net eastward transport” this wording is confusing, there is eastward and westward flow through Drake Passage, should this just say “net” to avoid confusion?

We have used “net transport” instead of “net eastward transport” throughout the revised manuscript.

L103-104: It is worth mentioning that this is exceptionally weak even compared to earlier / other estimates (Cunningham et al., 2003; Griesel et al., 2012; Meijers et al., 2012; Koenig et al., 2014; Firing et al., 2011; Xu et al., 2020).

We have added this point (L114-116 revised manuscript).

L108: “These counterflows significantly reduce the net eastward transport.” I would remove the word eastward after net and just say “these westward counterflows significantly reduce the total net transport”.

We have used “net transport” instead of “net eastward transport” to be consistent with the rest of the manuscript.

L109-111: It is worth mentioning that Xu et al., (2020) also shows net westward flow at depth - this is why the authors argue that Donohue et al., (2016) overestimated the net transport through Drake Passage. Although they are referring to bottom recirculations - different from what is shown here.

In fact, the 1/12 degree model discussed here has bottom-intensified recirculations similar to those described in Xu et al. but the ¼ degree model has very barotropic recirculations. We have added this point to the paragraph (L123-125 revised manuscript).

Figure 3 caption: Why is the 1950-54 climatology used here for comparison? Because it is close to the initial conditions?

Yes. Although it is also the case that the 1950-1954 climatology is likely very similar to a later climatology (see point 2 above), so it could be viewed as comparing to a “best available” long-term climatology.

L120: “partially eroded to the east of this region in the eddy-rich model” - swap “in the east” to “to the east”. Also, this is likely due to the delivery of cold, fresh Weddell Sea water to the WAP.

We have made this change.

L122-128: How different are the sea ice edge locations? It would be helpful to add these to the plots of Figure 5 for reference of where these anomalies are.

We have added lines showing the September sea ice extent to the plots in Figure 5 and updated the corresponding text in L141, and L241-242.

L131-133: Given that the gyres and ASC are governed by different dynamics, I strongly suggest breaking this down into an assessment of gyre strength and the ASC separately.

As noted under point 1 above it is difficult to separate the gyre transport and ASC transport in the region of gyres. We have added a timeseries of the magnitude of the westward counterflow in the Drake Passage to Figure 6, to give an indication of the slope transport not associated with recirculating gyres.

L133: remove “eastward” before “net” due to comments earlier.

Done.

L131-135: These series of sentences sound a bit choppy as they are currently written with all starting with “We”, “We”, “We”.

We have rewritten these sentences.

L134: Remove “deep” before “fields below 400 m” as “fields below 400 m” imply they are deep.

Done.

L143: “and comparing to a similar average performed on the 1950-1954 climatology of the EN4.1.1.g10 analysis dataset”. I might be missing something, but why is this time period used for comparison? Observations would be very sparse for this time period and particularly so in the Southern Ocean.

Addressed under General Comments, point 2 above.

L157-158: Yes, this could indicate and issue with CDW cross-shelf intrusions, but this could also be due to the westward transport of cold, fresh Weddell Sea water around the WAP mixing with CDW (making it colder & fresher). The maps of ocean velocities support this connection. This has been found in other simulations when the ASC accelerates (Beadling et al., 2022) and this mechanism has been documented as well by Morrison et al., (2023) “Weddell Sea Controls of Ocean Temperature Variability on the Western Antarctic Peninsula”. You mention this below in lines 168 – 169 but it should be mentioned here or this discussion combined.

We have mentioned the possible advection of fresh water around the Antarctic Peninsula at this point (L177-178 revised manuscript).

L163-165: This would be shown nicely with a surface water mass transformation analysis (sWMT). This is also consistent with Tesdal et al., (2023) which showed that when the ASC accelerates, DSW reduces as the shelf becomes more buoyant.

We agree and we plan to look at water mass transformation metrics as part of future analysis (see General Comments, point 4 above). We have added this point in the last paragraph of the Conclusions in the revised manuscript.

L180: Add "ocean" before circulation.

Done.

L183: Should you add "and associated treatment of mesoscale eddies" after "ocean resolution", to be clear this is not just due to horizontal grid spacing alone?

Yes that is a good point. Done.

L201: remove "eastward" when referring to the net transport.

Done.

Figures 1,3,4,5,8. It would make comparisons easier for the reader to add two additional panels to these plots of the N216-ORCA025-GM MINUS N216-ORCA025 and N216-ORCA025-PS MINUS N216-ORCA025. It is hard as of now to discern some of the differences.

In order to avoid cluttering the original figures we have followed the second reviewer's suggestion to include difference plots showing the impact of Gent-McWilliams and partial slip on the temperature and salinity fields in supplementary figures as an appendix – Figures A1-A4 in the revised manuscript. We have updated the text to refer to these in L217-218, 230, 233 and 240 and 302 of the revised manuscript.

Figure 2 & L200-203. It looks like GM really impacts the westward along-slope flow (ASC) through the passage while the rest appears unchanged. PS appears to reduce flow everywhere (even the eastward flow in the Subantarctic Front), however reduces the eastward components more … which is why the net increases. It is hard to see visually what component of the along-slope flow is decreasing --- is this mostly coming from the bottom flow or surface intensified flow?

The details of the impact of PS and GM in the Drake Passage are complicated. We have rewritten the end of this paragraph (L225-228 revised manuscript) to try to better capture the differences.

Figure 6: The lines for N216-eORCA025-GM and N216-eORCA025-PS are very hard to discern. Can you make one have circle markers? The dashed and the dashed-dotted are very hard to distinguish.

We have used dotted lines instead of dash-dotted lines for N216-ORCA025-PS. Hopefully this is clearer.

L204: “The fresh biases on the shelves are reduced, with some recovery of the HSSW in the western Weddell Sea and western Ross Sea (Figure 3)” - The shelves in the South Indian sector appear relatively unchanged too.

The difference plots (Figure A1) show some reduction of the fresh bias in the Indian sector as well.

L205: “The timeseries show that again, Gent-McWilliams appears to have a stronger impact than partial slip.” This sentence is referring to shelf salinity, yet this is only true for the Ross. The PS West Antarctic shelve Amundsen / Bellinghausen) looks better for the PS (Figure 3) (This is ALSO true for shelf temperature as you mention below, so this would just require rewording). The timeseries for the Weddell salinity looks similar between the two.

We have expanded the text in L230-238 to describe the results in more detail and include the reviewer’s point that PS has a stronger impact than GM in the Amundsen Sea for both temperature and salinity.

All figures: Increase size of text on color bars / axes, some of these are hard to see.

We have increased the font sizes for axis and color bar labels on all figures.

Figure 7:

• The top figures from Thompson et al. (2018) have a y-axis in km, but the rest are in m. This should be made to be consistent across the panels. Text on axes are also hard to read. The titles on the top and bottom also look very large compared to the other labels in the other figures in the manuscript.

We have increased the font sizes for axes and colour bars, and reduced the font sizes for the column headings. We have labelled the depth axis in km for all plots.

• Figure 7: I assume that the grey shaded regions are not the models true bathymetry? The blocky-nature makes it appear that the models do not account for partial cells. I assume in reality this is more smooth?

It is true that the masking in the plots does not include partial cells, and that if this were done, the bathymetry would look smoother. However, partial cells have the biggest smoothing impact in weakly sloping bathymetry. Over the shelf slope the bathymetry is still quite blocky even with partial cells.

• Figure 7: The Thompson et al., figures are conservative temperature, not potential temperature. These should be consistent between the observation panels and the model output panel.

This was an oversight – we have now plotted the model results as Conservative Temperature to match the Thompson et al figures.

L220: “The properties of the shelf water are controlled partly by local surface fluxes and partly by the exchange of water with the open ocean across the shelf break” -> “The properties of the shelf water are controlled partly by local surface fluxes and associated water mass transformations, and partly by the exchange of water with the open ocean across the shelf break”

We have made this change.

L223-226: This paragraph seems short for a stand-alone paragraph, consider merging with the one below.

We would prefer to keep this paragraph separate (even though it is short) because then we have a separate paragraph for each of the Fresh Shelf, Dense Shelf and Warm Shelf cases.

L229: This statement needs a citation: “these incursions are likely to only happen occasionally as tidal or eddy driven fluctuations of the front position onto the shelf.”

We have added citations for Wang et al 2013 (action of tides) and Goddard et al 2017 (action of eddies). L261 revised manuscript.

L237: Cite Thompson et al., (2018) after this statement: “the observed structure is more complex, with a V-shaped pattern of isopycnals associated with the incursion of CDW onto the shelf and its transformation and export as Dense Shelf Water (DSW)”.

It’s not clear that this is necessary, given that this whole section is comparing to the observations shown in the Thompson et al paper?

L255 (and throughout): Degree sign missing between number and letter for longitude (this is also true for latitudes in the text).

We have corrected this throughout the manuscript.

L247: Add comma after “in general”.

Done.

L268-270: Paragraph is short – suggest combining.

Done.

L276: This is consistent with the feedback to meltwater Bronselaer et al., (2018) suggested i.e., slumping of isopycnals resulting in more heat delivery to shelf --- just pointing it out, but perhaps not necessary to discuss.

Yes that's a good point. The mechanism described in their paper is similar to the one we are proposing here.

L281: Add "ocean" before flow.

Done.

L284: Should this be "wind stress curl", not "wind curl"?

Yes we have changed this.

L285: What does "medium resolution" imply in km?

N216 is roughly equivalent to 60km resolution. We have added a footnote to this effect.

Detailed comments reviewer 2

L. 1-2: "eddy-permittng ocean resolution" -> "eddy-permitting ocean resolution without additional eddy parameterisation"

This isn't strictly correct because the eddy-permitting (and eddy-rich) models both include isopycnal diffusion which is supposed to represent the action of eddies. The inclusion of isopycnal diffusion, but no Gent-McWilliams type parametrisation in eddying models is common practice (if slightly strange) and we explicitly state that we are testing the use of a kind of Gent-McWilliams parametrisation later in the abstract, so we would prefer to leave this as just "eddy-permitting ocean resolution"

L. 8: "unresolved eddy processes or the representation of bathymetric drag" -> and/or, since both can be (and are) causing issues at the same time.

We have included this change.

L. 9: Already here in the abstract, the authors mention the shallower isopycnal slopes of the eddy-permittng resolution, and it immediately caught my attention as being counterintuitive. Hence, I would have liked to see this acknowledged earlier in the paper itself.

See under General Comments point 5 above. We have discussed the counter intuitive nature of this result and the possible explanation in Section 4.2 in the revised manuscript. We have modified the title of this section to make the discussion of the slumping isopycnals more prominent.

L. 30-32: It would be appropriate to also mention the emergence of models with unstructured grid configurations, as they are an approach to overcoming the issue with affording high enough resolution to resolve high-latitude eddies in global models, e.g. FESOM (Wang et al., 2013; Scholz et al., 2019), ICON (Jungclaus et al., 2022;Korn et al., 2022).

We have added a footnote about unstructured grid models to L33 revised manuscript.

L. 61-62: “A free-slip boundary condition [...]” - It would be valuable to also mention what viscosity scheme is used, and potentially also the parameter settings since they likely differ between resolutions, as these also have the potential to affect the flow and thus the biases.

We have added a new table (Table 1 in the revised manuscript) with the viscosity and isopycnal diffusion parameters settings and referred to this in the main text.

L. 64-65.: “Diffusion of tracers along isopycnal surfaces, parameterising eddy mixing [...]” – Is there any regional reduction of the parameterised eddy mixing around the equator where eddies are fully resolved, in particular in the higher resolutions?

No. There is a reduction of the diffusion coefficient with reduced grid spacing at higher latitudes to avoid numerical instability. We have added a note to this effect in the new Table 1.

L. 75: “with the N216 atmosphere” - Are the results consistent or at least similar if one of the other atmospheric resolutions are chosen? This would indicate that the results are more widely applicable to other coupled models, regardless of what atmospheric setup they use.

We have not looked at the Southern Ocean biases in the other HighResMIP integrations in detail, but Roberts et al (2019) show that for the ACC transport, a change in atmosphere resolution makes little difference to the long-term behaviour (their Fig 18). For the SST biases there is some impact of atmosphere resolution (their Fig 7) as might be expected. We have added two sentences about the cross-resolution results of Roberts et al in the Conclusions (L386-390 of the revised manuscript).

Page 3, footnote 1: Was the eddy-permitting model ever tested with no-slip? It would be useful to motivate the choice of partial-slip over no-slip in this case, and discuss how choosing a no-slip condition might have impacted the overly strong ASC. Useful references may be Penduff et al., 2007; Deremble et al., 2011; Nasser et al, 2023.

The choice of free slip for the 1/4° model goes back to Barnier et al (2006) and Penduff et al (2007). We have not tested the 1/4° (or 1/12°) model with no slip, although with hindsight that might have been an informative thing to do. Any choice of lateral slip condition in z-level models is difficult to justify on a physical basis, and we tend to see this result as an indication that the large scale biases we are looking at may be linked to poor representation of bathymetry-flow interaction in the model.

L. 93-95: “As well as having more active gyres, the higher resolution models also have a stronger ASC [...]” - In the next paragraph, you mention the consequences of this feature in the Drake Passage, but only after you discuss the over-flattened isopycnals. This makes this part of the text feel somewhat fractured. Maybe mention the Drake Passage briefly already here, or rearrange the next paragraph to discuss the ASC behaviour before the overly flat isopycnal slopes.

Following the suggestion of reviewer 1 we have included an extra figure (Fig 2 in the revised manuscript) showing the depth-mean currents for the top 500m. This illustrates the circumpolar nature of the ASC in the eddying models. We have mentioned the westward flow in the Drake Passage when discussing the strong ASC.

L. 98-99: Klatt et al. (2005) is one of few observationally-based estimates of the Weddell Gyre strength published in recent decades. It is, however, not the only one. Reeve et al. (2019) estimate it to 32 +/- 5 Sv based on ARGO data. Older observational estimates by Farbach et al. (1991) and Yaremchuk et al. (1998) are also lower c.f. Klatt et al. Meanwhile, the transport in models ranges between at least 10-80 Sv (see e.g., Neme et al., 2021; Wang, 2013).

We have added a footnote to L109 (revised manuscript) referring to the Yaremchuk et al and Reeve et al estimates and justifying our choice of the Klatt et al. as being the most comparable observational estimate to our Weddell gyre strength metric.

L. 105-106: "The weaker ACC transport in the higher resolution models is associated with a flattening of the time-mean isopycnal slopes in the Drake Passage" - Given that particularly the ORCA025 resolution is not fully eddy-resolving in the ACC region, it is somewhat counterintuitive that there is an over-flattening of the isopycnals, which suggests too much eddy compensation. From reading the text here, it makes one wonder how that can be. As mentioned in the general comments, this counterintuitive behaviour is mentioned by the authors later in the manuscript, but should be acknowledged sooner. However, looking at the figures, it looks like the isopycnals are very steep (steeper than ORCA1) in the northern part of DP, where the core of the ACC is, and then flattened in the centre, where there is weaker (ORCA12)/counter(ORCA025) flow. In ORCA12, steeper isopycnals than in ORCA1 are also observed between -63 and -62 degN. These details should be mentioned, as they might help elucidate what is actually happening to the ACC transport.

For response to the point about slumping of isopycnals see General Comments, point 5 above. We have included a more detailed description of the isopycnal slopes in the Drake Passage in the revised manuscript (L117-123).

L. 107: "counterflowing currents [...] associated with the Shackelton fracture zone" – The authors mention that the modelled counterflow along the southern shelf break is unrealistic c.f. Meijers et al. (2016), but they give no indication of whether counter flows at the Shackelton fracture zone correspond to observations or not.

Xu et al (2020) show complex recirculations in the Drake Passage, especially at depth, in their 1/12˚ model and argue that these recirculations, if realistic, are challenging to sample with observational arrays. We have made this point in the revised text (L123-125).

L. 122-128: On resolution-dependent temperature biases - What are the differences in global mean surface temperature in these simulations, and compared to the observational dataset? As some of the biases in SST can stem from the atmospheric model, or results of the difference in resolution in other regions, it might be useful to make a supplementary figure where the biases are normalised to the observational global mean surface temperature.

The HadGEM3 HighResMIP models tend to have cold SST biases away from the Southern Ocean and these cold biases tend to reduce as the ocean resolution is increased (Roberts et al, 2019, Fig 7) mainly due to improved representation of boundary currents and ocean heat transports. We agree that the attribution of SST biases is more complex than for some of the other biases examined in the paper because of the direct influence of the atmosphere and possible teleconnections, but given the complexity of the global picture we aren't sure that a comparison with the global mean SST bias will be very informative here.

L. 132: "observational estimates of Klatt et al. (2005)" – as this is not the only observational estimate of the Weddell Gyre strength (see L. 98-99), this may need to be modified, or at least motivated why this particular estimate is used for comparison.

See response to L98-99 above.

L. 165: "in the eddy-permitting model [...] deep water formation has been suppressed" - Does this lead to (more) unrealistic open-ocean convection than in the other two model versions.

In the control runs, both of the eddying models develop regular open-ocean convection after a few decades. The 1/4° model seems to be no worse than the 1/12° model in this respect.

L. 168-169: Cold, fresh water advecting around the Antarctic peninsula in the eddy-rich model suggests the ASC and is too strong through the Drake Passage also in this model version.

We have added a note about the possible link between the westward flow in the Drake Passage and the cold, fresh biases in the Amundsen Sea. (L177-178 of revised manuscript).

L. 185-189: On introduction of the scale-aware G&M: What are the implications of running a resolution that to some degree allows eddy formation, but then also parametrising eddies on top of it using G&M, which can lead to "smoothing out" of the actual eddies? This should be clarified in the text.

The idea is similar to that in Hallberg (2013) where we only "switch on" GM where the model fails to resolve eddies, so that we don't smooth out the eddies where the model is eddy-resolving. But of course this is difficult to do in a precise way and the whole question of how to parametrise eddies in partially-eddying models is a big research topic as we have noted in Appendix B. We have modified the main text slightly to emphasise that we are only turning on GM where eddies are unresolved (in some sense) and to emphasise that we are testing quite a simple idea compared to some schemes in the literature (L206-212 revised manuscript).

L. 190-192: On the introduction of the partial-slip condition – As mentioned above, it would be useful to motivate why the choice was to go with partial-slip and not no-slip for this resolution. Also, based on the description of how partial slip affects the biases in ORCA025, it seems that introducing it south of 50S in ORCA12 could potentially fix the remaining biases with counter flows in the Drake Passage, and thus with cold waters being advected around the Antarctic Peninsula in this resolution as well.

As discussed under point 6 above we have tested scale-aware GM and partial slip in the Southern Ocean in a forced integration of the 1/12˚ model and the combination of these two changes does appear to reduce the Drake Passage counterflow in this model. We have added a note along these lines in the Conclusions (L391-394 of revised manuscript).

L. 223-224: "the Fresh Shelf, the Dense Shelf and the Warm Shelf. In Figure 7" - It would help guide the reader's eye if it were also indicated in the figure and/or the figure caption what column exemplifies which one of these three regimes.

We have added these labels to the figure.

L. 238-239: About the V-shaped pattern of isopycnals in the higher-resolution models - I struggle to find this V-shape in the drawn isopycnals in the eddy-permittng resolution. If so, I can only see it in the two isopycnals labelled 27.5 (this might be a mistake in the labelling, as it occurs twice).

We agree that the eddy-permitting model only has a shadow of the V-shape – and it also clearly fails to represent the cascade of dense water. We have updated the text to qualify our statement that the V-shape is present in both eddying models (L270).

L. 262-265: The EN4. 1 climatology does not show the same steep ispoycnal slopes near the continent as the model in this region but, as mentioned, the observations included in the climatology are sparse. It could be useful to cite other data (not included in the EN4.1 climatology) that give indications about the isopycnal structure in the area even if those are from from a later time period.

Pena-Moleno et al (2016) show observations with a strong ASC and associated front in the same sector of Antarctica. We have cited this reference in the revised text (L297 revised manuscript).

L. 310-320: On open-ocean polynyas - Are these events stronger/more frequent in the eddy-permittng c.f. the eddy-rich resolution, given that the latter appears to have some more capability of forming dense water on the shelves? (see also L. 165)

See response to L165 comment.

L. 332-336: About biases along the continental slope/shelf – Here, it would be helpful to clarify which factor is the more important in reducing these biases: adding G&M or introducing the partial-slip condition.

We have added a sentence about the relative impact of GM and PS in the first paragraph of the Conclusions (L371-373 revised manuscript).

Figure 1 (caption): See comments to L. 98-99 and L. 132 regarding Klatt et al. (2005)

See response to L98-99.

Figure 5: mean SST-lines - It is unclear to me which mean SST this refers to, and as the lines are completely unlabelled, there is no indication of what temperatures the different lines represent.

The lines in Fig 5 are mean SSH and are simply there to help locate the SST biases with respect to the gyres and the ACC. We have expanded the caption to make this point. At the request of reviewer 1 we have also added a dashed line showing the maximum ice extent to these plots.

Figure 9: In this figure, it might be more illustrative to show the model results as anomalies from the reanalysis data. In the other figures, observational datasets are shown in the bottom-right subpanel. It would be helpful to keep the same structure throughout the manuscript.

The main purpose of including this figure is to argue that the large scale magnitudes and patterns of the wind forcing are very similar between the different model runs and so the differences we see in the model response must arise principally from differences in the ocean model. We think that the plots of the fields presented here illustrate that basic point and we’re not sure that showing anomalies against JRA would add much. We have moved the JRA plot to the bottom right in line with the other figures as suggested.

Figures overall: It could be helpful with supplementary figures showing the differences between the standard N216-ORCA025 and the other two ORCA025 versions (GS and PM) as anomalies from the standard (GS-standard, and PM-standard).

We have provided the difference plots for the temperatures and salinities between the GM and PS test and the control as appendix figures and referenced these from the main text. (This was also requested by reviewer 1).

References

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