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

The authors present an analysis of the Southern Ocean state during the spin-up stage for three versions of the HadGEM3 model with two versions in which Gent-McWilliams and bathymetric drag are adjusted. The three versions of HadGEM3 include models which are "non-eddying" (nominal 1-degree), "eddy-permitting" (nominal 0.25-degree), and "eddy-rich" (nominal 1/12-degree). The "eddy-permitting" version is in the grey area of resolutions where no GM is utilized, but eddies are not completely resolved at high latitudes. Two additional versions of the "eddy-permitting" model are used to identify the role of mesoscale mixing parameterized by GM and bottom drag in simulating the Southern Ocean and Antarctic margin state. The authors find introducing baroclinic stability via GM or changing bottom drag results in a generally improved simulation for the eddy-permitting model. However, even with this bias reduction, for many of the metrics analyzed here, the Southern Ocean remains far from observed (very weak ACC, too strong ASC, too strong gyres, too fresh shelf).

The manuscript is well written, and the results are useful to the community. My main concerns are outline below followed by more specific comments.

General comments:

- If the authors wish to make statements regarding the ASC strength, this needs to be separated from the Gyre. The ASC strength should be assessed at particular locations along the slope or via the along-slope flow. See my notes below on this topic.
- Language of "too-strong", "too-weak". The comparisons are being made against a 1950-54 climatology based on EN4 ...... so, this language needs to be careful. For a 1950-54 climatology, observations over this period and particularly in the Southern Ocean are going to be extremely sparse. Thus, an assessment of realism using a 1950-54 climatology as "truth" is not accurate.
- 150 years is relatively short, have these simulations been ran out longer and the ASC does not weaken to lower values in the eddy-permitting model?
- Complementary analysis of DSW formation / export and AABW export would provide additional insight to the processes discussed in the manuscript and particularly the shelf hydrography. A disconnect between DSW formation and AABW export might provide some insight into magnitude of interior / spurious mixing.

Overall: I recognize this is a non-trivial problem and I am still puzzled by it. Even with the added GM and additional drag, the ACC is still exceptionally weak due mostly to a huge component of westward flow coming from the overly-large Weddell Gyre & ASC. Does the basal melt introduced at depth play any role in this? (thinking compared to models that do NOT account for this). Could the buoyancy introduced in this manner and the lack of resolved eddies play any role?

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.

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

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.

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.

L50-53: This paragraph is probably not necessary.

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

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).”

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?

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

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.” ← 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.

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.

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?

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)

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".

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.

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

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.

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.

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. The ASC could be computed as the total transport from the slope southwards at specific transects (this will also include the Antarctic coastal current too so this will not be perfect) (as in Moorman et al., 2020; Beadling et al., 2022) or as the total along-slope flow as in Huneke et al., (2022).

https://journals.ametsoc.org/view/journals/phoc/52/3/JPO-D-21-0143.1.xml

https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2021JC017608
https://journals.ametsoc.org/view/journals/clim/33/15/jcliD190846.xml

L133: remove "eastward" before "net" due to comments earlier.

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

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

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.

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"

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL103018
https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2021JC017608

You mention this below in lines 168 – 169 but it should be mentioned here or this discussion combined.

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. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022JC019105.

L180: Add "ocean" before circulation.

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?*

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

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.

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?**

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.

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.

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.

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

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.
- **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?
- **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 panels.

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"

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

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.”

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)”.

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

L257: Add comma after “in general”.

L268-270: Paragraph is short – suggest combining.

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.

L281: Add “ocean” before flow.

L284: Should this be “wind stress curl”, not “wind curl”?

L285: What does “medium resolution” imply in km?