Response to review comments on gmd-2019-106 'ACCESS-OM2: A Global Ocean-Sea Ice Model at Three Resolutions' - Geoscientific Model Development

Referee comments are in bold, author responses are in plain text, and changes in the revised manuscript are in italic.

Comments from Referee #2 (anonymous)

This is a thorough description of the performance of the ocean and sea-ice component of the Australian Community Climate and Earth Simulator (ACCESS-OM2). In my view this paper will be a really useful reference for anyone working with ACCESS but also for anyone looking for a good reference to illustrate the impact of model resolution on ocean simulations. The paper provides a nice overview of key ocean circulation features at horizontal resolutions of 1, 0.25 and 0.1 degrees. Even though it is well known that the circulation changes with resolution there are (to my knowledge) not too many examples of papers showing a systematic comparison of the global ocean circulation at non-eddying, eddy-permitting and eddy-rich resolutions.

The paper itself is clear and well written and perfectly fits the scope of Geoscientific Model Development. I strongly recommend publication subject to some clarifications of the minor points listed below.

Our thanks to the review for their insightful comments which have been implemented as detailed below. We found this review to be very helpful in improving the manuscript.

Comments:

1) Page 4, line 5: I was a bit surprised at the choice of 50 vertical levels for the ACCESS-OM2 and ACCESS-OM2-025 rather than 75 levels. Given that most HPC time is eaten up by ACCESS-OM2-01 the amount of time saved seems minimal. This seems to go against the philosophy outlined earlier, namely to keep the three resolutions as similar as possible. It would be worth explaining a bit more why this choice was made (e.g. 50 levels are sufficient at 1 and 0.25 degree as suggested in Stewart et al. 2017).
There are two main reasons why we retained a 50-level configuration at lower resolutions. The first reason is that we want our primary 1° configuration to be as close as possible to Australia's CMIP6 model, as noted in the introduction. The second reason is that enhanced vertical resolution is only required when the horizontal resolution is sufficient to resolve more baroclinic modes (Stewart et al., 2017). As suggested by the reviewer, we have added this justification to the manuscript:
The vertical grids are optimised for resolving baroclinic modes, based on the KDS grids recommended by Stewart et al. (2017), who suggest that finer horizontal resolution necessitates finer vertical resolution.

2) Page 6, line 10: Perhaps it would be worth noting why the choice of 0.004s-1 was made for the buoyancy frequency. Is this a typical value for this depth range?
This is the default value in MOM5.1. We have added a note:
(these three values are the defaults)

3) Page 9, line 5: “downwelling” → “downward”

done

4) Page 9, line 5: I suppose the river run-off is essentially climatological -especially during the first part of the forcing?
River runoff is from Suzuki et al. (2017); as explained by Tsujino et al. (2018) this is daily, interannually-varying runoff from the JRA55 land surface model at 0.25° resolution, adjusted to match the observational estimates of Dai et al. (2009). We have added this clarification:
JRA55-do also provides total runoff (river, calving and basal melt), at 0.25° resolution; river runoff is daily and interannually-varying (Suzuki et al., 2017), Greenland runoff is monthly climatological (Bamber et al., 2012), and Antarctic calving and basal melt are climatological means (Depoorter et al., 2013). Liquid runoff is deposited at the coast in the top 40 m of the ocean, whereas solid runoff and basal melt are deposited as liquid at the ice shelf edge at the surface. The total runoff is spread horizontally if needed to keep the flux below a threshold (see Sec. 2.3.1).

5) Page 9, lines 29-31: I am not sure I really understand what is being done here. This could be read as if salinities were being nudged to WOA +/- 0.5 psu whenever, restoring fluxes try to push SSS outside that range. Obviously this is not what is being done since in Figure 11 there are regions where the salinity mismatch exceeds 0.5 psu (e.g. in ACCESS-OM2-01 off Grand Banks, in the Arctic and at the entry into the Caribbean Sea). This needs to be explained a bit more carefully.
The SSS restoring flux is calculated using the SSS bias or +/-0.5 psu, whichever is larger. We have clarified this sentence:
The SSS restoring flux is determined from the difference between model and WOA13 SSS; the restoring flux is calculated from the maximum of this difference or +/-0.5 psu, in order to avoid excessively large fluxes.

6) Page 10, lines 26-27: Is there any particular reason why the period from May 1984 – to April 1985 is chosen? Was this a year that was reasonably neutral for most major indices e.g. ENSO. . .etc i.e. “normal" year-ish"?
Yes. A clarifying statement has been added.
This 12-month period was chosen because it is particularly neutral in terms of the major modes of climate variability (Stewart et al., 2019).

7) Page 10, line 33: Do you mean that the Kuroshio separation was too far north? Also I suppose this refers to the 0.25 and 0.1deg versions as WBCs are so diffuse at 1 deg?
Yes, this is exactly what we meant, but we were actually referring only to the 0.1° case. We have clarified this sentence:

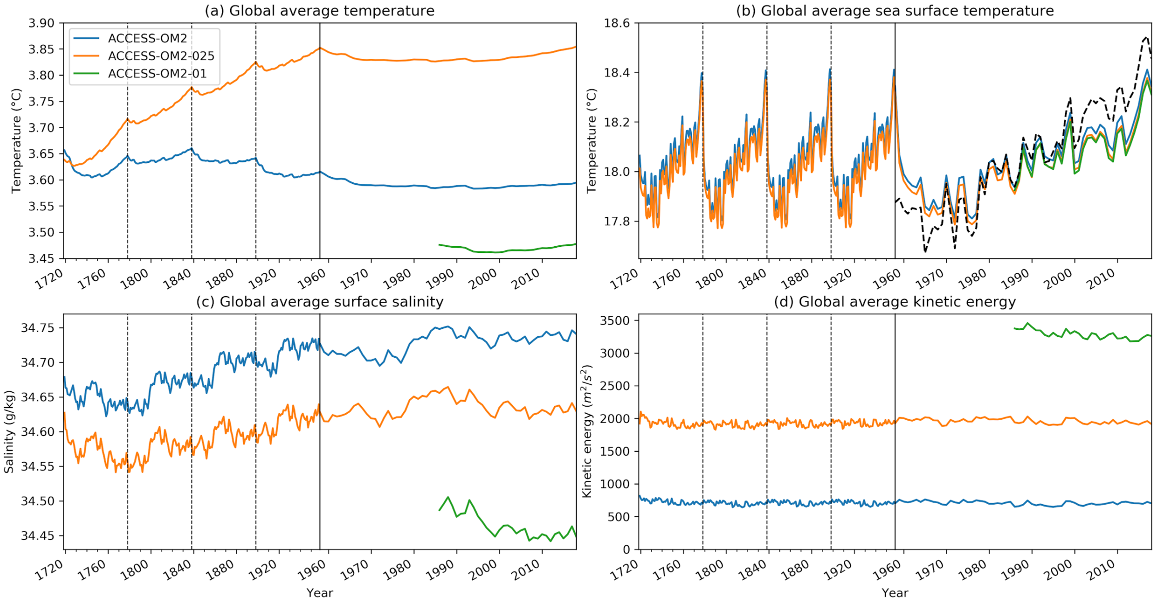
… produced a northward bias in the separation of the Kuroshio Current in the ACCESS-OM2-01 initial condition (i.e.\ the end of the RYF spinup), which largely disappeared under the subsequent interannually-varying forcing (Fig. 21).

8) Page 12, line 32: Is the timestep for ACCESS-OM2-01 400 or 450s? (The latter number is given in table 2).
The tests used 400s but the production runs used 450s as shown in the table. This has now been clarified.
In the MOM section:
The tests used baroclinic timesteps of 5400\,s, 1800\,s and 400\,s at 1°, 0.25° and 0.1° (respectively).
In the CICE section:
The tests used thermodynamic timesteps of 5400\,s, 1800\,s and 400\,s at 1°, 0.25° and 0.1° (respectively).
In table 2 caption:
Outline of model grid, size, cores and typical performance for production runs.
The timestep given is the ocean baroclinic timestep (at 0.1° this differs from the 400\,s timestep used in the scaling tests), which equals the ice thermodynamic timestep (but is three times longer than the ice dynamic timestep at 0.1°).

9) Figure 3: To me it seems that for the globally averaged SST (panel b) the last pass looks different from passes 1-4. In passes 1-4 the globally averaged SST varies between about 17.8C to 18.4C and looks very similar for all passes. However, in pass 5 the SSTs vary between about 18 and 18.4C. This seems quite a large difference for a global average. It is noticeable that ACCESS-OM2-01 starts from much colder conditions (panel c). this linked to the stronger MOC in ACCESS-OM2-01? At the end of the spinup the globally averaged temperature is about 0.2 deg colder than for the lower resolutions. Again, for a globally averaged value this is quite a big difference.

We thank the reviewer for pointing out the scaling error in the last cycle in Figure 3b, which has now been corrected; see revised figure below.

Regarding global average temperature (presumably the reviewer meant panel a, not c), the two coarse models begin from the WOA climatology, whereas the 0.1 degree configuration begins from a 40-year spinup under 1984-5 repeat-year forcing, resulting in a colder initial state (as stated on p 16, line 5).



10) Page 16, line 5: The initial cold drift cannot be seen in Figure 3a.

Thank you for pointing this out. The drift primarily occurs during the RYF spinup, so is effectively part of the initial condition for this run. We have rewritten this sentence as follows:

The ACCESS-OM2 and ACCESS-OM2-025 experiments and the RYF spinup prior to ACCESS-OM2-01 all start with nearly identical global average temperature from the WOA13 initial condition, from which ACCESS-OM2-025 drifts warm due to heat uptake (Fig. 3a), whereas ACCESS-OM2 remains relatively stable. ACCESS-OM2-01 is cold relative to the WOA13 initial state due to a cold drift during the repeat year spinup prior to the interannually-forced run shown in Fig. 3a.

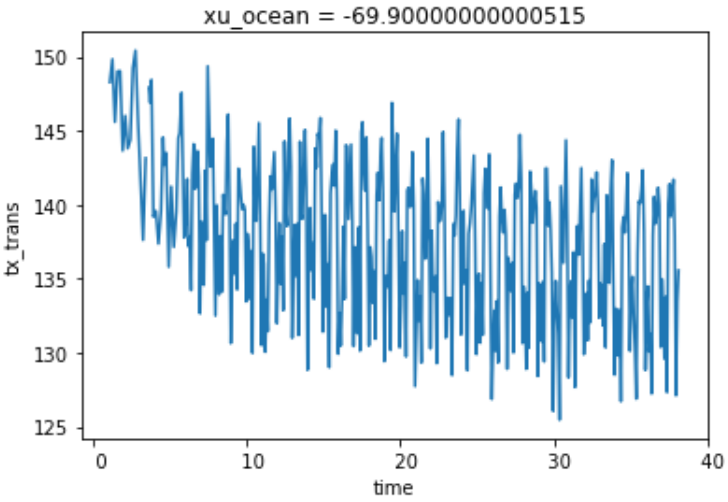
and also appended

(not shown in the figure)

to the sentence discussing SSS (fig 3c).

11) Page 16, lines 23-25: It is interesting that a large ACC variability is only really seen in the first pass for 1 and 0.25 degrees where there is a pronounced and broad peak in transport during the first pass which is not seen at 0.1 deg. Is there a spike in the AABW formation during the first pass in ACCESS-OM2/-025?

The variability in the first cycle of the lower resolution cases is indeed due to the formation of dense water in open ocean convection (as noted in section 4.1.4 of the manuscript). This open ocean convection does not occur in ACCESS-OM2-01. Accordingly, ACC transport during the preliminary spinup (reproduced in the figure below, but not shown in the manuscript) does not spike in the same way, but smoothly reduces towards the quasi-equilibrium value.



[referee did not provide a comment 12]

13) Page 18, lines 16-18: There are clear differences for e.g. the Gulf Stream at 0.1 deg. The SSH variability suggests that Gulf Stream path may be too variable just after separating from Cape Hatteras. The SSH variability is confined to a broad patch North of Cape Hatteras rather than extending further east along the extension as suggested by AVISO.

This sentence now points out these issues in the Gulf Stream region, which are discussed further in Sect. 4.2.4:

The SLA variability magnitude in ACCESS- OM2-01 is closer to the observational estimate but still somewhat low, and with a differing pattern in the Gulf Stream region (discussed further in Sect. 4.2.4); the highest values are found south of the African continent in the Agulhas retroflection region, and the Gulf Stream and Kuroshio extension.

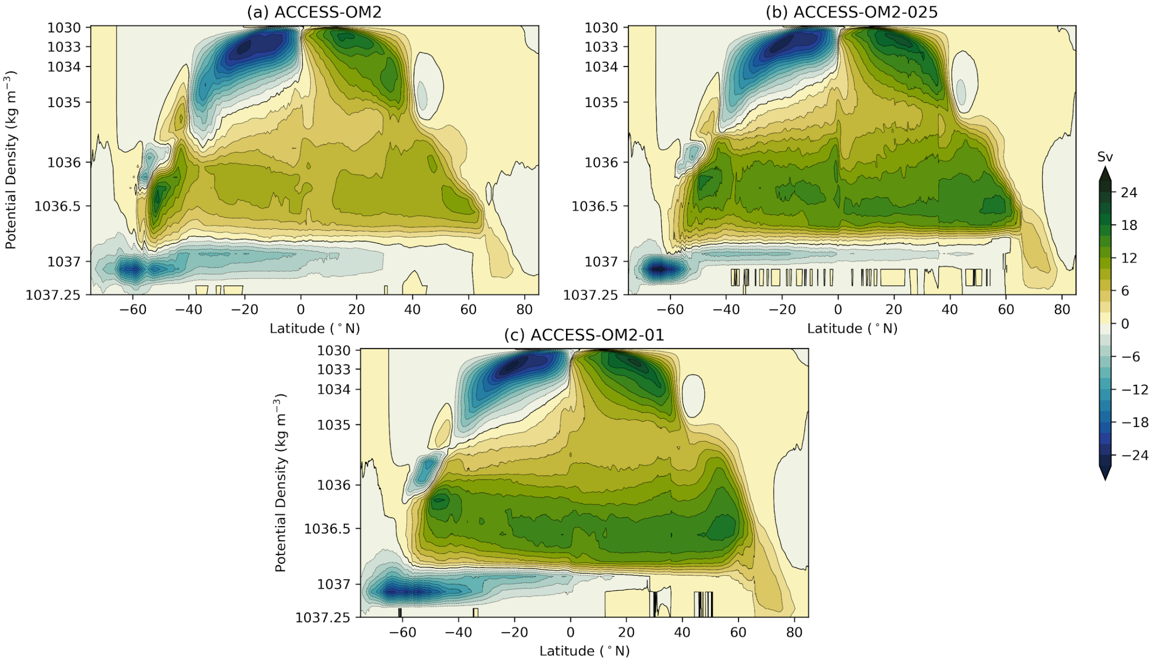
14) Page 18, lines 25-28: I don’t think that this can really be inferred from Figure 6. . .

The paragraph has been made clearer and now reads:

Fig. 6d also shows broad regions of enhanced sea level variability at lower latitudes, with less amplitude than the western boundary currents. These patterns are typically associated with slower modes of climate variability. ENSO cycles drive variability in the Eastern Equatorial Pacific, and the Western Pacific, east of the Philippines and Papua New Guinea (Han et al., 2017; Mu et al., 2018). All resolutions simulate these patterns of variability associated with ENSO, though they all underestimate the observed variability by 10–20%. In the Indian ocean, variability is associated with both the Indian Ocean Dipole and ENSO (Li and Han, 2015). Anomalies in the tropical South Indian Ocean (5–15° S, 60–80° E) are driven by ENSO-related wind anomalies (Xie et al., 2002) and the associated pattern of variability is simulated in each model resolution. Enhanced variability from the coasts of Indonesia (Potemra and Lukas, 1999) and the West Australian coast in Fig. 6d extends westward into the Indian Ocean due to Rossby wave propagation. The pattern of variability in ACCESS-OM2-01 is consistent with this, *although somewhat weaker, whereas the pattern is much more muted in the coarser resolution simulations.*

15) Page 18, line 30, Figure 7, Page 19, line 10: It would be nice to plot the overturning for the full range of densities i.e. not to cut off the lightest densities . I'd also suggest to expand the higher densities e.g. between 36.5 and 37.5 as this would show the AABW cell more clearly. This I feel could be relevant to understand differences in the ACC strength between the different resolutions (see comment 21) below.

Done - Fig 7 now shows the full density range and uses a non-linear scale to show the AABW cell more clearly:



16) Page 22, Figure 9: Is the data temporally filtered for the 1 and 0.25 degree resolutions? A seasonal signal is clearly visible in the surface layers of the 0.1 degree model but not at 1 and 0.25 degrees.

Yes, as stated in the caption, they are annual averages at 1 and 0.25 degrees, and monthly averages at 0.1 degrees.

17) Pages 23, lines 3-4, Figure 10: For some regions the anomaly patterns look quite different in ACCESS-OM2-01. For example in the Northern North Atlantic there is a large-scale warm bias and a strengthening cold bias in the southern SPG whereas the rest of the northern North Atlantic is warmer in ACCESS-OM2-01 than in the other cases.

We agree, and have added the following comment:

ACCESS-OM2-01 does, however, differ from lower resolutions in the northern North Atlantic ocean, with a stronger cold bias in the southern part of the subpolar gyre and a large-scale warm bias elsewhere; this is discussed further in Sect. 4.2.4.

18) Page 26, lines 7-8: To me the picture does not always seem as clear cut here. Between 0-30S ACCESS-OM2 has the best agreement with Ganachaud & Wunsch.
This sentence has been clarified and now reads
Nonetheless, the results show that ACCESS-OM2-025 has a clear advantage over ACCESS-OM2 in representing heat transport at most latitudes, with the possible exception of 0--30° S.

19) Page 27, line 15: Define all terms for the PV equation.
Done

20) Page 29, Figure 15: I suppose the maximum/minimum mixed layer depths are from September (max) and March (min)?
They are the maximum and minimum monthly mean mixed layer depths over the 300 months in the 25 years 1993-2017 (rather than climatological monthly max/mins). The caption has been amended for clarity:
black (blue) lines represent the minimum (maximum) of the monthly mean mixed layer depth (defined by a 0.03\,kg\,m^{-3} density criterion) over 1993--2017.

21) Page 34, Figure 20: I can see that there seems to be a problem with the Antarctic mode water at the lower resolutions. However, what is equally pronounced in my view is the cold bias seen south of about 60S. In addition there is also a weaker cold bias at depth extending from the high southern latitudes to the northern end of the domain for the 1 and 0.25deg resolution versions of the model. This is also seen for the latitude-depth sections through the Atlantic and Indian Oceans (Figures 23 and 25) as well as for the zonally averaged temperatures shown in Figure 12. Could this explain why the ACC transport is weaker in ACCESS-OM2-01 than in ACCESS-OM2? The cold bias around Antarctica increases the meridional density gradient across the ACC which may explain the higher ACC transport in ACCESS-OM2 (where the cold bias is strongest). The coldest bias around Antarctica in ACCESS-OM2 may seem at odds with the overturning cell associated with AABW formation shown which is weaker than for the higher resolutions (Figure 7). However, my impression is that although weaker the overturning associated with AABW involves higher densities at 1 deg than at 0.25 and 0.1 deg. This would come out more clearly if the overturning is expanded for higher densities in Figure 7 (see comment 15).
We agree with the reviewer that the deep cold bias is significant. This issue was primarily dealt with in section 4.1.4, in which we propose that unrealistic open ocean convection is key here. The reviewer is correct to infer that this bias may be relevant to the ACC transport differences between the resolutions, and we have added this suggestion as a new added a sentence to the end of this section, which now reads:
In the Southern Ocean, the signature of Antarctic Bottom Water (AABW) shows a cold bias in ACCESS-OM2 and ACCESS-OM2-025 that spreads into the abyssal ocean. This bias is likely associated with large areas of anomalous deep (often full-depth) convection that appear every winter and spring in the eastern Weddell Sea and western Ross Sea in the ACCESS-OM2 and ACCESS-OM2-025 simulations. The behaviour of the two coarser models is typical of CMIP5

models, which produce bottom water by spurious deep-ocean convection rather than down-slope flows (Heuzé et al., 2013). In some models this convection is associated with spurious open-ocean polynyas (Heuzé et al., 2015); however, as in the GFDL CM2.5 model (Dufour et al., 2017), persistent open-ocean polynyas do not form in the ACCESS-OM2 simulations. The deep cold bias is much reduced in ACCESS-OM2-01, which has a more realistic AABW formation in the Antarctic continental shelf, with anomalous open-ocean convection confined to a much smaller and more interannually-variable region in the northeastern Weddell Sea (but has also had less time to drift away from climatology). The differences in Southern Ocean convection may partially explain the stronger ACC transport in the lower resolution configurations (Fig. 4).

22) Page 36, lines 8-9: Note that there are also uncertainties in the observational estimate of the barotropic streamfunction of DeVerdiere & Ollitrault. So I suppose that rather small features of the barotropic streamfunction such as this recirculation have to be taken with care.

This paragraph has been rewritten and no longer contains this statement.

23) Page 42, lines 3-4, Figure 27: The sea ice decline only seems too slow for ACCESS-OM2-025. For 1 and 0.1 deg there is a small positive bias compared to the observations that remains almost unchanged during the simulations but the long-term sea-ice decline looks very similar.

We agree that the Arctic sea ice extent decline in Fig 27a seems slower at 0.25deg than at the other resolutions, and have added this sentence:

The long-term trends in sea ice extent are also tracked by the 1° and 0.1° configurations, but the Arctic decline is slower than observed at 0.25° (Fig. 27a).

24) Caption Figure 16 (and other Figure captions): I suggest to replace “overlain” with “overlaid”.

Done. Changed “overlain” to “overlaid” throughout.