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Interactive comment on "ACCESS-OM2: A Global Ocean-Sea Ice Model at Three Resolutions" *by* Andrew E. Kiss et al.

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Review Comments on 'ACCESS-OM2: A Global Ocean-Sea Ice Model at Three Resolutions' – Geoscientific Model Development

Comments from Pat Hyder (Met Office)

I recommend publication with minor changes. This is an extremely thorough and generally well written documentation and assessment of the ACCESS ocean-only configurations at three ocean model resolutions. I have some general and minor comments, which I have included below.

General comments

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Discussion paper

1) The paper is very thorough and present a wealth of interesting results. As a result it is fairly long so I have not been able to comment on the wording of all the individual sentences. However, my feeling is that in quite a few places sentences are rather long and could usefully be split into two or more sentences to aid clarity. I also thought in several places the inferences seemed to me at least to be a bit speculative so could perhaps from a few more caveats.

2) This paper appears to suffer from the same frustrating difficulty in identifying clear benefits of ocean model resolution that all similar coupled and ocean-only studies have experienced. As previous studies, the only clear benefits are seen in metrics related to boundary and frontal currents and associated eddy variability. These difficulties are no surprise because both ocean forcing sets and coupled atmospheric models have large errors, giving rise for enormous potential for error cancellation between forcing set errors and changes due to ocean model resolution. This seems likely to be what is giving rise to a mixture of improvements and degradation that all studies of coupled and ocean-only simulations find. A few more points on model errors are included below:

- Errors are often no smaller in ocean forcing sets than unconstrained atmospheric models, particularly in radiative fluxes and near surface air temperature and relative humidity. This is because all of these parameters are virtually unconstrained by atmospheric data assimilation and therefore in reanalyses, such as JRA-55, they just reflect the underlying model atmospheric biases. The reason SST biases are smaller in ocean-only simulations is simply that the SST are far too strongly tied by surface fluxes to prescribed air temperatures from the re-analyses (e.g. Hyder et al 2018, Nature Communications), which are in turn tied to the observed SSTs used in the re-analyses.

- For ocean-only models the problem is compounded by the deficient surface boundary layer which assumes that the near surface atmosphere has large heat capacity compared to the underlying ocean which is clearly incorrect. Potentially this could give rise to errors, including substantial over-estimation of ocean convection. Also in the real world, coupled feedbacks of the ocean onto the atmosphere (and associated energetic constraints) are important so it is not clear that resolution-dependence can be adequately represented in ocean-only simulations. For example, we find the resolution dependence of SST biases, i.e. differences between configurations at different ocean resolutions are completely different in ocean-only and coupled simulations.

In my view, it might be good to set out these problems in the introduction and refer back to them in the conclusions?

3) Surprisingly, this study does not find the commonly-seen benefits in the Gulf stream northward turn representation at high ocean resolution, or the very cold bias in the this region at low resolution, e.g. see Storkey et al, 2018, fig 16 (www.geosci-model-dev.net/11/3187/2018/gmd-11-3187-2018.pdf - it might also be worth referring to this ocean-only resolution study) or Hewitt et al 2016 or Griffies et al studies of GFDL coupled models. It would be great, if possible, to understand better why this is, perhaps the formulation of the slip coastal/topographic vorticity boundary condition and formulation of Coriolis, momentum advection or viscosity? Do the GFDL coupled (or ocean-only) simulations have similar issues in this region?

Minor Comments

All of the following suggested changes are optional from my perspective. There are also lots of comments and questions, which don't require changes or need including (unless you think anything is relevant). Feel free to e mail me if any of the comments are not clear.

Abstract – Is it worth making it clearer in the first sentence that these are ocean-only simulations?

Page 2, line 7 – It might be useful to include a paragraph or two setting out the issues with ocean-only simulations including errors in the forcing set and surface boundary condition that hinder the identification of clear resolution dependent benefits in most studies (see general comments above)?

Page 2, line 8 – Is it worth changing it to read 'is currently 1o'

Page 4, line 8 – Since both vertical and horizontal resolution are changed, it is worth saying something along the lines of 'so we cannot distinguish between these changes in our results'?

Page 5 – Is it worth stating that bathymetric inconsistencies could contribute to resolution-related differences in performance, particularly near topography or coastlines?

Page 6 – Parameterisation section – As you mention later in the paper, it would be great, if possible, to better understand the rather unusual changes in representation of the Gulf Stream with resolution in your models. Your low resolution model does not have the common extremely cold bias in the northward turn region (which is up to 5 degrees in other models). However, you also do not appear to see the representation benefits with increasing resolution that are seen in other systems, in particular, much reduced cold biases in the northward turn region. Typically, Gulf Stream, and associated eddy variability, are influenced by viscosity, formulation of slip condition, GM if it's used, etc. One might expect the Gent-McWillliams parameterisation that you use to damp the Gulf Stream and eddies in the $\frac{1}{4}$ degree model, making it behave more like the low resolution model (this is why we haven't used it so far). We are also wondering if the high resolution model might be adversely affected by the no slip condition (we use free slip at all resolutions)?

Page 6 line 32 – Does 'as a consequence of the B grid' mean that one cannot formulate a free slip condition on a B grid?

Page 7 line 5 – Is there a more plain English way of say 'obviating', i.e. perhaps avoiding or removing?

Page 8 – Is it worth defining 'roundrobin'?

Page 8 line 32 – Is it really true that there is evidence that the JRA-55 forcing set is

a major step forwards compared to CORE-II? From what I understand, all re-analyses have major issues, particularly with clouds and associated downward shortwave and longwave forcing and the near surface boundary layer including near-surface temperatures and humidities. These errors are not corrected for by the atmospheric data assimilation in the re-analyses and from what I have seen in met office assimilative models are largely unaffected by increased atmospheric resolution. The corrections one can apply to these deficiencies are rather crude, in part due to the very limited near surface observations, and often not physically-based so my suspicion is that errors in the JRA-55 forcing set are of a similar magnitude to those in CORE-II.

Page 9 line 14 - Do you find that using relative winds rather than absolute winds over-damps eddies and EKE? We also use relative wind stress to be consistent with our coupled configurations but I think the DRAKKAR NEMO ocean-only community often to use absolute winds in ocean-only for this reason.

Page 9 line 19 – is there a reason to use the slightly higher surface albedo than CORE-II or is this tuning?

Page 9 line 28 – Is there not still substantial global mean salinity and sea-surface height drift due to the global mean imbalance between prescribed precipitation minus prognostic evaporation and the prescribed run off?

Page 10 line 20-22 – Is it worth clarifying this sentence?

Page 10 line 22-24 – We tend to start sea-ice off from the restart file of a separate spun up run to avoid contaminating the salinity with melt/formation as the ice-spins up. However, perhaps this does not matter too much when you run the models for such a long time?

Page 10 line 26 – Might it be useful to explain your different run lengths for the different models before you discuss the initial conditions?

Page 11 line 2 – Perhaps you could say ‘coupled ocean-sea-ice model’ to make it

clearer that it's not a fully coupled model?

Page 12 line 22 – Would it be useful to state the years per day for the medium resolution $\frac{1}{4}$ degree model?

Page 12 line 31 – Is it worth defining 'sectrobin' and 'roundrobin'?

Page 14 line 16 to Page 16 line 3 – Do you think it might be worth calling this section experimental strategy and moving it to just before you discuss the initial conditions? In my opinion, this would be a bit clearer.

Page 16 line 2-3 – Is it worth clarifying the last sentence in the paragraph?

Page 16 line 6-7 – I guess one might expect SST to be tightly coupled to forcing in ocean-only configurations since the hugely over-active surface flux response to SST changes ties the SST strongly to prescribed air temperature (e.g. see Hyder et al, 2018, Nat. Comms)?

Page 16 line 13 – Is it worth changing 'by the following analyses' to 'in the following sections'?

Page 16 line 23-25 - Is it worth clarifying these two sentences and adding a bit more explanation?

Page 16 line 26-29 – Is it worth clarifying this text, perhaps by splitting the sentence into two sentences?

Page 16 line 32 – Is it worth changing this text to something along the lines of 'but a shallower MDSL, i.e. larger errors compared to AVISO, in the ACCESS-OM2-01 case.'
Page 17 Figure 5 – This is a great figure but I am wondering if also plotting differences compared to the observational estimates might help to highlight the resolution dependence differences?

Page 18 line 1-4 - It might be useful to state the frequency of the data used to generate the standard deviation (i.e. are they daily data)?

Page 18 line 19 – Is it worth adding 'as expected since the eddies are not resolved but parameterised in the low resolution configuration'?

Page 18 line 20 – Change to 'of larger sea-level variability. . .'

Page 19 line 15 and Page 20 line 3-4 – Can the dense shelf water really cascade down the slope accurately in the high resolution model since it still uses z-coordinate in which the bottom boundary layer is not at all well resolved (or is it perhaps also localised spurious but intermittent convection, perhaps in polynyas, that is making the bottom water)? In the UK ORCHESTRA project 1/12th degree sector model we find one need to use a sigma coordinate to get the dense water to cascade down slope, even at high resolution.

Page 21 line 12 – Is there perhaps also a contribution from numerical mixing?

Page 22 line 6 – Perhaps SSTs and trends might also be expected to be tied to any trends in air temperatures in the forcing set over the forcing period in an ocean-only model?

Page 23 line 9 comment – In the Gulf Stream region, your models seem to behave rather differently to other ocean-models. For example, your low resolution model doesn't seem to have the typical very large ($\sim$5 degree) cold bias seen in our low resolution model (Storkey et al, 2018) but also doesn't show the clear improvement in this region with resolution. This could be due to the experimental design and particularly the new JRA55 forcing or the long spin ups you use. However, as I discussed in the earlier comment on the parameterisation section, it could also be due to your configuration. In particular, we wonder if the $\frac{1}{4}$ degree model might be adversely affected by GM and all resolutions might be adversely affected by the no slip condition (see earlier comment for details).

Page 23 line 21 – Is it worth trying to explaining why you think it is due to the vertical diffusivity? From what I understand, the upwelling regions often get better, i.e. colder,

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at higher resolution? However, there are often substantial cloud biases in upwelling regions in atmospheric models and hence ocean forcing sets, usually too much downward short wave – this would be consistent across resolutions but I guess potentially could give rise to error cancellation?

Page 23 – line 33-35 –Does the z coordinate model really allow the dense water to cascade down the slope correctly (see earlier comment on this)?

Page 24 line 8-11 – Is it worth clarifying this sentence, perhaps by splitting it into two sentences?

Page 25 line 4 & fig 13 – This probably doesn't make much difference for your long averaging period but does the Trenberth and Carron method assume equilibrium, i.e. no heat content tendency.

Page 26 line 7 – To me this seems to me like quite a strong inference from the results?

Page 27 line 5 – Could you perhaps use 'comprehensive' instead of 'exhaustive'?

Page 27 line 8 – Would it be clearer to change this text to 'A significant motivation for moving..'?

Page 27 line 9 – Is it worth changing this to read 'mesoscale variability plays a critical dynamical role in the evolution. . .'?

Page 28 line 5 - Is it worth adding 'representation of surface boundary condition in ocean-only runs' as a possible factor? For example, the prescribed air temperatures effectively give air an infinite heat capacity compared to water, which is clearly wrong. In the real world or a coupled model, air temperature adjusts very rapidly towards SSTs because of the much smaller heat capacity of air compared to water.

Page 28 line 7-10 – Is it possible to clarify this sentence and the associated Fig 15? Perhaps you could also label the regions you describing with arrows on the plot as I could not really follow exactly what you were referring to?

Page 29 line 1-3 – Could this sentence be clarified?

Page 29 line 4 and Fig 16 (and all subsequent sea level STD plots) – Might be worth mentioning the frequency of the data used for the standard deviations?

Page 29 line 6 and Fig 17 – Could this suggest you need higher still resolution to represent the EAC?

Page 29 line 10-11 – Is it worth clarifying this text: 'indicator of the overall robustness of the model for this region'?

Page 32 final paragraph – Some of the inferences seems a bit speculative and could perhaps benefit from more caveats?

Page 34 line 3-8 – Is it worth adding a caveat to the final inference as it seems a bit speculative, to me at least?

Page 36 line 3 and Fig 22 – See earlier comments on representation of the Gulf Stream and its dependence on configuration (the viscosity and slip condition in particular).

Page 36 line 13 – Perhaps you could say 'the loop current in the Gulf of Mexico'?

Page 37 Figure 22 – Again the Gulf Stream eddy variability improvement with resolution does not seem as good as often seen, e.g. by GFDL in their coupled models?

Page 39 line 1 and Fig 25 - From the figure it appears that the biases are much reduced in the high resolution model? This did not come over clearly to me from this paragraph so could the text be clarified?

Page 40 line 7 – Is it worth referring to Fig 27 in first sentence and changing the text to include something along the lines of 'acceptable and very similar sea-ice extent and concentration at all three resolutions? I guess one might expect this because the sea ice is tied to the forcing fields in ocean-only runs by dramatic change in air temperature over ice and non-ice regions in the re-analysis, which are in turn tied to the observed

sea-ice it uses as a lower boundary condition.

Page 44 line 2 – Perhaps this text could be changed to ‘ocean mixed layer and associated SST and SST biases’?

Page 45 line 1 – Summary - It is worth starting by referring back to the introduction and in both sections summarising the pros and cons of ocean-only versus coupled, problems with forcing and surface boundary condition errors and substantial potential for error cancellation which makes it hard to robustly identify resolution related benefits.

Page 45 line 1 – We use the term ‘traceable’ to refer to minimising configuration changes between resolutions.

Page 46 – line 3 – Again, is it worth stating that with the exception of the boundary currents and strong eddying regions, where resolution related benefits appear to be clear, one sees a mixture of improvements and degradation. This might be expected from the substantial errors in ocean forcing sets and the surface boundary condition, which provide a lot of potential for error cancellation.

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