

Interactive comment on “The impact of resolving the Rossby radius at mid-latitudes in the ocean: results from a high-resolution version of the Met Office GC2 coupled model” by Helene T. Hewitt et al.

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This manuscript describes the development of a version of the UKMO GC2 coupled climate model with enhanced resolution in both atmosphere and ocean, as well increased coupling frequency. The development of this model is a significant achievement - and at 1/12° ocean resolution is, to my knowledge the highest resolution coupled model available. In addition, and contrary to many previous coupled models with high ocean resolution, the authors systematically include the effect of enhanced atmospheric resolution.

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However, the technical achievements outlined here are not quite matched by the depth of analysis of the model results. In many cases, changes between results from different simulations are causally attributed with only a superficial analysis. I accept that, for GMD readers, the attribution of different physical effects may be of secondary importance to the technical achievement; but if the authors want to imply causality then a more rigorous analysis is required. In a number of cases (details below) the authors could sidestep this issue by rephrasing the text - i.e., by making it clear that they are speculating on the cause rather than attributing, and pointing out where additional experiments will enable them to resolve the uncertainty. If these issues are addressed I would be happy to recommend this paper as a suitable contribution to GMD.

Major comments

On p.4 (line 26) it is noted that the transition from ORCA025 to ORCA12 is accompanied by a reduction in the isoneutral diffusivity from 300 to 125 m^2/s . It would help to have a justification of this change - in particular, if eddies are fully resolved, why do we need isoneutral diffusivity at all? If it is needed, then on what basis do we choose 125? This question is relevant because, for example, the reduction in SST biases is attributed to resolution (p. 6, line 24). However, this result (at least for the Southern Ocean warm bias) might alternatively be attributed to reduced parameterised upwards eddy heat flux. This effect may be consistent with the analysis on p.11, which shows a reduction in the time-mean southward heat transport at southern latitudes. And, finally, in the discussion there is reference to previous experiments in which changes in isoneutral diffusivity are associated with high-latitude cooling, but the authors argue that this "is believed to be a secondary effect" due to the long timescales associated with that paper.

This is one example where the authors need to pose one of two possible causes (with further experiments to tease out the root cause) or else perform a more in-depth analysis. For this case there are some clear, but simple, tests which could be performed. The isoneutral diffusivity contribution to the southward (or upwards) heat flux could be

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calculated explicitly. Alternatively, this question could be resolved by one additional GC2.1 simulation with reduced isoneutral diffusivity.

I wasn't entirely convinced by the description of the MOC changes (bottom of p. 9). Firstly, it is argued that changes are dominated by the cell associated with NADW - this may be true, but the other cells are not shown. This manuscript would be much more complete if the full MOC were shown, including the Southern Ocean (which would require transforming the overturning analysis into density space). In addition, the attribution of both NADW formation and Denmark Strait outflow increases to higher resolution seems fraught; the GC2.1 case sees a modest increase in both of these quantities, implying that the higher coupling frequency is partly response for the changes.

On p.10, l. 17, the ACC transport increase at higher resolution is noted as being consistent with both enhanced NADW and the Weddell Sea polynya. It seems unlikely that NADW formation can affect ACC transport in a short 20 year run (see Allison et al., JMR, 2011) - meaning that it is most likely that the Weddell Sea effect is dominating. Either way, both effects probably need to be supported in the form of a reference to existing literature. On a similar note, it seems likely that the small ACC transport in these simulations may be linked to weak AABW formation because of the Southern Ocean SST bias. This point could be further clarified if the full MOC were shown as suggested above.

The paragraph starting on p. 12, l. 29, is somewhat unconvincing. The case is made that refining both atmosphere and ocean resolution is important to gain the full benefit of resolution improvements. Yet, for almost all the metrics shown here, the N512 case showed only minor differences from GC2 (as noted in the first paragraph in this section). It may be that there are other metrics on which the N512 case performs well, but they are not shown here, so should not be included in the summary of this paper.

Minor points: - p. 4, line 22: I'm not sure I would call this an aspect ratio. Maybe just ratio?

- p. 6, line 9: It seems to me that the Southern Ocean SST biases here are larger than they were for CMIP-5. If true, then this should be explicitly stated, along with a reference to the published bias (it looks as if you're hiding something by stressing the pattern, rather than magnitude, of the bias).
- Several times through the manuscript the N512O12 simulation is listed as N512-ORCA12 - best to be consistent if possible. (p. 6, l.24; p. 7, l.24; legend of Fig. 5,)
- p.10, l.31: isopycnal -> isopycnal
- p. 11, l.4: There are four instances of "change/s" in the one sentence here, which becomes a little repetitive.
- p. 12, l.17: I'm not convinced that we expect more slumping of ACC isopycnals in the eddy-resolving simulation - changes in eddy KE are more likely to control the ACC through enhanced vertical momentum transport - but if there is a previously published expectation supporting this statement then I suggest a reference.
- The reference to "seamless" prediction makes little sense to those outside the UKMO community, and I suggest it should be either explained to great depth, or removed.
- p. 35, l.6: specify "north pole".

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