Review by Andy Hogg

We thank Dr Hogg for his constructive comments and address them here. References refer to the manuscript with tracked changes.

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.

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.

We have made a number of revisions which we hope address the 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 mˆ2/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 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 calculated explicitly. Alternatively, this question could be resolved by one additional GC2.1 simulation with reduced isoneutral diffusivity.

- We agree that the isoneutral diffusivity needs more discussion.
    - We include the following text on p5, lines 7-11 'While reducing the isoneutral tracer diffusivity is consistent with the increase in resolution, we note that results may have some sensitivity to its magnitude. Experiments to investigate the impact of this parameter in GC2 were not performed but will be pursued in future work with GC3 (the next version of the coupled model).'

- In the discussion, the following text has been included on p14, lines 28-32 ‘Given that the results here exhibit some consistency with those of Pradal and Gnanadesikan (2014) in the Southern Ocean, further work is required to quantify the role of isoneutral diffusivity in producing changes in SST on decadal timescales.’

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.

- Unfortunately, we did not have 5 day means of the full velocity field saved to allow calculation of the overturning in density space. As many authors have shown, this field is not entirely meaningful unless the eddy component is included. Calculations of the overturning in density space with monthly means suggest that the changes in NADW due to resolution and coupling period are robust but we can’t be certain about the AABW cell. The 5 day mean velocity fields was an oversight in setting up the model diagnostics and we will address this issue in future runs with the GC3 model for CMIP6. A comment has been added to the paper on this point (p10, lines 28-30).
- We have however modified the text on p10, lines 21-26 to reflect that the coupling frequency plays a role as well as on p11, line 6 and p14, lines 12-13

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.

- P11, lines 20-23 we have included a reference to Jones et al. (2011) who show that transient responses in the ACC can be seen within 10 years in their idealised experiments.
- References have been added for the NADW and the Weddell Sea links to the ACC (p11, lines 17-18)

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.

- The paragraph p15, lines 11-17 has been changed in response to the comments to state that ocean resolution and coupling frequency is important and states that further work is required to quantify whether a high resolution atmosphere component is required.

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

- P4, line 29 aspect deleted

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).

- Comments on the magnitude of SST biases in GC2 have been added on p6 lines 21-25. Basically the SST warmed everywhere in GC2 relative to the CMIP5 model HadGEM2-AO (see figure 1 of Williams et al., 2015) leading to improvements in the Northern hemisphere and degradations in the Southern hemisphere.

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)

- Corrected to GC2.1-N512O12 in text and figures 5, 7, 8, 11, 12

p.10, l.31: isopyncnal -> isopycnal

- Changed isopycnal to isoneutral p12, line 3

p. 11, l.4: There are four instances of "change/s" in the one sentence here, which becomes a little repetitive.

- Text on p12, lines 18-22 changed to improve reading

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.

- Text has been changed on p13, lines 27- p14, line 1 to reflect the discussion on the ACC

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.

- P15, line 18 seamless removed

p. 35, l.6: specify “north pole”.

- P49. North Pole specified