Review by Stephen Griffies

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

This is generally a well written and concise entree into a suite of coupled climate models run for 20 years. To my knowledge the finest resolution model, GC2.1-N512O12, is the state-of-the-science, at least for global models run for more than a few years. This point is worth emphasizing.

- A sentence has been included (p3, line1) to emphasise that with the resolution of atmosphere and ocean components and hourly coupling, this model is state of the art.

The tasks required are immense to produce a sensible simulation, even for the rather brief 20 years considered here. I applaud this effort, though note it is far from complete!

Yet as an introduction to the model suite, this is a useful contribution to the literature, and it provides an important peer-reviewed touchpoint for the developers. This manuscript is appropriate for GMD. I recommend publication after minor revisions.

Others who have developed models of this resolution with refined coupling periods (hourly or smaller) sometimes have problems related to coupled ocean/sea ice instabilities as discussed by Hallberg (CLIVAR Exchanges, No.65 (Vol 19 No.2) July 2014). It would serve the reader to know if you encountered any similar instabilities, and if so, what methods were used to suppress them. If you did not encounter such instabilities, it would be useful to state that as well.

- We did not experience instabilities associated with the hourly coupling. This is stated (p4, lines 24-25)

The Weddell Sea polynya in GC2.1-N512O12 warrants more discussion. In similar models at GFDL, we have seen that such polynyas can increase ACC transport, much as noted on page 10, lines 10-22. Do any of the other models in your suite have a polynya? Is the polynya large in area and going very deep? How long does it last? I am puzzled that the SST biases in Figures 2 and 3 show no sign of the polynya. In other models I have seen, such polynyas increase SST due to release of mid-depth heat. That SST signature is missing here. Perhaps the polynya is only for a year or two, and is averaged out by the 10 year mean? Please discuss, as this is an important feature to expose.

- Further investigation shows that the polynya first appears in year 12 and varies from year-to-year. In years without a polynya, the maximum mixed layer depth is less than 1000m. However, in year 15, the maximum mixed layer depth is 1556 m and in year 20 it is 2070m. More information about the polynya and its variability is given on p8, line 7, p10, lines 6-10 and in the new figure 9.

Pg1, line18: I appreciate that it is the surface ocean that the atmosphere cares about, and the sentence is referring to air-sea fluxes. But the sentence can be construed, incorrectly, to mean that ONLY surface eddies and boundary currents are necessary to do air-sea coupling right. As the authors show in this paper, there is more to air-sea *fluxes than the surface ocean. For example, overflows and the AMOC are key. So I recommend finding a different way to write this sentence.*

- Sentence has been changed in abstract (p1, line 17-18)

Pg1,line23: Admittedly a picky point, but worth being precise: hours are listed here as "frequency" for coupling (1-hour versus 3-hour). In fact, these are the "coupling periods" not the "coupling frequencies".

- P1, line 23 frequency changed to period (also p4, line 14 and in table 1)

Pg3,line5: Behrens et al. (2013) should be Behrens (2013). This citation refers to a single-authored PhD thesis.

- P3, line 8 reference corrected

Pg4,line9: again, "3-hourly to hourly" refers to "coupling period" not "coupling frequency

- P4, line 14 corrected to period

Pg4,line11: it is not clear what model is being referred to here when discussing the time step. I assume the ocean, but it should be clearly stated.

- P4, line 16 ocean inserted

Pg4,line27: Viscosity is a positive number. The biharmonic operator carries the negative sign. Please change. Doing so will also make the sentence correct. Namely, it presently reads "a reduction in the bilaplacian viscosity from -5e11 to -0.25e10". With the minus sign, this is not a reduction, but an increase! Again, just drop the minus signs on the viscosity so that all will make sense.

- P5, line 4 viscosity sign corrected

Pg5,line5: Including tides generally increases the flow speed in simulations. So what you mean here is that there is missing "tidal dissipation" in the model. That is, you are not suffering from missing tides, but instead suffereing from missing tidal dissipation.

- P5, line 15 tides changed to tidal dissipation

Pg5,line6: what is "atmospheric theta"? Please define the jargon.

- P5, line 17 theta changed to temperature.

Pg5,line28: what sort of "instabilities" do you find enhanced with the finer atmosphere? Those instabilities discussed earlier near the UK due to missing tidal dissipation? Something else?

- P6, lines 7- 8 clarification added to the instabilities

Pg8,lines10-11: More heat into the ocean interior is NOT what Griffies et al (2015) found with the GFDL CM2.6 simulation (1/10th degree ocean) relative to the coarser ocean (1/4th degree) in CM2.5. Instead, enhanced mesoscale eddy actitivity led to less heat entering the ocean. So...why does GC2.1-N512O12 get warmer in the interior *than GC2-N512? Could it be an increase in spurious diapycnal diffusion from advection errors? It is useful to speculate here, even if you do not perform a budget analysis as in Griffies et al.*

- Paragraph has been inserted p8, lines 26-32 to discuss the heat uptake issue and contrast with Griffies et al. (2015)

Pg9, It is useful to state how the mixed layer depth is computed.

- This has been added to a footnote on p9

Pg 10,line6: a more recent Denmark St overflow measurement paper is Jochumsen et al. (2012), 10.1029/2012JC008244

- P11, line 4 Jochumsen reference added

pg10,line18: a more recent Drake Passage transport paper is Meredith et al. (2011) 10.1029/2010RG000348

- P11, line 13 Meredith reference added

*Pg10, equation (1): dS is not defined. I presume it is dS = dx * dz. Please specify.*

- P12, lines 4-6 definition of dS and also dA added

pg10,line31: A_iso is not the "isopycnal diffusion". Instead, it is the "isopycnal diffusivity". Or more properly, it is the "isoneutral diffusivity", which is consistent with terminology used elsewhere in this manuscript.

- P12 line 3 changed to isoneutral diffusivity and on p14, lines 25-26

pg11, line1-3: I puzzled by this discussion. You state that the isoneutral fluxes are smaller than other terms, but then state, parenthetically, that the dianeutral diffusive fluxes are very small when integrated over the full depth. I think there is some confusion here.
In particular, the dianeutral diffusive fluxes, which are computed as vertical diffusion in NEMO, should have a zero depth integral since vertical diffusion only redistributes heat within a column. In contrast, the depth integrated isoneutral diffusion fluxes have a nonzero depth integral.
Are you arguing that the depth integrated isoneutral diffusive heat fluxes are small?

- P12 line 7 This comment is correct. The wording has changed to isoneutral

pg11, lines23-24: I fail to see how removing a global mean from the right hand side of equation (1) will not be seen by the left hand side of equation (1). Is that what you want? Please detail more of what you mean by "subtracting the global mean imbalance from the surface fluxes before integrating zonally and meridionally." It is vague as stated.

- Removing the global mean surface flux from the rhs of (1) is equivalent to removing the global integral of the temperature drift from the lhs of (1). This means that the imbalance represents the residual local drifts. Text has been added to make this clear (p12, line 31 – p13, line 4) *pg12, line13-ff: Again, I wonder how much of what you are seeing relates to the Weddell Sea polynya*

- A caveat has been added on p13, line 31-p14, line 1

pg13,line32: GFDL prediction folks claim that eddying oceans are too expensive for initialization schemes. So they are not pursuing ocean resolution. That contrasts with your motivation at the Met Office. The community would be well served to know more about your initialization strategies with an eddying ocean. It is worth at least a paragraph.

- A short paragraph on initialisation has been included on p15 lines 26- p16, line 2

Figure 1: legend font is tiny; needs to be larger.

- Figure 1: legend size increased

Figures 2,3,4: is the land/sea mask based on the model grid, or based on an observed topography dataset? It looks like observed. I suggest it more useful and honest for a modelling paper to show the land/sea mask based on the model grid.
I also dislike white land since there is also a white part of the colour bar for ocean fields. I suggest colouring the land light brown or light gray, in order to clearly distinguish water from land.

- Figures 2,3,4: land has been changed to grey and mask from model grid used

Figure 3: the colour range should be smaller in order to better see the anomalies.

- Figure 3: range reduced to -2 to 2

Figure 5: the HadISST values should be coloured to better distinguish from the many model lines. The present light gray shading does not come through well.

- Figure 5: HadISST values coloured. Legend changed to GC2.1-N512O12

Figure 6: how deep does the MLD penetrate in the saturated regions? This issue goes to the question about how significant is the polynya.

- Figure 6: Maximum MLD in the average is 788m. However, as seen in the extra figure, the polynya only appears in the last 9 years of the simulation and then it is only in the final year that it extends down to below 2000m. Land has been changed to grey and definition of sea ice edge added to caption.

Figure 9: The bottom topography appears nearly the same across the ocean resolutions. Are you sure you are showing the proper bottom?
Does the model make use of the partial bottom cells? If so, then the bottom shown here does not appear to reflect the partial cells; this instead figure looks like it is showing full cells. Again, it is preferable to show the what the model is actually using.

- Figure 9 (now figure 10): The model does use partial cells and the average topography is changing between ORCA025 and ORCA12