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Geoscientific Model Development Discussions

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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Review from Stephen Griffies, NOAA/GFDL

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.

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.

General points/questions:

–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.

–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.

Minor points.

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.

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".

pg3, line5: Behrens et al. (2013) should be Behrens (2013). This citation refers to a single-authored PhD thesis.

pg4,line9: again, "3-hourly to hourly" refers to "coupling period" not "coupling frequency".

pg4, line 11: 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.

pg4, line 27: 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.

pg5, line 5: 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.

pg5,line6: what is "atmospheric theta"? Please define the jargon.

pg5, line 28: 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?

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.

pg9, It is useful to state how the mixed layer depth is computed.

pg10, line6: a more recent Denmark St overflow measurement paper is Jochumsen et al. (2012), 10.1029/2012JC008244

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

pg10, equation (1): dS is not defined. I presume it is dS = dx * dz. Please specify.

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.

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?

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.

pg12, line13-ff: Again, I wonder how much of what you are seeing relates to the Weddell Sea polynya.

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.

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

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.

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

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

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.

END OF REVIEW

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