Interactive comment on "A near-global eddy-resolving OGCM for climate studies" *by* X. Zhang et al.

S. M. Griffies (Referee)

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This manuscript provides a summary of features found in a mesoscale eddy rich ocean simulation forced by JRA55 atmospheric reanalysis. The manuscript is well written and offers useful diagnostics for others to compare/contrast. It is suitable for GMD, and its publication should ultimately occur. However, there are some overall minor changes needed to bring the manuscript into a more suitable format. So long as the authors address all reviewer comments, and I trust they can, then I recommend this work be published in GMD.

General comments:

**Remove "eddy resolving" everywhere

I strongly rebel against the term "eddy resolving". That term is not justified here, nor even defined. The ocean mesoscale, as defined by the 1st baroclinic Rossby radius, has a non-homogeneous eddy length scale spanning from 1km on the shelves of the high latitudes, to 100km in the low latitudes (see Figure 1 in Hallberg, Ocean Modelling 72 (2013) 92–103). So even from the 1st Rossby radius perspective, this model is not "eddy resolving" everywhere. What about higher baroclinic modes?? And what about submesoscale eddies, and then nonlinear gravity wave "eddies", each of whose features reach down into the sub-kilometre range?

Furthermore, there is no study showing numerical convergence in the mesoscale resulting from a model that has a resolution equivalent to the first baroclinic Rossby wave. What in fact do we need to resolve in order to claim we are "resolved"? Is it just a linear baroclinic wave itself? Or the flux convergences? What fluxes? PV, heat, salt, momentum, etc? The question of what defines "eddy resolving" is not closed, so please avoid this sort of terminology. You have a model that richly represents nonlinear mesoscale features, and you are exploring elements of the simulation. But you cannot claim to be "eddy resolving" by any stretch. Period.

So please drop the pretentious and ill-defined "eddy resolving" term ***everywhere*** in your manuscript. Instead, be more explicit and honest by using language such as "mesoscale eddy rich".

**Why no sea ice model?? It needs to be better motivated, even if it is the result of a "model of opportunity" exercise.

**change "and also" to "and" throughout the paper. page 2, line 12 page 7, line 17 page 12, line 22

Specific comments:

page 3, line 12: "ever" should be "even"

page 3, line 23: "Saramiento" should be "Sarmiento"

page 4, line 9: The MOM code uses partial bottom cells based on the work of Adcroft et al (1997) as well as the following paper, which should also be cited:

@Article{PacGnan1998, author = "Ronald C. Pacanowski and A. Gnanadesikan", title = "Transient response in a z-level ocean model that resolves topography with partial-cells", journal = "Monthly Weather Review", year = "1998", volume = "126", pages = "3248-3270" }

page 4, lines 18-20: Boussinesq models need not retain a constant volume. They are quite able to increase or decrease sea level through water addition or removal. For example, see the paper

@Article{Lorbacher_etal2012, author = "K. Lorbacher and S. J. Marsland and J. A. Church and S. M. Grif\/f\/ies and D. Stammer", title = "Rapid barotropic sea-level rise from ice-sheet melting scenarios", journal = JGR, year = "2012", volume = "117, C06003", doi = "doi:10.1029/2011JC007733", }

The reason that groups often keep the net water flux equal to zero over the globe is to reduce model drift. It has NOTHING to do with the Boussinesq approximation.

page 6, line 8: What is "The correct spin-up of OGCMs"? Do you presume to have example of such? I for one have been on a 25 year quest for this method... :-)

page 7, line 13: I prefer to think of the global OHC as the global volume mean heat content. However, as used here, the authors refer to the horizontally integrated heat content as a function of depth. I suggest a more suitable language is useful to avoid reader confusion.

page 7, line 14: "staring" should be "starting"

page 7, line 18: there seems to be a missing number on this line. It presently reads "change of Global OHC...". The blank must be a number, but that number is missing.

Figures: I encourage placing more statistical information on each of the figures or their

captions, so to better allow them to be self-contained. Many statistics are noted in the text, and they should also be placed along with the figure. Additionally, for the maps, it would serve the reader well to also provide a zonal mean of the biases to better identify the latitude where the biases are localized. All of this information is useful for others aiming to perform quantitative comparisons to your work. Merely showing the maps is insufficient.

page 9, line 19: "indicates" should be "indicate"

page 9, line 24: JRA-55 is available at a resolution of 55km as well as 1.25 degrees.

http://rda.ucar.edu/datasets/ds628.0/

You can examine this hypothesis concerning upwelling by moving to the finer resolution data.

page 10, section 4.1.4: Please show the maths for how you computed the Sverdrup/Island rule transport shown in Figure 10. Others may wish to repeat this calculation, and your method should be clearly documented. Reliance on literature is not needed, since you can summarize the method in a few lines of words and maths.

page 11, line 2: The meridional overturning streamfunction is not "zonally averaged", in which case the units would be m2/sec. Instead, it is "zonally integrated". Please correct.

page 14, line 13: More discussion is needed regarding this point. It contrasts with points that others have made. Namely, even though OISST is 1/4 degree, it is sampling the real ocean with real ocean variability. This situation is quite distinct from a 1/4 ocean model that only admits variability at the 1/4 and coarser range. For more on this point, please see the paper Levy et al, Ocean Modelling, 48, 2012, Pages 1–9.

page 15, line 9-10: I am unsure why the bulk formula should produce less wind stress than the wind stress directly provided by JRA55. What are the issues? Perhaps it is due to use of (U_atmos - U_ocean) to compute the stress??

page 15, line 30: "Domingues"

page 16, line 3-4: I have trouble with your statement that "no observational data are assimilated in our historical experiment." And later the statement "nearly free OGCM". Your results are an achievement. But there are huge limitations.

–What about the non-adaptive forcing in the ocean interior that reduces drift (page 16, line 20)?

–What about the adaptive surface nudging?

–What about the truncated northern and southern domain boundaries?

–What about the absence of sea ice?

All of these limitations are helpful to reduce model drift. The high latitudes in particular are very very tough in ocean-ice models. See the Griffies et al, 2009 CORE paper for mechanisms leading to drift, with these mechanisms largely eliminated by truncating your model domain and removing sea ice.

page 16, line 29: "...associated with the..."

page 18, line 11: Suggested edit: "Despite good model performance for certain diagnostics/metrics, we fully acknowledge many caveats and limitations."

page 18, line 17: "combing" should be "combining"

page 27, line 4: "annual"

page 28: this table needs observational estimates for the reader to have any idea of how relevant the model transports are!

page 29, Figure 1: I can barely read the numbers places on the map. They will undoubtedly come out even less visible in print.

page 31, Figure 3 caption: What does "Equivalent surface heat flux" referred to here? Please define.

page 32, Figure 4: I suggest rotating these panels clockwise by 45degrees, so that a full profile for temperature and salinity sit atop one another.

page 33, Figure 5: Again, what is "equivalent surface heat flux"???

page 34, Figure 6 caption: penultimate domain should be "north subtropical", not "south".

page 35, Figure 7: place zonal mean bias next to bottom panel, and make note of other statisticis in the figure caption.

page 36, Figure 8: difference map should have smaller colour range than raw fields, in order to better view the biases. Also add statistics information to the caption.

page 41-42, Figures 13,14: The BGC figures use black land mask, whereas other figures use grey. Please be consistent.

page 50, Figure 22 caption: "Domingue"

END OF REVIEW