

Responses to Reviews of manuscript "A near-global eddy-resolving OGCM for climate studies" (gmd-2016-17) by X. Zhang, P. R. Oke, M. Feng, M. A. Chamberlain, J. A. Church, D. Monselesan, C. Sun, R. J. Matear, A. Schiller and R. Fiedler.

(The reviewer's comments are in back and our responses are in blue)

Referee Comment by referee #2 (S. M. Griffies)

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.

Thank you for your positive general comment above, and detailed comments below. We are going to address your comments point by point below.

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".

"Eddy-resolving" and "eddy-permitting" are used to "label" the ocean models by some authors. For example, Marsh et al (2009) directly referred to their 1/4° models as "eddy-permitting", and 1/12° models as "eddy-resolving".

We fully agree with your comment above, and in the revised text, we have avoided using the "eddy-resolving" term and use the term "eddy-rich" or "1/10°" instead.

Reference:

Marsh, R., and Coauthors: Recent changes in the North Atlantic circulation simulated with eddy-permitting and eddy-resolving ocean models, Ocean Modelling, 28, 226–239, 2009.

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

We admit that we chose to adapt an available model and designed new strategies to run it. The OFAM3 (Oke et al. 2013), based on MOM, did not include sea ice because it was mainly designed for mid and low latitude studies, which excluded the Arctic and avoided the complication of sea ice. We started with this near-global OGCM and gained useful experiences in running it for climate applications in non-polar regions. We plan to set up a true global OGCM with a sea ice model in coming years.

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

Specific comments:

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

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

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 \$\$-level ocean model that resolves topography with partialcells", journal = "Monthly Weather Review", year = "1998", volume = "126", pages = "3248-3270" }
Reference is added

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. Griffn/fn/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.

You are right about Boussinesq models need not retain a constant volume. Our writing was a bit confusing. The sentence is now modified to
“The model adopts Boussinesq Approximation (Greatbatch 1994). The net global freshwater flux (i.e., sum of evaporation, precipitation and river run-offs) is balanced at each model time step, thus the global mean sea level is kept constant.”

We chose to balance the freshwater flux at each time step mainly because the quality of freshwater flux is insufficient to ensure a realistic temporal evolution of the global mean sea level. Moreover, reliable fresh water fluxes from melting of glaciers and ice sheets are not available and therefore not included in most OGCM experiments. Since we are mainly interested in dynamic sea level (i.e., deviation of regional sea level from the global mean), we chose to keep the global mean sea level constant by having no net freshwater flux. The zero net freshwater flux to the ocean was not set up for controlling model drift (in the deep ocean).

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... :-)

As both you and referee #1 pointed out, it's really a challenging question. It may require tailored treatments for different model experiments. Here, we intended to strengthen the importance of "correct spin-up". The methodology we used in this paper may be one of feasible solutions to a similar set-up. The sentence and terminology are modified now.

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.

It's changed to "ocean heat content – integrated both vertically over some depth ranges and spatially over all ocean areas".

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

Corrected

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.

It's not due to missing number. It's modified to "the global OHC change rate".

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.

Statistical information is added to figures now. Zonal mean of biases is also added to Figs. 8 and 9.

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

Corrected

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.

That's a good suggestion. It could be a nice future work.

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.

Details about the Sverdrup Balance and Island rule calculation are given in the Appendix B now.

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

Corrected

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.

Point acknowledged.

Text is modified to state that the model is still missing unresolved sub-mesoscale processes that could reduce the variability simulated.

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??

The model uses $(U_{atmos} - U_{ocean})$ for the calculation, which plays a role in wind stress difference. Additionally, although both JRA55 and our model use bulk formula based on the Monin-Obukhov similarity theory, the implementations are not identical.

page 15, line 30: "Domingues"

Corrected

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.

The sentence is revised to

“This is a good indication that the model represents change and variability of OHC realistically, although it’s not an ocean reanalysis product like SODA (Carton and Giese, 2008) and ECMWF ORAS4 (Balmaseda et al. 2013), both of which assimilate massive in-situ and satellite data. The surface flux adjustment and non-adaptive restoring in the deep layers, as well as nudging in the northern boundary, prove to help achieve the good comparisons.”

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

Corrected

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

Corrected

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

Corrected

page 27, line 4: "annual"

Corrected

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

Observational estimates are added.

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

The readability of Fig. 1 is improved now.

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

Revised to “ocean heat content change rate”

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.

Revised as suggested

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

Revised to “ocean heat content change rate”

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

Corrected

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

Revised as suggested

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.

Revised as suggested

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

Revised as suggested

page 50, Figure 22 caption: "Domingue"

Corrected