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

Anonymous Referee #3

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I find little merit in this manuscript and feel it is unsuitable for publication. The paper describes a collection of ad hoc fixes and patches to the forcing of a high-resolution global model to compensate for shortcomings in the formulation of the problem (non-global, lack of sea-ice). The methods described are typical of those used in the 1980's and 1990's when available surface forcing products were far inferior. The apparent goal of the authors is to share "Experiences gained from our numerical experiments . . ." (pg 2 line 15). I cannot agree that this experience is something any other group should be following in the 21st century.

There is no innovation in ocean model development described in this paper. There are some changes made relative to the model formulation described in the previous paper by Oke et al (2013), but none of these are new to the modeling community (e.g. the incorporation of the KPP mixing scheme). Further, the authors do not described the

sensitivity of the solutions to these changes in formulation.

The main argument for "innovation" in the manuscript is in the adjustment of the surface forcing to constrain global energy balance and addition of fictional forcing in the deep ocean to limit drift. The procedures are described as they were implemented without discussion of their merits relative to other alternatives, nor of why the particular choices were made. In the case of the surface flux adjustment, an obvious and simpler method would be to assess the global imbalance in the JRA-55 forcing using observed SST in the bulk formula prior to model integration and subtract the necessary correction a priori. What is the advantage of the iterative approach described in the paper? Why was the adjustment applied to long wave down flux? Why not reduce shortwave down? There is no rationale provided for the choices made. In the case of the deep restoring, the authors imply that this is common practice in high resolution modeling, but only reference their own work as an example. I cannot think of a single study since the pioneering work of Semtner and Chervin in the 1980s that has used deep restoring in a forward integration of a high-resolution model. The main "innovation" the authors claim is the introduction of "non-adaptive" restoring. This is not a new idea. The same basic ideas are described in for example Eden et al JPO, 34, 701-719, 2004 (and likely a number of other papers).

Beyond the generally poorly motivated paper, the manuscript is quite superficial in its assessment of the solutions, and largely speculative when it comes time to attribute bias. Phrases like "may explain" or "could indicate" appear throughout when a more quantitative evaluation at a process level is called for. The aspects of the manuscript dealing with the BGC simulation seem to have been added as a complete afterthought.

DETAILED COMMENTS

pg 4 / lines 14-16. What is the implied global net heat flux for the JRA-55 product when using observed SST? This is a direct indication of the expected drift.

pg 4 / line 19: Why should the hydrologic cycle be balanced instantaneously? Cannot water be stored on land seasonally?

pg 4 / line 19: Global volume conservation is unrelated to the Boussinesq approximation

pg 4 / line 24: if relaxation is "often used" provide some references of examples other than your own model

pg 4 / line 33 : it is not a common technique (as above - provide some references)

pg 5 / lines 8-22: This is a slew of no-conservative physics. There needs to be a fuller demonstration of its impact not just on global measures, ut on the local structure of the solution. What is the spatial distribution of the restoring term? What spurious heat transports are implied by it? How does it impact the mesoscale?

pg 5 / line 8 : what is the "correct" spin-up. The ocean state in 1979 was not in equilibrium with the forcing in 1979.

pg 6 / line 15-20: How is the year-end discontinuity handled?

pg 6 / lines 27-bottom: There is no justification provided for this ad hoc procedure. (see above)

pg 7 / line 12 "does not necessarily imply a net heat flux correction" Of course it does. That is what you have constructed it to do!

pg 7 / line 32-35: Why does it agree better later in the run? The SST has diverged further from the observed initial conditions. You should be comparing to OAflux for 1979, not its climatology.

pg 9 / line 12 : "model may be too efficient" This is not a meaningful diagnosis and is purely speculative. Could be equally well attributed to any other process.

pg 9 / line 18: "This systematic difference ..." The biases in the Gulf Stream and Kuroshio appear well to the north of separation points, an indication of poor boundary

current separation. This is consistent with the biases int he SSH as well.

pg 9 / line 29: "Their global means are almost the same ..." By construction - you formulated to model to keep global mean sea level constant!

pg 10 / line 28 " "we do not repeat the detailed comparison" Then you don't need the detailed Table.

pg 11 / line 10 : A more useful and critical comparison would be against the vertical structure of the RAPID overturning.

pg 11 / line 27 : They appear more dissimilar than similar to me. The model is completely missing most of the upwelling productivity and vastly overestimates the equatorial productivity.

pg 13 / line 21 : as above - they are more dissimilar than similar. The largest observed variability is in the upwelling areas, not the central Pacific.

pg 14 / line 14: "its possible that ..." There is no need for speculation here. Smooth the model solution to the length scales used in the OI procedure for the observations and compare the smoothed fields.

pg 14 / line 22 : How can the mean upwelling be simulated poorly, but the variability be well reproduced?

pg 15 / line 15: What are we supposed to be seeing in the single-month plots? The eddies will all be in different places for a forward model run.

pg 15 / line 31 - : The full depth OHC is completely determined by the surface heat flux, so it is not surprise these are similar. Why are there no comparisons with the vertical structure from the reanalysis products?

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