Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-351-RC2, 2020

Geoscientific Model Development Discussions

Interactive comment on "A global eddying hindcast ocean simulation with OFES2" *by* Hideharu Sasaki et al.

Anonymous Referee #2

Received and published: 3 March 2020

This paper presents a newly released OFES2 ocean model and some improved model results compared with the old OFES. A sea-ice model and a tidal mixing scheme are included with the new JRA55-do atmospheric forcing. Sea surface temperature and sea surface salinity are both improved in many key regions. Specifically, the new atmospheric forcing improves the eastern Indian Ocean sea surface temperature. However, the authors attribute to the change of atmospheric data in many improvements without further convincing evidences. Also, this manuscript should provide a more general overview of this product (e.g., Pacific/Atlantic/Indian Ocean) to the readers instead of many regional discussions. In general, this manuscript will be used as the future reference for OFES2 and is appropriate to be published in GMD after considering the following comments.

1. Introduction: p2, other global eddy-resolving models are briefly mentioned here. However, the authors should comment on the differences and the unique characteristics of the new OFES2 among them. It seems the main OFES2 is to update the old OFES.

2. Section 2 requires further detailed description for the new features because this paper will be representative for OFES2 in the future. Line 71, is the thickness within the upper 500m non-uniform? Can you describe more about the coupling of sea-ice model? Can you comment on the impact of without polar region on the sea ice model and the ocean model? Is the sea-ice model coupled to the ocean model internally or through a coupler? This is important because the model results may be very sensitive to the coupling frequency. There are many issues related to the sea-ice model. The authors have to address this clearly.

3. In addition, can you clarify if you replace the original KPP by the Noh and Kim (1999)? Then, add the new tidal mixing scheme. If so, why do you replace the KPP? Any specific reason? For the tidal mixing, why do you include only K1 and M2? Are they the dominant components everywhere or specifically for the Indonesia region?

4. Section 2: The most important change is the inclusion of the JRA55-do. The big difference is the use of relative wind speed. Can the author quantify how big the final wind stress is changed to force the ocean? Also, what's the global distribution of this improvement overall? This is important because the authors contribute many improved results to this updated dataset. Also, does the double-counting of the ocean current suggested in Sun et al. (2019) exist? Is the NECC still weak in the OFES2?

Sun, Z., Liu, H., Lin, P., Tseng, Y.âĂŘh., Small, J., & Bryan, F. (2019). The modeling of the north equatorial countercurrent in the community earth system model and its oceanic component. Journal of Advances in Modeling Earth Systems, 11, 531– 544.

5. line 98, what's the time scale used in the temperature and salinity restoring? Any sponge layer? Is this the reason causing the large SSS biases near the northern

boundary (Figures 4c and 4d)? If you impose the restoring, why do you still get such a large bias there? Should this affect the coupling with sea-ice model? These issues need to be addressed in a greater detail.

6. Line 104: do you have any results to support this? This manuscript only emphasizes on the Indonesian and Arabian Seas. Unfortunately, these are very regional validation. The watermasses in the Atlantic, Pacific and Indian Ocean should be discussed.

7. Section 3.5 It seems the Sea-ice distribution is generally reasonable. However, the interannual variation and its long-term trend are what we care most. Is the long-term trend consistent with the observation? What's the major advantage of bringing this sea-ice model if Arctic region is not simulated? Also, can the missing of North pole cause any artifacts in the northern boundary? Particularly, does this have any impact on the deep water formation? Should the reader need to be caution for any potential problem?

8. Lin128-132, it seems the eddy resolving model still cannot resolve the Kuroshio and Gulf Stream separation in OFES2. The authors need to comment on this further (e.g., Schoonover et al., 2016; 2017; McWilliams et al., 2019). Although a brief discussion here and next paragraph, I cannot see any useful information from the discussion. McWilliams, J. C., Gula, J., & Molemaker, M. J. (2019). The gulf stream north wall: Ageostrophic circulation and frontogenesis. Journal of Physical Oceanography, 49(4), 893– 916. Schoonover, J., and Coauthors, 2016: North Atlantic barotropic vorticity balances in numerical models. J. Phys. Oceanogr., 46, 289–303 Schoonover, J., Dewar, W. K., Wienders, N., & Deremble, B. (2017). Local sensitivities of the gulf stream separation. Journal of Physical Oceanography, 47, 353– 373.

9. Line 136-137, is this really due to the inclusion of weak river runoff (underestimation)? Or is it possible due to the vertical/horizontal mixing? These regions have not only the positive biases but also negative biases if you look carefully. Particularly, negative biases are mostly along the coast. This seems not "underestimation".

10. Line 138, are you sure more realistic product help? OFES (no inclusion of river runoff) shows negative biases in these regions. Maybe the estuary circulation mixing or tidal mixing is more important?

11. Line 140, "Observation errors . . ." Can you clarify this further? Why this comes from the observation errors? The northern boundary is restored to the observation, right? This is confusing.

12. Line 150-152, is this correct? If so, why not in salinity? The atmospheric surface data change should contribute mainly to the momentum flux, which should impact both temperature and salinity, right?

13. P5, last sentence, what is this "something"? Also, the northern boundary is restored to the observation. Why the model results are not converging to the observation?

14. Line 182, how and why replacing to JRA55-do change the SSH directly? Is this error very common in other ocean models using the same forcing? I don't think so. The authors need to clarify this further.

15. Section 3.1.2 shows the impact of tidal mixing on water mass property. However, the author only show the results of Indonesian Seas and Eastern Indian Ocean. What about other key regions? Most important of all, can this deteriorate other regions?

16. P7, first 2 paragraph, I suggest to reduce these paragraphs or provide some more new information. The message for these two paragraphs is the salty biases are reduced in the subsurface in OFES2. That's it.

17. P8, what's the main purpose of using the restoring in these marginal seas? The niche is the restoring can help regionally if the process is local (like Persian Gulf and Red Sea). However, it is well-known that the Mediterranean overflow can affect the Atlantic Overturn Circulation while the restoring cannot capture its overflow process therefore, the restoring can cannot help the simulation. It is not clear why the authors

consider this approach here. It doesn't help fundamentally.

18. Section 3.4, line 248-251. What about other subsurface regions? Why this region is chosen? Is this the region where the large difference of JRA55-do and NCEP product? I suggest the authors to show the regions of the largest and smallest differences. Also, the tropic is a well-known region that the wind correction is largest (Large and Yeager, 2004). It may be better to use these specific regions to show the impact of differences. Otherwise, it is just hand waving to say every improvement comes from the atmospheric wind changes.

19. Line 258-263, why do you think this improvement in OFES2 come from the momentum flux change? Why not other flux? Can you provide a more convincing evidence? If this is the main cause, the momentum fluxes should change both temperature and salinity, right? It seems only temperature is greatly improved but salinity is not.

20. Section 3.5, it seems the sea-ice model is also missing for the polar region. Then, . Is adding the sea-ice model affect the large-scale general circulation? Any global impact or is it just a regional impact? A key question is that does adding the sea-ice model improve the deep water formation and overall model performance?

21. Section 4.1 why are these two indices are chosen? Why not checking the AMO or other important ocean indices? Before discussing the interannual variation, I suggest the authors to discuss the long-term trend first. This is very important for the first order evaluation.

22. Line 292, again, why? Here, the authors contribute the biases to the JRA55-do without further information. Any result to support this speculation for the good flux?

23. Line 321-329, here, the author attribute the improvement to the coastal upwelling resulting from the winds. Is this a general cause? I suggest the authors to replace (or add) the particular year by the IOD composite years (i.e., positive composite years and negative composite years). This may support the discussion here.

Minor comments: 1) The labels in Section 3 look strange. 3.1(3.1.1, 3.1.2, 3.1.2) 3.1, 3.4. They are totally messed up. 2) Line 179 "which reduces" changed to "which is reduced" 3) Line 208, remove "a" after "large. Also -2 should be the superscript.

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