

Geosci. Model Dev. Discuss., referee comment RC2
<https://doi.org/10.5194/gmd-2022-52-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on gmd-2022-52

Anonymous Referee #2

Referee comment on "Development and validation of a global 1/32° surface wave-tide-circulation coupled ocean model: FIO-COM32" by Bin Xiao et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-52-RC2>, 2022

This paper summarizes two high-resolution experiments at 1/32 degree with and without tides. The simulations are spun up for 3 years and diagnostics are performed over the last two years.

This is a major computational exercise and the results are worth documenting. However, as presented, most of the results are expected and in agreement with previous studies. Perhaps the most novel result is the outcome of adding tides which leads to a better agreement between satellite-measured and modeled power spectra. It is indeed nice to see that an increase in resolution leads to a better distribution and a higher magnitude of EKE, but for this paper to be publishable, it needs to go beyond a simple show and tell and provide new insights by performing more in depth analysis of the results and differences.

Specifically,

- What does 1/32 give you in terms of which physical processes are better resolved? It is probably marginal in terms of the submesoscale, so do you see a difference in mixed layer instabilities between 1/10 and 1/32? Is the increase of EKE because of a stronger mesoscale field (lower viscosity) or the addition of submesoscale features?
- You allow for only one year spin up from the data assimilative 1/10 run. Is the KE in steady state? Is it sufficient for a mechanical adjustment?
- What is the T and S bias after 3 years? You do not use any relaxation to surface salinity which is known to lead to a significant drift in salinity. Can this be quantified? This may not be the main focus of the paper, but it is of importance as it impacts the 3D T and S distribution and strength of the western boundary currents.
- This is more of a comment. You use relative wind which is known to have an eddy killing effect (Renault et al., 2019). This is reflected in a modeled EKE is lower than the smooth satellite observed geostrophic EKE.
- Line 62: What do you mean by "it is necessary to validate the effect of Bv "? What is the exact question being answered here? Is Bv still effective at 1/32? Do you have any

reason what it should not? Is the impact of adding of B_v at $1/10$ similar to that of $1/32$? Is the mixed layer physics responding differently at $1/10$ versus $1/32$ (in other words, what is the impact on the MLD of resolving smaller oceanic features?)?

- Snapshots are not representative of a solution. Improvements in western boundary current separation, extent and EKE need to be quantified by comparison to observations, not just stating that they qualitatively look better. Figure 1 and 2 are very small and it is really hard to see how the solutions differ, except for gross patterns.
- What is the rationale for presenting barotropic tidal results? There is some improvements with the increase in resolution, but they are relatively small and not significantly better in the $1/32$. Furthermore, since you are not using any drag in the $1/32$, how is the RMSE of the barotropic tides when compared to TPXO? How does it compare to the barotropic simulation?
- Can you provide a quantitative measure of “your belief that the global tide accuracy is reasonable” (line 230)?
- Line 268 – The MOIST data are significantly weaker than the model. How does it compare to other published tidal global models or in-situ observations? This needs to be better quantified, even if the difference cannot be fully explained with the current set of experiments.
- Line 296: Showing figures with more “textures” is not very informative. Can you quantify how the internal tides signature affect the specific locations and why? How were the three locations chosen? I presume it is because of different surface internal tide signature, but this would benefit from a thorough discussion of how the internal tides modify the spectra at each location. BTW, location of site C is not shown in Figure 9a.

Minor comments:

- Why do you have a maximum depth of 7000 m in the tidal simulation and only 5500 m in the non-tidal simulation?