Geosci. Model Dev. Discuss., referee comment RC3
https://doi.org/10.5194/gmd-2022-52-RC3, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on gmd-2022-52

Anonymous Referee #3

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-RC3, 2022

This manuscript describes the implementation and initial results of simulations using a very high-resolution (1/32°) global ocean model, including waves and tides. This is an exceptional effort and adds to a small handful of similar very high-resolution simulations of the ocean which have been undertaken to date. The paper describes the results of including "mixing from non-breaking waves", and from the (surface) tides, which in turn generate internal tides. There is clearly merit in publishing some of the results, particularly those describing the tides and internal tides and their implications for comparisons with satellite spectra (i.e. the internal tides induce significant surface variability, leading to better agreement with satellite observations in regions of otherwise low variability), but I think a major revision would be needed first. This would be to address concerns about the "mixing from non-breaking waves", and also to include some further analysis to look at the evolution of the deeper ocean.

The main difficulty with the paper is the inclusion of the Bv mixing term and referring to this as "mixing by non-breaking waves", as I do not think that Bv represents such mixing. While the theory is discussed in Qiao et al. (2004), it is simpler to refer to Qiao et al. (2010, Ocean Dynamics 60: 1339-1355), in which a single monochromatic wave is considered in section 2.5. Prandtl theory stipulates that a diffusivity or vertical mixing rate can be specified from the wave average of the vertical velocity perturbation (w′) and some mixing length based on the vertical displacement (l′) of a fluid particle via the formulation <w′l′>, which is defined to be Bv in equation 35 (using slightly different nomenclature). They then choose l′ (below equation 35, their l_{3w})) as the orbital vertical excursion due to the wave. It is clear that in this case, for instance, the vertical velocity will be a maximum when the particle is at its mean position (l′=0), and w′ will be zero when the particle is at its maximum vertical position, etc. That is, w′ and l′ are 90° out of phase, or in quadrature, so that <w′l′> = 0. This would actually result from taking the w′ as the vertical component of the wave orbital velocity as specified in their equation 34 (their u_{3w}), in which an "i" imposes a 90° shift as compared with l′. Instead, the choice (between equations 34 and 35) is made to take w′ to be directly proportional to, and in phase with, l′, so that the resulting average <w′l′> is NON-zero. This choice is difficult to understand and means that Bv will represent an arbitrary mixing term which adds a potentially significant amount of mixing to the ocean near-surface, but which does not represent mixing by non-breaking waves.

Therefore, the paper should remove all reference to Bv as a mixing term due to "non-breaking waves". Ideally EXP2 should be re-run without the inclusion of Bv. If this is not possible, EXP2 could be used by simply saying that this includes "Bv as a mixing term", but without referring to this as being "mixing by non-breaking waves". In particular, the phrase "mixing by non-breaking waves" should not be included anywhere in this paper. In this context, I suggest that Equation 1 should be removed, and also those parts of Figure 7 which show the differences due to Bv (panels(c), (e) and the green line in panel (f)), as this appears to be an unphysical mixing term. Continuing to refer to Bv as "mixing by non-breaking waves" in papers such as this one will only serve to increase the confusion and misunderstanding over this term in the ocean modelling community.

On the other hand, I would like to see a more complete analysis of the behaviour of the model. In particular, it would be important to see how good the model is for purposes of climate modelling, for which the maintenance of a reasonably stable inventory of water masses is needed, or that the model should not drift too quickly from the initial conditions. I suggest the paper therefore include plots to show how the deeper water masses are drifting, and include figures showing the globally-averaged Temperature and Salinity (T and S) anomalies (differences from initial conditions) versus depth and time, also zonally-averaged (or sections at say 40°W and in middle of Pacific) T and S versus depth and latitude at the end of the runs.

It would also be good to include a discussion about how the model scales on various numbers of processors e.g. a figure showing the run time for 1 year of simulation on various numbers of cores, if this is possible.

Further Comments

The English is readable but would benefit from being checked by a native English speaker (e.g. in the Abstract alone in l.s 19, 23, etc).

l. 12 etc. FIO-COM is not a "fully coupled" or even a "coupled" surface wave-tide-circulation model (as claimed several times e.g. l. 12, l. 321) as the waves are being run offline and fed into the tide-circulation model. There is no coupling back from the ocean circulation (or tides) onto the wave field. These claims need to be moderated.

ls. 140-142: what are the barotropic and baroclinic experiments referred to here?

ls. 162-164: was the viscosity higher in the 1/10° model than in the 1/32° model or the same (this is relevant for the EKE discussion, as it is usual to reduce the viscosity at higher resolution as more of the eddies are resolved).

Fig. 1d. The caption should state that this is the globally-averaged EKE.

ls. 188-190. Why is the 1/32° model EKE higher than in the satellite observations in Fig. 1d? Is this because the model can resolve the internal tides but the satellites do not have sufficient resolution to do so? What is the along-track resolution of the satellites, for instance?

Fig. 2. Titles on the subplots (a) and (b) says CMMES – this should be CMEMS.

ls. 213-222. This is mostly a description of the barotropic model set-up and it would be better to discuss this in section 2 (Model description) rather than here in the Results section.

ls. 233-240 and Fig. 6. It is not clear if the model results in panels (c) and (d) show the ¼° model as implied by the text, or the 1/32° model as implied by the figure caption.

Fig. 8. Caption to say that these are internal tide amplitudes *at the surface*.

Fig. 8. The internal tides in EXP2 are more energetic than in the MOIST observations. This implies that the dissipation of the internal tides is not being properly handled. Please comment on this. Has any explicit dissipation been applied to the internal tides to reduce their propagation?

Fig. 9. Add lines to panels (d) to (f) to show the 70-250 km wave number band referred to in the text (e.g. l. 279).

Fig. 9. Colour bar for panels (a) to (c) should show negative values i.e. from 0 to -5.4 (rather than from 0 to +5.4).

l.321 and elsewhere. The FIO-COM model is NOT fully coupled as the waves are run offline and fed into the tide-circulation model.