“Improved ocean circulation modeling with combined effects of surface waves and M2 internal tides on vertical mixing: a case study for the Indian Ocean” By Zhuang et al.

In this study, the authors incorporated three mixing schemes into the ocean general circulation model, namely non-breaking surface-wave-generated turbulent mixing(NBSW), the mixing induced by the wave transport flux residue(WTFR), and the internal tide-generated turbulent mixing(IT) along with Mellor-Yamada 2.5 mixing scheme. This study of quantifying the role of wave and tide-induced mixing in an ocean model is a timely and valuable contribution. However, the authors are unable to represent it in terms of value addition to its scientific contributions. There are many gaps in this study starting with ocean model configurations and their different experiments. The introduction lacks the present status of the state of the art model’s mixing schemes with details and its drawbacks in the Indian Ocean. The authors are unable to give the scientific objectives to be achieved in this study as compared to the previous works. The representation of the internal tide-generated turbulent mixing is not new, in fact, it’s been introduced by Simmons et al. (2004) in a global Ocean General Circulation model. The author did not mention this work and its related works (Nagai and Hibiya (2015). Also, the authors presented the results only up to 130 m which does not represent insight into the mixing process related to internal tides since its effect could be seen in the deeper layers. A very recent study by Lozovatsky et al. (2022) showed that the observed eddy diffusivity in the ocean pycnocline over the southeastern BoB is likely related to internal-wave generated turbulence. In line-121-22 the authors wrote “….., the mode-1 M2 internal tides, which mainly originate from regions with steep topographic gradients, are considered….” . Doesn’t it imply that the mixing will be more over the steep topographic gradients?. But the author did not show any results related to this. The authors implemented the mixing schemes in the momentum equations. This implementation will also affect the dynamics as well. But the authors did not show any results on whether any changes are there in the circulations. The authors should show a few results about how the upper ocean currents improved with implementations of NBSW, IT, and WTFR mixing schemes. It will be good if the authors also can show spatial comparisons of model-simulated temperature diffusivities with Argo observations (Whalen et al. 2012). I am unable to recommend this manuscript for publication in this form. However, it can be considered for publication if they address my above queries and the below comments.

1. Line 173-174: “The initial temperature and salinity are interpolated based on the annually mean Levitus data with the horizontal resolution of 1° by 1° and 33 vertical layers..” Which Levitus data authors have used? Should give the version and reference.
2. The author used a regional model in which the lateral boundary condition is very important for any basin-scale model, particularly for the Indian Ocean which is affected by the Indonesian Throughflow in the eastern boundary. The author did not give any

details about how the boundary condition is prescribed. Is it a boundary condition with a sponge layer? The authors should provide the details about the lateral boundary conditions used in this study.

3. Line 175-180: The initialization strategy and the experimental details are also not very clear. It looks like the author used a cold start and then inter-annual forcing from NCEP/NCAR (1948-2021). This means its inter-annual simulations. On the other hand, they wrote "The model is integrated from the quiescent state for 10 climatological years. The simulated temperature in the last 1 year is compared with the monthly World Ocean Atlas 2013 (WOA13) climatologic data" . This implies it's only 10 years of simulations. It's confusing what experiments the authors exactly carried out. It seems 10 years of simulation may not be sufficient to reach the steady-state. The authors should give the evidence that the model reached steady-state in 10^{th} year of simulation.
4. The author used MASNUM wave spectrum model simulations to get the inputs for the NBSW parameterizations scheme they incorporated. But how good the model simulations compare with observations?
5. In Figures 2c and 3c authors represented it as the IT-generated turbulent mixing scheme based on Exp-3 but in this experiment, NBSW is also included, then how can it be an IT-generated turbulent mixing scheme?
6. In Figures 2 and 3 for the vertical profiles of the monthly mean vertical temperature diffusive terms, the author choose to show the results for 10.5 °S, and for the temperature comparison, they showed 30.5 °S. What is the physical basis to choose these sections? Authors should show such results for the Arabian Sea and Bay of Bengal as well.
7. In Figure 4 in exp1& 4 why the model does show the cooler temperature in the thermocline depth region? In general, over the Indian Ocean, almost all forced model shows warm bias (Rahaman et al. 2020). Although the thermocline bias was reduced in Exp 2 and 3, it became reversed with similar magnitude why does it so? Why there is no difference between exp-1 and exp-4 in Figures 4 and 5? Does it mean WTFR does not impact temperature simulations? Authors should show such results for the Arabian Sea and Bay of Bengal as well.
8. Figure 8 What is the physical basis of choosing the different zone? Looks like the present defined zones will not give true representation, for example in zone 1 since the dynamics and thermodynamics are different in the Arabian Sea, Bay of Bengal and South China Sea, hence the mixing characteristics are also different. I suggest excluding the regions outside of the Indian Ocean such as South China Sea and Atlantic Ocean as included in the present zone 2 and zone 3. I also suggest the author should select the zones based on past studies or based on the dynamics and thermodynamic properties of the Indian Ocean basin.
9. How the RMSE is statistically robust when the authors used the seasonal cycle and computed the RMSE?

10. As already pointed out in the case of the thermocline in the MLD bias given in Figure 9 for Exp-1 too looks not consistent with the previous works. In general OGCMs simulates deeper MLD in the Indian Ocean (de Boyer Montégut et al. 2007). A very recent study by Pottapinjara et al. (2022) too shows similar results. Hence, how the MLD simulation, in this case, shows shallower than observations? The authors need to explain why the model simulated MLD is shallower as compared to observations. Also, the criteria used to compute MLD is not very widely used. The authors did not provide any reference to compute MLD or any explanation why they choose the 1 °C criterion to compute MLD.