A point-by-point listing of response for each of the reviewers' comments

We thank the reviewers for careful reviews and constructive comments in improving the manuscript. Below is our point-to-point reply to these comments (the reviewers' comments in the last review are in black, the reviewers' new comments are in blue, our responses are in red, and our changes in manuscript are in red and italics).

All of the changes in the revised manuscript are marked using 'track changes' in Word. Please refer to the marked-up manuscript version showing the changes made in detail.

Response to Comments from Referee #1

As the former Reviewer #1, I believe that the authors have responded to my suggestions and comments, and those of the other reviewers, satisfactorily. However, there are some further minor points:

* Former minor point 1: The explanation should be placed at the end of Line 70, "In this study, we analyze the effects of the turbulent mixing generated by the M2 internal tides on the ocean circulation."

We agree with the reviewer's suggestion. The explanation has been placed at the end of the sentence "In this study, we analyze the effects of the turbulent mixing generated by the M2 internal tides on the ocean circulation." in the original manuscript.

Please refer to Line 74-81 and 110-117 in the revised manuscript.

* Former minor point 3: Please provide references for your first and second arguments.

Many thanks for the suggestion. The related references have been added.

The references about the reasons for the choice of the 10.5°S Section have been added in Line 314 and 316-317 of the revised manuscript.

* Fig. 1: Please add the unit and also check the rest of the figures for similar mistakes.

Many thanks for the reviewer's suggestion. We agree with the reviewer and accept the suggestion. The figures have been replotted with adding the units.

Figures 2-8, 10-13 in the revised manuscript have been replotted.

Thank you to the authors for responding to all of the reviewers so thoroughly, and congratulations on your manuscript. I am happy to recommend publication in the Geoscientific Model Development after the small corrections noted above.

Many thanks for the reviewer's encouragement and comments. We tried our best to revise the manuscript following the reviewers' suggestion and comments.

Response to Comments from Referee #2

"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.

Author partially responded to my above comment. In terms of scientific contribution author represented "Furthermore, addition to the scientific value, the results in this study are helpful to improve the accuracy and timeliness of the global ocean numerical prediction for the national or regional forecasting agencies, because the MASNUM ocean model is able to depict more complete physical processes. In our opinion, it is important to study the NBSW- and IT-induced mixing for promoting the development of the ocean and coupling models." How does the accuracy and timeliness of the global ocean numerical prediction for the national or regional forecasting agencies are helpful?? The MASNUM ocean model is able to depict more complete physical processes, HOW???

The statements about the scientific contribution were not very correct. It is inappropriate to present “the results in this study are helpful to improve the accuracy and timeliness of the global ocean numerical prediction for the national or regional forecasting agencies”. Actually, we would like to say that the mixing schemes introduced in this study contain the effects of the surface waves and internal tides, which are thought to be the supplement of the physical mechanism for the vertical mixing processes in the OGCMs, because the original turbulent mixing schemes, such as the M-Y 2.5 scheme, neglected the interaction between the surface waves and the currents (Huang et al., 2011; Qiao and Huang, 2012). The M-Y 2.5 mixing scheme combined with the NBSW- and IT-induced mixing schemes should become more complete for modeling the vertical mixing processes.

The statements have been revised in Line 631-639 of the revised manuscript.

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).

Again same for my above comments. Author did not give any insight about what are outstanding issues in terms of mixing schemes in the ocean model with respect to Indian Ocean. For example overflow schemes in the Indian Ocean models to represent Red Sea salty water yet an outstanding issues. I agree that the internal tide-generated turbulent mixing is not explicitly implemented but yet it shows significant improvement, however, in the present study as such no significant improvement can be found.

We agree with the reviewer. The direct modeling of the internal tides is an effective and important way to study the internal tide-generated turbulent mixing, but there still some issues need to be solved to improve the simulation, including more accurate wind stress and simulated temperature and current structure, the establishment of a reasonable non-hydrostatic ocean model, and the parameterization of the interaction between different tidal constituents (Nagai and Hibiya, 2015). However, it should be noted that there should be a disadvantage for this direct modeling, which is that the simulated internal tide processes will become inaccurate if the temperature and current structure cannot be modeled accurately.

We have to admit that the issues mentioned above cannot be solved entirely when the NBSW- and IT-induced mixing schemes introduced in this study are adopted. The present study just provided another way and preliminary attempt to study the mixing processes induced by the internal tides. It should be more convenient to improve the simulation further because the mixing schemes are independent with the ocean models. The research team including the authors is developing an internal-wave/tide spectrum model. And a multi-scale process coupling model, including atmosphere, ocean current, tide, surface-wave, and internal-wave/tide component models, will be established in future for the accurate and high-resolution ocean modeling. The NBSW- and IT-induced mixing schemes and the related results in this study are helpful and valuable for establishing the coupling model.

The explanations have been revised in Line 127-134 and 664-669 of the revised manuscript.

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.

Authors gave explanation that below 130 m the model simulations shows too warm when compared with WOA13. How much warm? What is the exact value? 3, 4, 5 °C ???? The explanation given for this warm simulation as " The reason is that the Haney equation (Haney, 1971) was used to modify the climatologic surface heat flux and improve the large-scale thermal coupling of ocean and atmosphere, but a disadvantage of the Haney modifying method is the destruction of the heat balance, so excessive heat may be transmitted into the ocean interior." This explanation of temperature restoration is the only reason as given by the authors may not be correct. It seems MASNUM model has fundamental problem to reproduce it even in the control simulation. Author should give concrete scientific evidence what the exact cause of warm temperature simulation.

Many thanks for the reviewer's comments. The comparisons between the simulated temperature and the WOA13 data are analyzed detailedly again, and shown in Figures 1 – 3 with the depths from 0 to 300 m in January. Along the 30.5°S Transect, most of the deviations appear in the upper-100 m layers, the simulated temperature in Exp 1 is cooler than the WOA13 data and improved dramatically in Exp 2 and 3 when the NBSW- and IT-induced mixing schemes are adopted. The only special region is that on the southwest of the Australia (100° - 120° E), in which the simulated temperature is warmer than the WOA13 data when the depth is deeper than 150 m. This indicates that the simulated temperature is often cooler than the WOA13 data in the ACC region and the south of the IO. The mean deviations of the temperature in the upper-300 m regions are also shown in the figures.

However, along the 0.5° S (near the equator) and the 7.5° N Transects, the simulated temperature is warmer obviously than the WOA13 data when the depth is deeper than 120 m, and even the NBSW- and IT-induced mixing schemes cannot work anymore. Therefore, the distribution pattern of the temperature in the ocean interior (from 100 m to 300 m or deeper) seems to appear as cooler in the SIO and warmer in the NIO and the tropics.

The intermediate and deep water masses in the IO are often effected by the Southern Ocean including Antarctic Intermediate Water (AAIW), Circumpolar Deep Water (CDW), North Atlantic Deep Water (NADW), etc. These cooler water in the

Southern Ocean is carried by the meridional overturning circulation into the IO throughout south of the South Equatorial Current in the subtropical Indian Ocean, but the situation does not appear in the simulated current fields (Figures 4). Therefore, in addition to the relatively coarse and smoothed surface forcing mentioned in the last response, the another important reason should be that it is hard to simulate accurately the meridional overturning circulation in the present experiments, especially the meridional transport of the heat (cooler water in the Southern Ocean) from south (Southern Ocean) to north (South Indian Ocean). This makes the simulated temperature warmer than the WOA13 data when the depth is deeper than about 120 m along the 0.5° S (near the equator) and the 7.5° N Transects. More optimization and improvement of the real-time experimental design will be implemented in future work to solve the related issues.

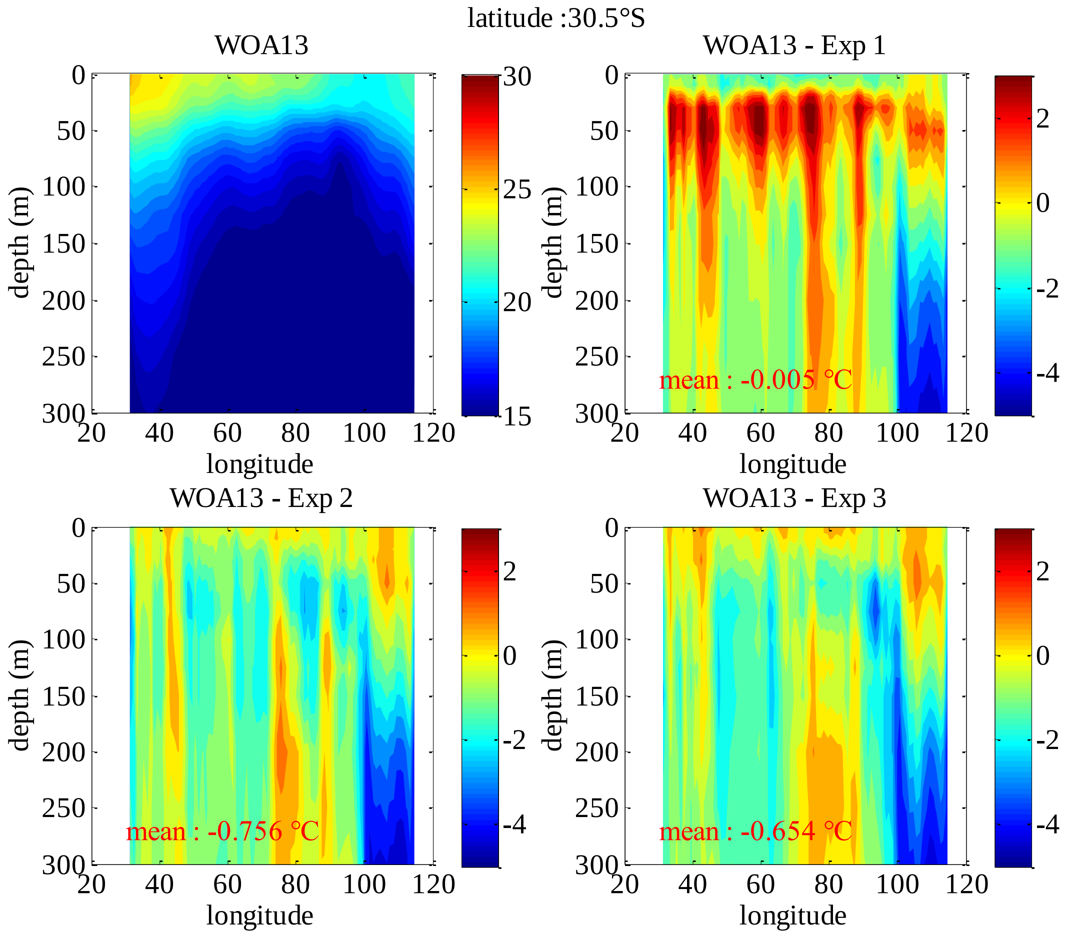


Figure 1. The vertical temperature profiles along 30.5° S in January. Left-top: The temperature structure from the monthly WOA13 data (units: ℃). Remaining 3 sub-

figures: the difference of the temperature calculated by subtracting the monthly mean results simulated in Exp 1 - Exp 3 from the monthly WOA13 data, respectively. The mean deviations of the temperature in the upper-300 m regions are also given as red text

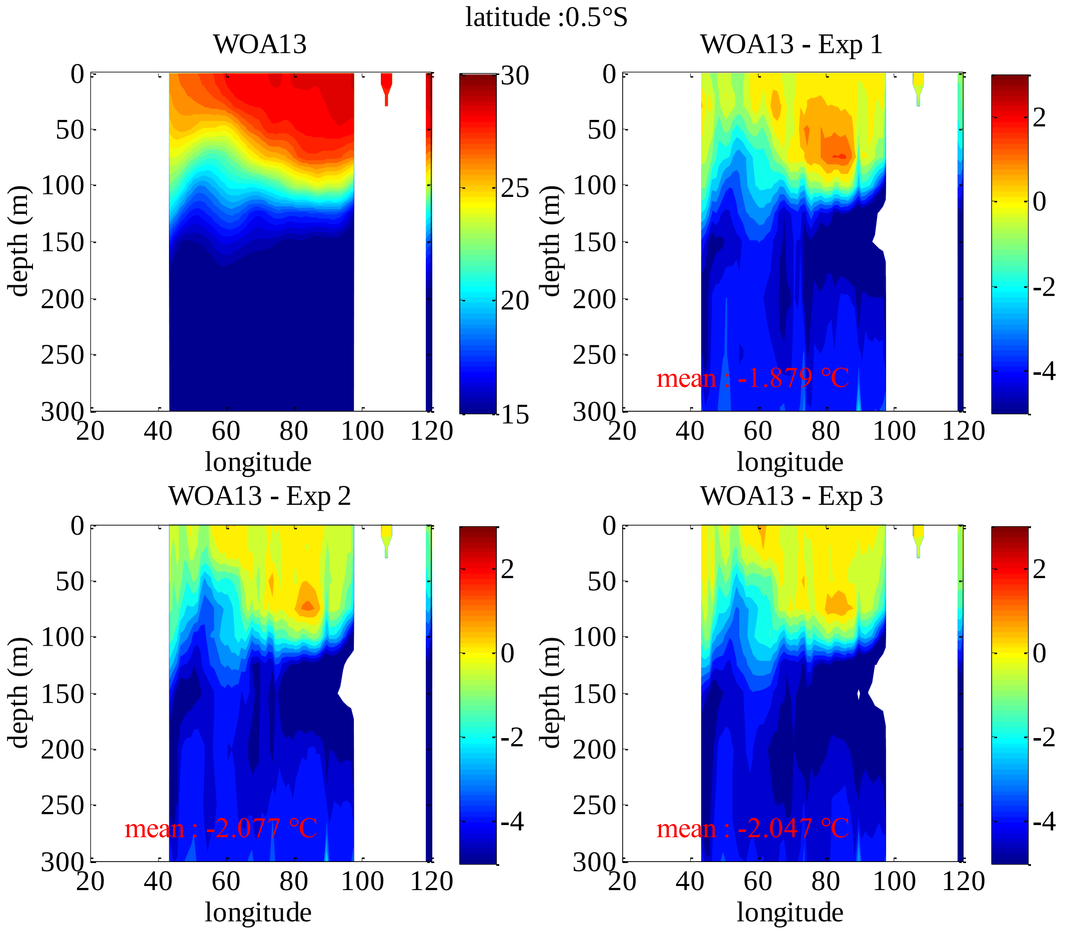


Figure 2. The same as Fig. 1, but along 0.5° S

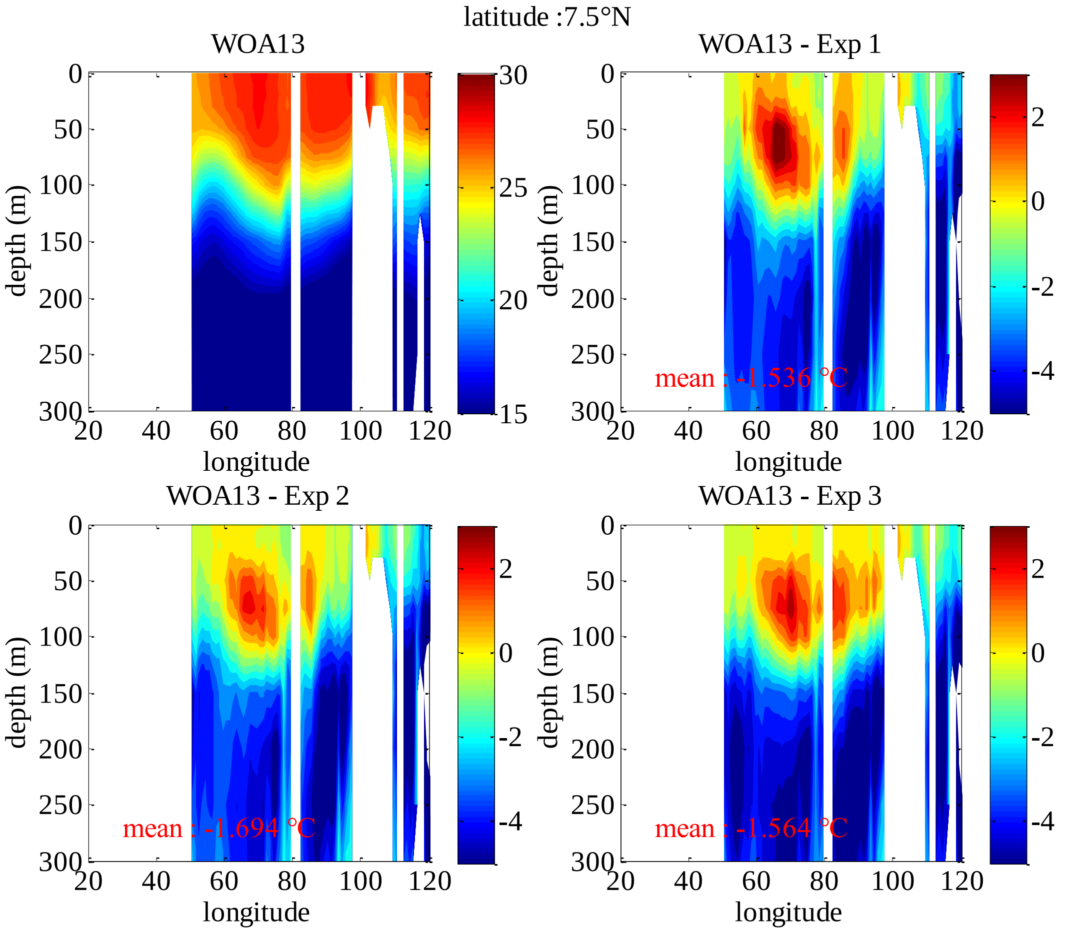


Figure 3. The same as Fig. 1, but along 7.5° N

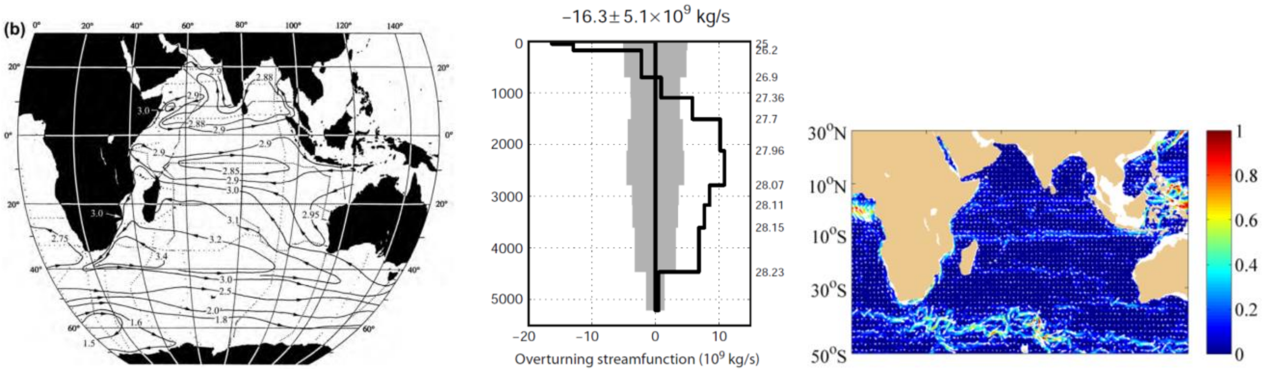
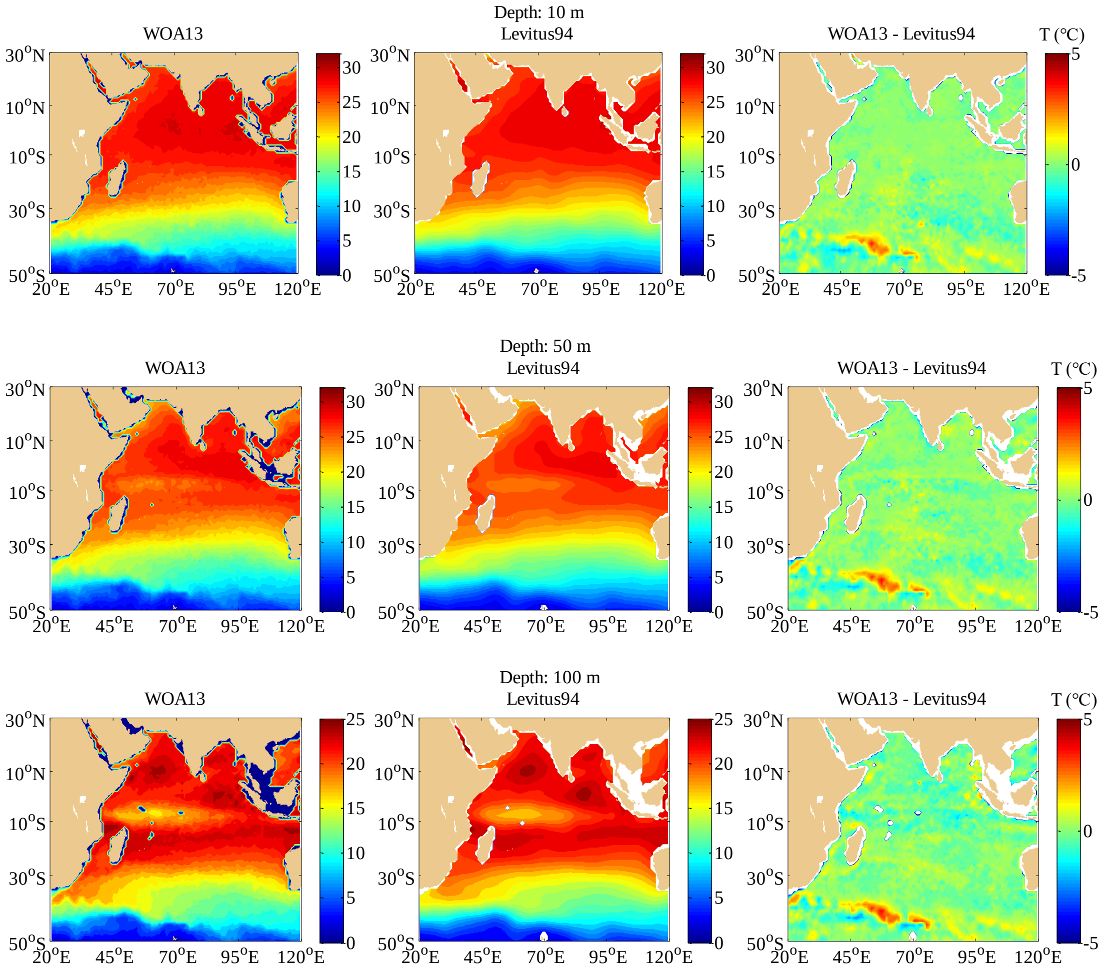


Figure 4. Left: adjusted steric height (related geostrophic streamfunction) (10 m^2/s^2) with the depth of 200 m (Reid, 2003). Middle: Net northward (meridional) transport (Sv) for the Indian Ocean at 33° S, integrated from the bottom to the top (Ganachaud et al., 2000). Right: Simulated horizontal currents in Exp 1.

Following the reviewer's comments, the initialization design is also important for the ocean modeling. The comparison of the annual mean temperature between the Levitus94 data and the WOA 13 data is shown in Figures 5 and 6. One can see that the

WOA13 data contains more meso-scale information than the Levitus94 data. The comparison shows that the temperature from the Levitus94 data is cooler obviously than that from the WOA13 data in the ACC regions (45° - 75° E, 35° - 50° S), while warmer generally in the whole IO with the depth from 200 to 500 m. Therefore, the inaccurate initial field should be also one of the reasons why the simulated temperature in the ocean interior is different from the WOA13 data. A series of the high-resolution real-time numerical experiments for the circulation in the IO will be carried out to examine the influence of different initial fields, parameterization schemes, surface fluxes, and open boundary conditions in future.



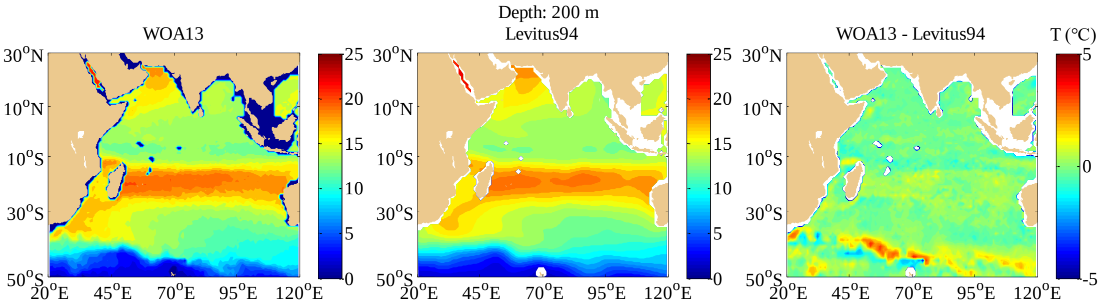


Figure 5. The comparison of the horizontal distribution of the temperature from the WOA13 and Levitus94 data.

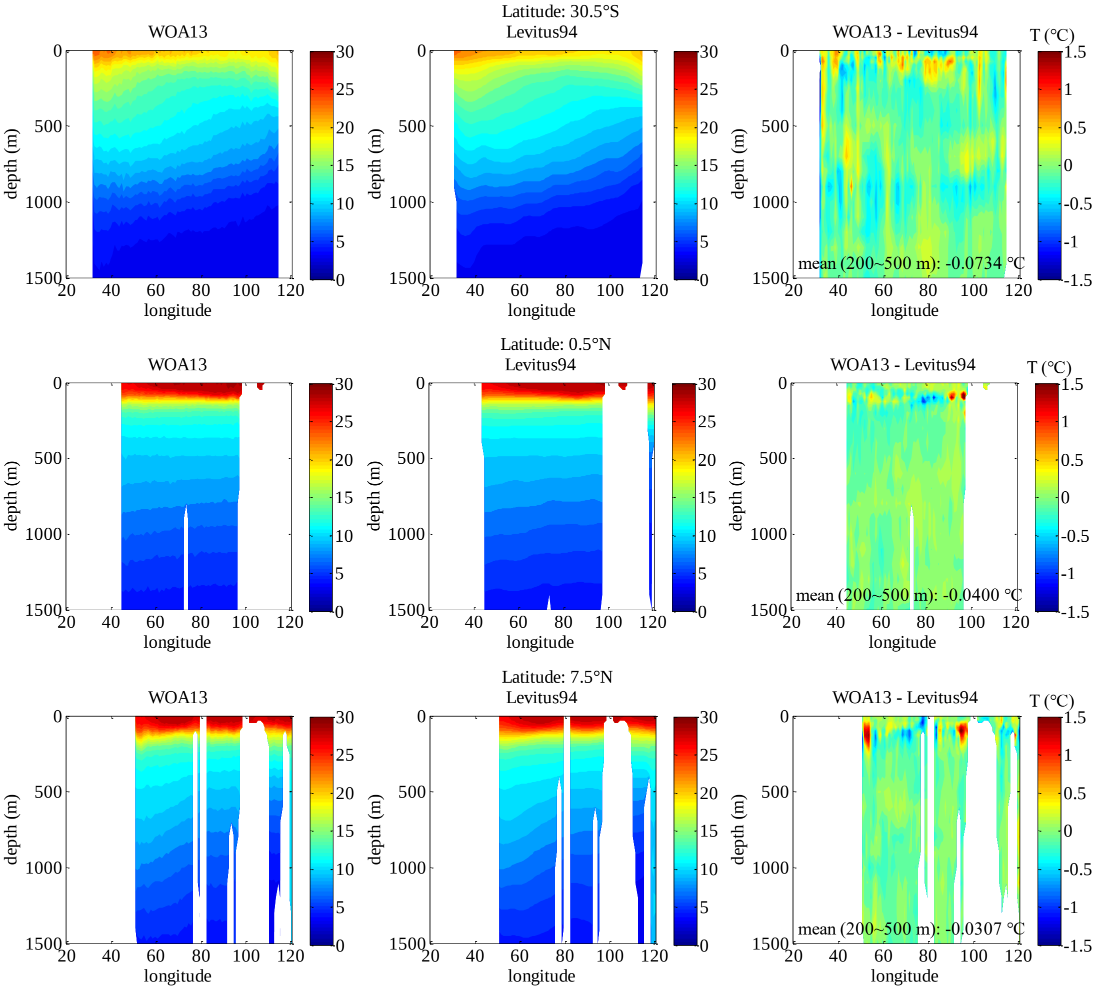


Figure 6. The comparison of the vertical distribution of the temperature from the WOA13 and Levitus data.

The explanations and analysis have been revised in Line 389-393 and 445-475 of the revised manuscript. Figures 1-3 have not been plotted in the revised manuscript because the detailed analysis for the deep ocean was not performed further here and the vertical mixing in the upper ocean (0 ~ 100 m) is the main focus of this study.

Figures 5 and 6 have also not been plotted in the revised manuscript because of the relatively minor role in the research on the vertical mixing and the limitation of the length of the article.

Reference:

Reid, J.L., 2003. On the total geostrophic circulation of the Indian Ocean: Flow patterns, tracers and transports. Progr. Oceanogr. 56, 137-186.

Ganachaud, A., Wunsch, C., Marotzke, J., Toole, J., 2000. Meridional overturning and large-scale circulation of the Indian Ocean. J. Geophys. Res. 105, 26117e26134.

The other existing Indian ocean regional model simulation based on MOM, ROMS, HYCOM etc does not show too warm temperature below 130 m as the authors mentioned.

In our opinion, the MASNUM ocean circulation model is suitable for the ocean modeling in the IO. Han (2014) and Han and Yuan (2014) have tested the modeling ability of the MASNUM model compared with the POM, the results showed that the MASNUM model could produce quite identical simulation results as the existing models with only half computer cost. The effects of the NBSW and IT on the vertical mixing processes are the main part of this study, but regrettably, the simulations especially in the control experiment seems to be unable to obtain satisfactory results because of the relatively coarse model design. We believe that high-resolution real-time numerical experiments based on the MASNUM ocean model developed in future will obtain more accurate simulation of the temperature and currents in the IO.

The explanations have been added in Line 228-230 of the revised manuscript.

I understand its climatological simulation but the eddy diffusivity in the ocean pycnocline as a model diagnostics can be obtained from all these sensitivity experiments. This will give some preliminary idea how much its differs with respect to instantaneous values given in Lozovatsky et al. (2022).

Following the reviewer's suggestion, the eddy diffusivity ($K_N = {}^{\gamma\varepsilon}/_{N^2}$) can be characterized by the vertical mixing coefficients (km, Bus and Bui are viscosity corresponding to the momentum equations, kh, Bts = 2Bus and Bti = 2Bui are diffusivity to the tracer equations). The km and kh are calculated by M-Y 2.5 scheme, Bus and Bts are calculated by NBSW-induced mixing scheme, and Bui and Bti are calculated by IT-induced mixing scheme. The comparisons of the eddy diffusivity are shown in Figures 7 and 8. The eddy diffusivity of the WTFR is omitted here because the expression of the diffusive term is not in a standard form, which means that we can not obtain the eddy diffusivity directly from the expression of the diffusive term.

From Figures 7 and 8, one can see that the vertical distribution are very similar to that of the diffusive terms (Figures 2 and 3 in the manuscript). Especially in January, Bts is the largest in the upper-30 m layers and Bti is larger generally in the ocean interior with the depth deeper than about 40 m. Kh and Bts decay with the depth below the sea surface, the delay rate of Bts is slower obviously than Kh, so Bts is larger than Kh in the ocean interior. The high-value layers (>10^{-5} m^2/s) of the Kh are as thin as about 20 m in January, and up to about 80 m partially in July, while the high-value layers of the Bts are generally about 70-100 m both in January and July. When the depth is larger than 40 m, the value of Bti appear to be about 10^{-5}-10^{-3} m^2/s.

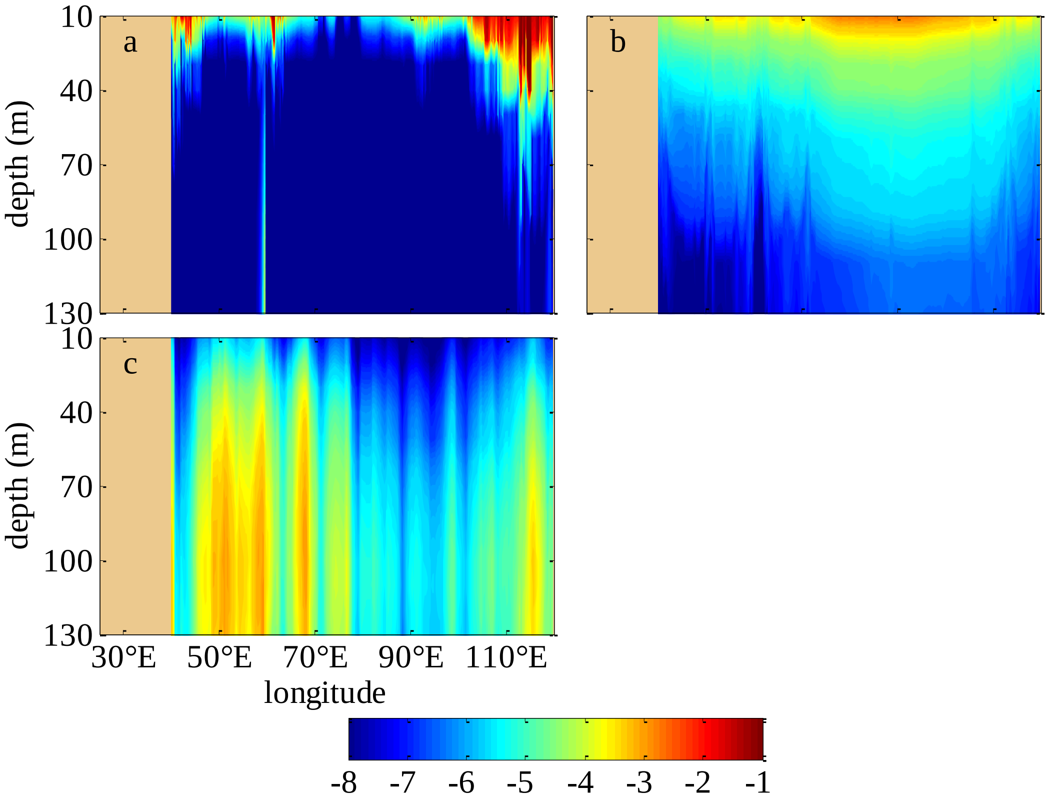


Figure 7. Vertical profiles of the diffusivity in logarithmic scale along 10.5° S in January, including Kh (a), Bts (b), and Bti (c)

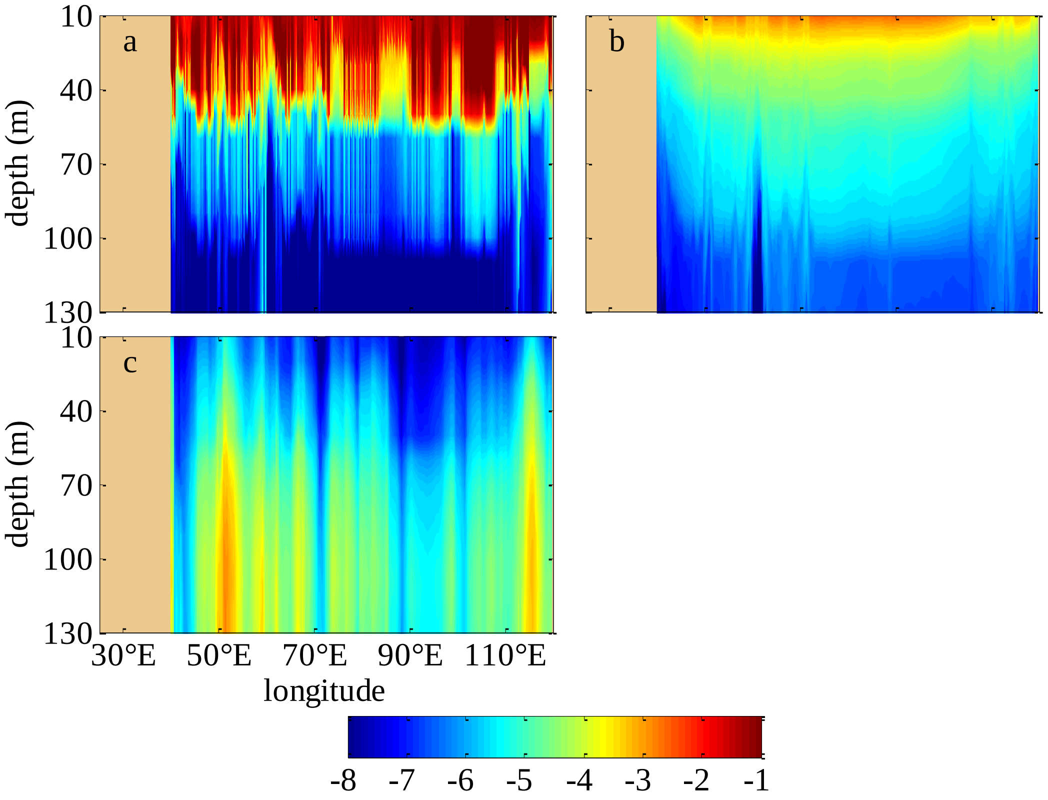


Figure 8. The same as Fig. 7, but in July

The explanations and the description of the values of the eddy diffusivity have been added in Line 325-331 of the revised manuscript. Figures 7 and 8 have not been plotted in the revised manuscript because they are very similar to Figures 2 and 3 in the manuscript.

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.

Reply: Explanation with respect to my above comment looks fine.

Many thanks for the reviewer's suggestion. We tried our best to revise the manuscript.

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.

With reference to my above comment, although the author gave comparison plot with OSCAR but it's very hard to see any changes between Exp-1, Exp-2 and Exp-3. It looks no significant change of circulation is induced with the inclusion of the stated schemes explicitly as a subroutine in the momentum equation.

On one hand, only the simulated surface current fields were compared with the OSCAR data, the RMSEs were reduced slightly because the surface currents are controlled dominantly by the surface wind stresses, the interaction between the ocean and atmosphere, etc., rather than the vertical mixing in the upper layers. Even so, the NBSW- and IT-induced mixing schemes still can partially improve the simulation of the surface currents (RMSE decreased in Exp 3 compared with Exp 1). On the other hand, regrettably, the simulated currents in the ocean interior were not improved when the NBSW- and IT-induced mixing schemes were adopted. This implies that the currents are complicated and vertical mixing is just one of the influencing factors. Unlike the temperature, the NBSW- and IT-induced mixing schemes are unable to improve the simulated currents obviously at present (may be applicable to the thermohaline circulation, which is the oceanic deep circulation system with global scale in a relatively steady state), more accurate mixing schemes or other ways for parameterizing the physical mechanism will be developed in future.

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.

The argument given for not to give the comparison results not acceptable. I agree this will not give exact values but at least will give the spatial distribution pattern. Author must show the spatial comparison.

Many thanks for the reviewer’s suggestion, the existing Argo-derived gridded products, which are named Barnes objective analysis (BOA)-Argo datasets (Li et al., 2017), are chosen to validate the simulated temperature structure. The climatologic monthly mean BOA-Argo data (multi-year mean from 2004 to 2014) are used and can be downloaded directly from ftp://data.argo.org.cn/pub/ARGO/BOA_Argo/. The BOA-Argo data with 49 vertical levels from the surface to 1950 m depth is produced based on refined Barnes successive corrections by adopting flexible response functions. A series of error analyses are adopted to minimize errors induced by non-uniform spatial distribution of Argo observations. These response functions allow BOA-Argo to capture a greater portion of mesoscale and large-scale signals while compressing small-sale and high-frequency noise the performance of the BOA-Argo dataset demonstrates both an accuracy and retainment of mesoscale features. Generally, BOA-Argo seems compare well with other global gridded data sets (Li et al., 2017).

Figures 9 – 12 shows the comparison of the temperature structure between the monthly BOA-Argo data and the model results. The patterns are similar to those from the WOA13 data (please see Figures 4a, 5a, 6a and 7a in the manuscript, and Figures

9a, 10a, 11a and 12a). The difference between the BOA-Argo data and the model results along 30.5° S is also similar to the WOA13 data. Compared with Exp 1, the difference for Exp 2 often decreases remarkably, the difference for Exp 3 is much smaller than that of Exp 1 and Exp 2 because of the incorporation of the IT-generated turbulent mixing, especially in the layers with depths between 20 m and 50 m. In addition, the improvement of the NBSW and IT along 7.5° N is not obvious (RMSEs decrease a little), this conclusion is also similar to that for the WOA13 data. This implies that the three mixing schemes introduced in this study may not be appropriate in the marginal sea simulation that if full of small- and meso-scale processes. In order to solve the issues about the accuracy, we attempt to design the high-resolution real-time numerical modeling experiments in the North Indian Ocean (or Arabian Sea and the Bay of Bengal only), as well as the finer simulation of the surface waves and more accurate estimation of the ITs.

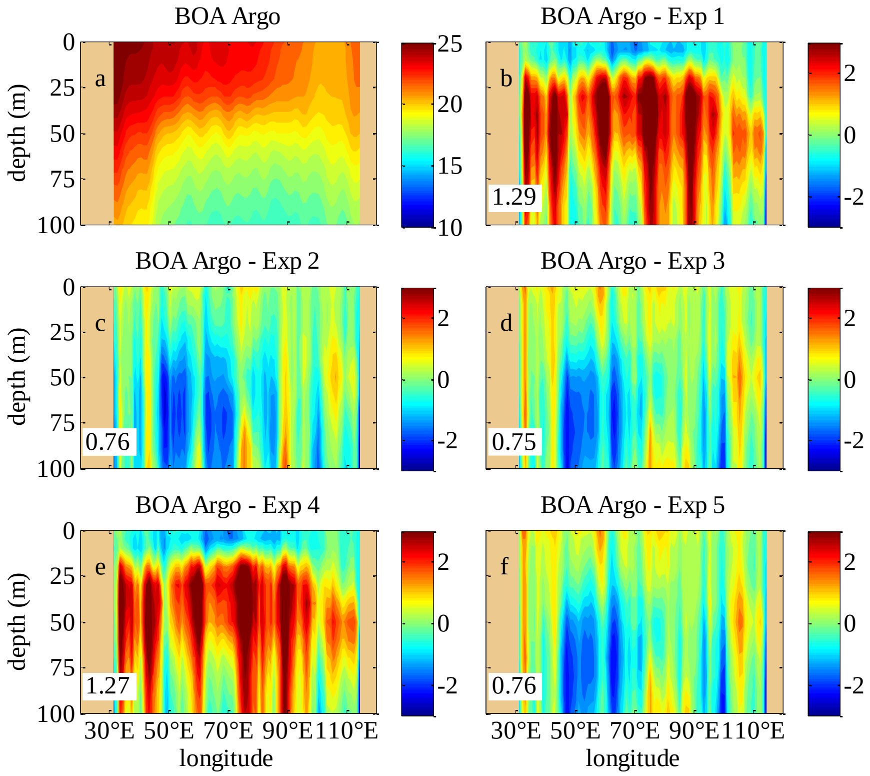


Figure 9. The vertical temperature profiles along 30.5° S in January. (a) The temperature structure from the monthly BOA-Argo data (units: °C). (b) - (f) The difference of the temperature calculated by subtracting the monthly mean results simulated in Exp 1 - Exp 5 from the monthly BOA-Argo data, respectively. The

RMSE of the temperature in the upper-100 m regions between the BOA-Argo data and the model results are given. Deep yellow areas correspond to the lands

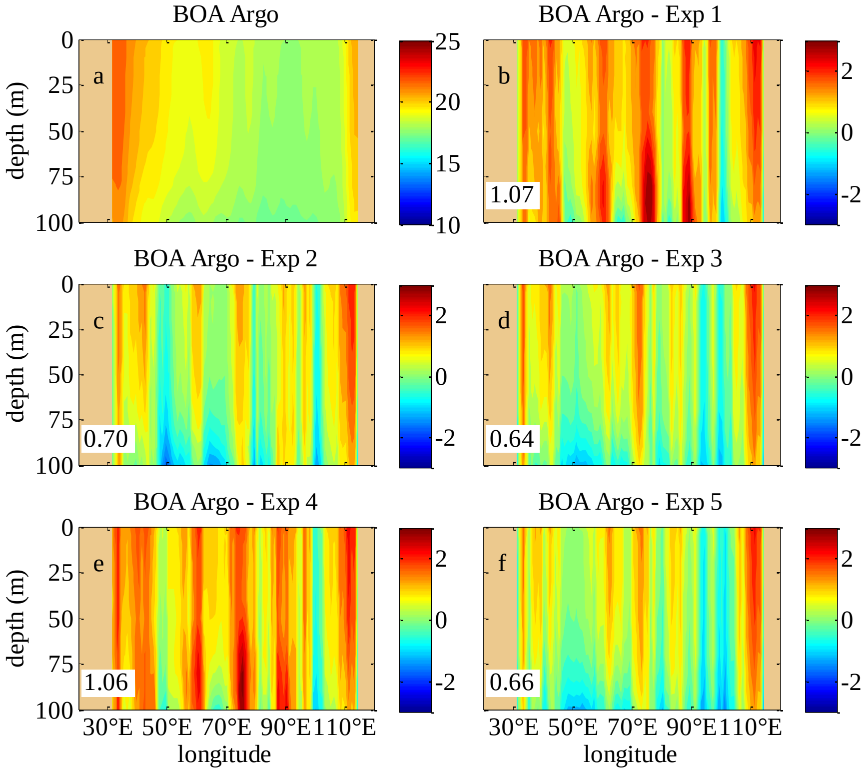


Figure 10. The same as Fig. 9, but in July

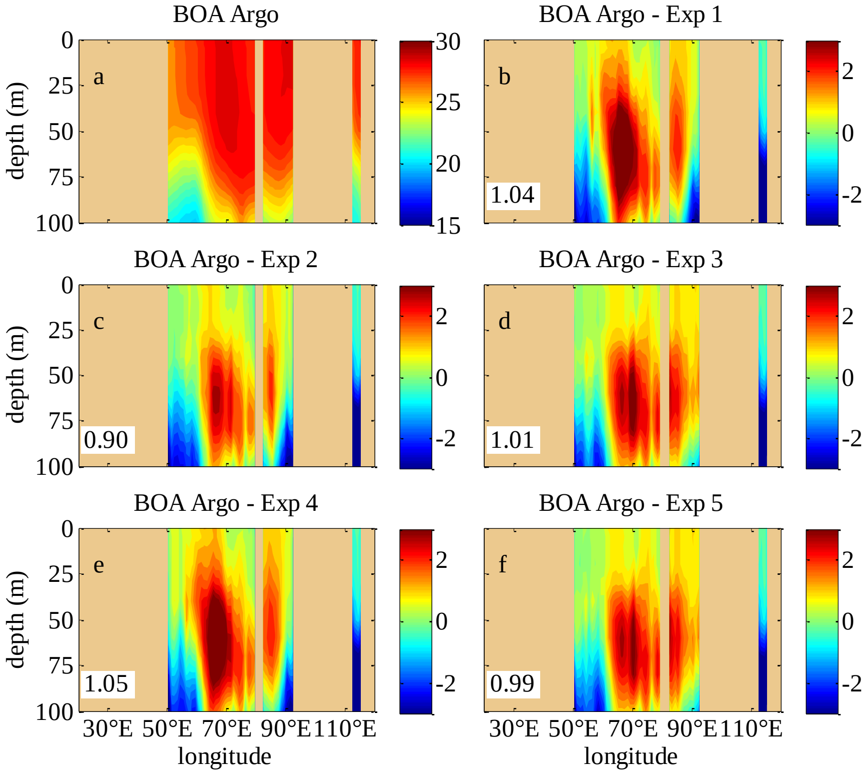


Figure 11. The same as Fig. 9, but along 7.5° N

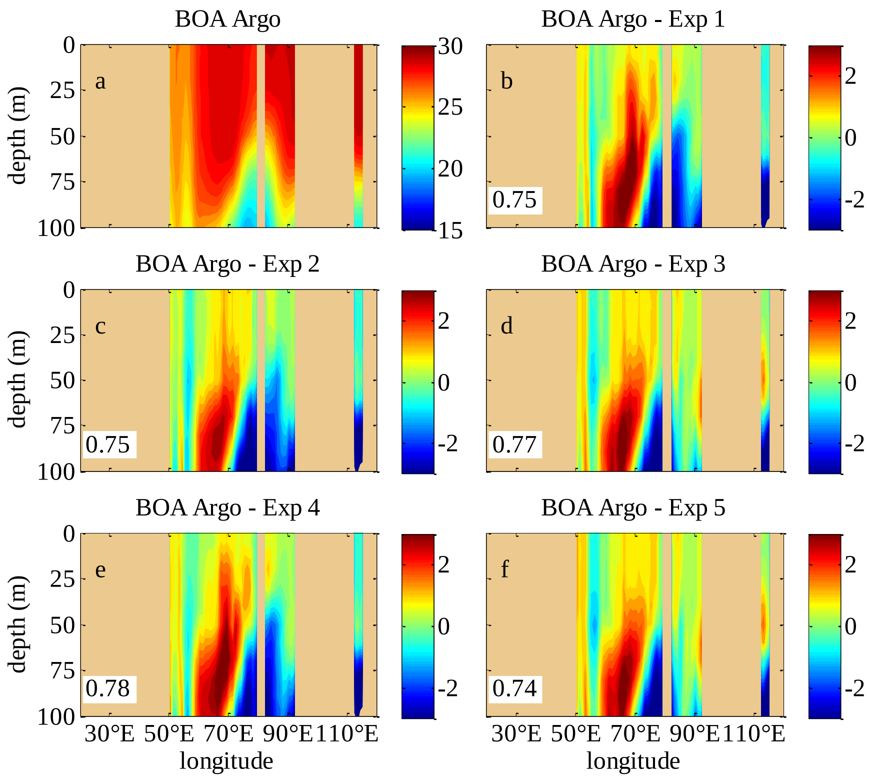


Figure 12. The same as Fig. 9, but along 7.5° N and in July

The analysis has been added in Line 507-532 of the revised manuscript. Figures 9 and 11 have been added in the revised manuscript.

Reference:

Li H, Xu F, Zhou Wet al. 2017. Development of a global gridded Argo data set with Barnes successive corrections. J Geophys Res Oceans 122(2):866–889

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.

Reply: Levitus94 data for Indian Ocean model initialization may not give realist climatological spatial pattern. It's too old and I guess hardly any representative data went onto this. I think author must use the recent WOA atlas may be 2013 or 2018 for the initialization.

We agree with the reviewer's comments. As we explained above (please see the analysis of Figures 5 and 6 in the response), the WOA13 data contains more meso-scale information than the Levitus94 data. The temperature from the Levitus94 data is cooler than the WOA13 data in the ACC region, while generally warmer in the IO with the depth from 200 to 500 m. The difference of the initial fields between the Levitus94 and WOA13 data may be one of the factors to make the simulated temperature in the IO warmer than the WOA13 data (Figures 2 and 3).

It is worth noting that, we attempt to carry out the experiments using WOA13 as the initial fields, this means that the works about the simulation and analysis should be done all over again. And we also find other factors that can improve the modeling. Therefore, now we (together with other colleagues) are carrying out a series of the **high-resolution real-time** numerical experiments for the circulation in the IO, to examine the influence of different parameterization (including vertical and horizontal mixing schemes), surface fluxes (different heat and momentum fluxes), open boundary conditions (quasi-global modeling results or HYCOM reanalysis products), initial fields (newly added following the reviewer's suggestion, the WOA18, BOA-Argo or HYCOM reanalysis products will be used). The model design was implemented referencing some previous studies such as Nagai and Hibiya, (2015).

Please refer to Line 389-392 and 458-470 of the revised manuscript.

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.

Reply: Explanation with respect to my above comment looks fine.

Many thanks for the reviewer's suggestion. We tried our best to revise the manuscript.

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 10th year of simulation.

Reply: Explanation with respect to my above comment looks fine.

Many thanks for the reviewer’s suggestion. We tried our best to revise the manuscript.

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?

Reply: Explanation with respect to my above comment looks fine.

Many thanks for the reviewer’s suggestion. We tried our best to revise the manuscript.

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?

Reply: Explanation with respect to my above comment looks fine.

Many thanks for the reviewer’s suggestion. We tried our best to revise the manuscript.

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.

Reply: Partially responded. If the authors compared it for Arabian Sea and Bay of Bengal, then why they did not gave the figures?

Following the reviewer’s comments, the simulated temperature structure in the Arabian Sea and the Bay of Bengal is compared with the WOA13 data. Three transects, including 11.5° N, 15.5° N and 19.5° N, are chosen to show the vertical distribution of the difference of the temperature (Figures 13 – 18). In July (Figures 14, 16 and 18), the NBSW and IT can improve the simulation obviously in the Arabian Sea, but do not work in the Bay of Bengal, especially the difference for Exp 3 (NBSW+IT) or Exp 5 (NBSW+IT+WTFR) became larger in the Bay of Bengal, which is a hot spot for generation of the ITs. This implies that the IT-induced mixing scheme may not be appropriate in the marginal sea simulation that is full of small- and meso-scale processes. In January (Figures 13, 15 and 17), the NBSW and the IT even have a negative effects on the modeling. In summary, the simulated temperature structure along three transects, which are located in the Arabian Sea and the Bay of Bengal, can be improved by the NBSW and IT partially, further test based on high-resolution and real-time experiments will be implemented in future. As we mentioned above, we attempt to design the numerical modeling experiments in the Arabian Sea and the Bay of Bengal only (or the North Indian Ocean), as well as the finer simulation of the surface waves and more accurate estimation of the ITs.

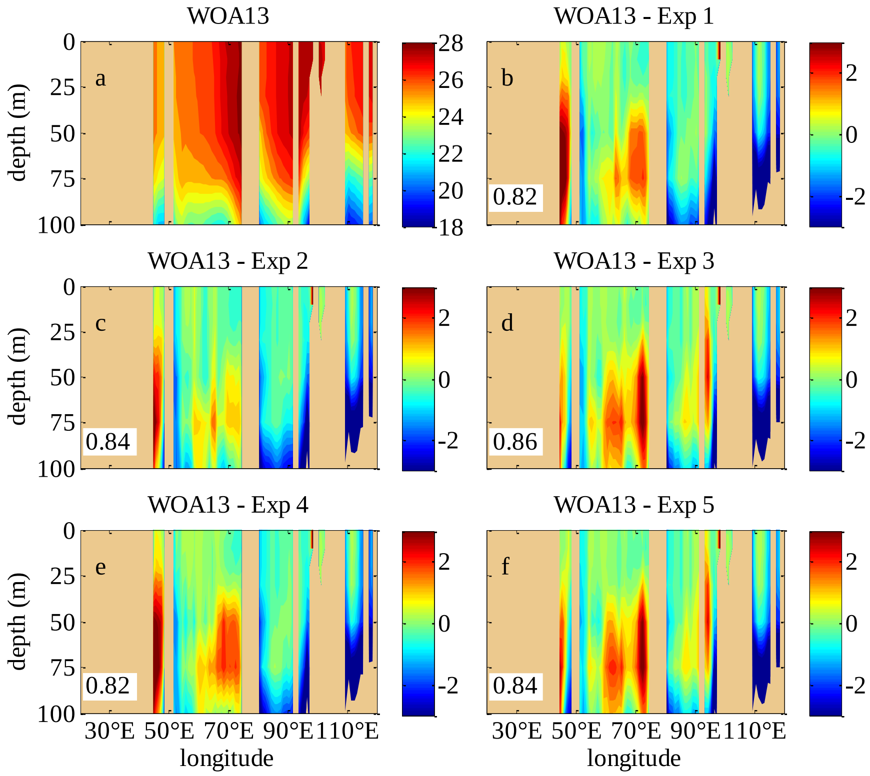


Figure 13. The vertical temperature profiles along 11.5° N in January. (a) The temperature structure from the monthly WOA13 data (units: °C). (b) - (f) The difference of the temperature calculated by subtracting the monthly mean results simulated in Exp 1 - Exp 5 from the monthly WOA13 data, respectively. The RMSE of the temperature in the upper-100 m regions between the WOA13 data and the model results are given. Deep yellow areas correspond to the lands

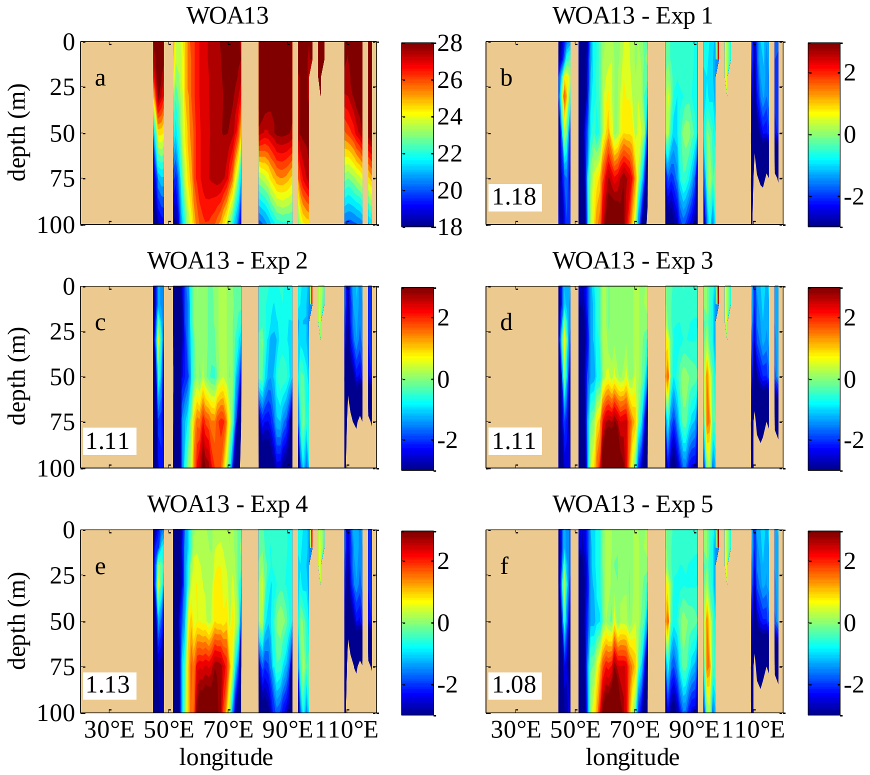


Figure 14. The same as Fig. 13, but in July

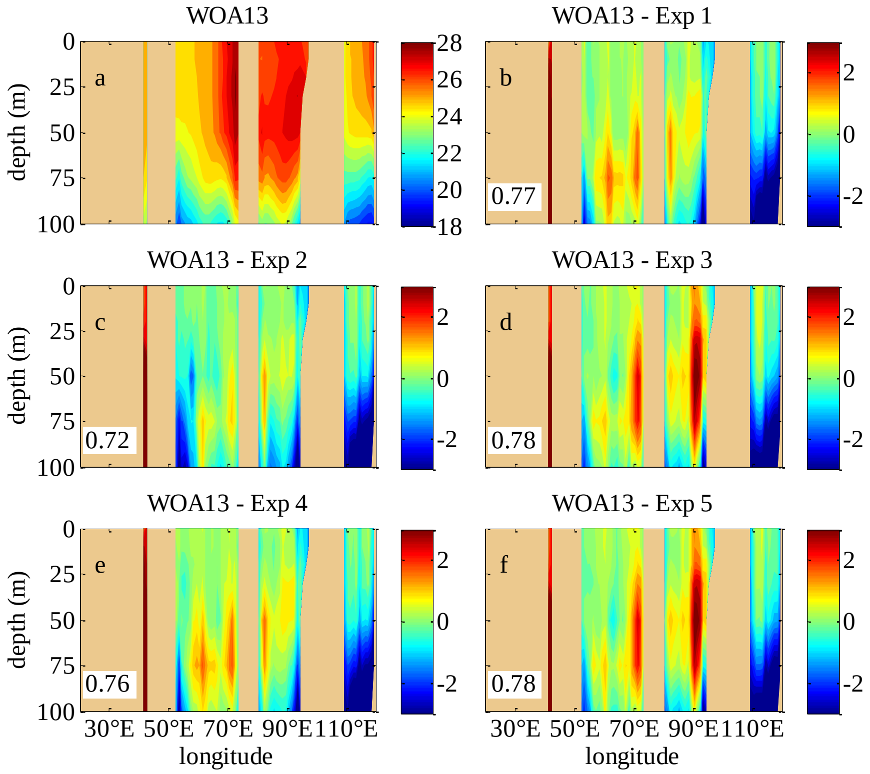


Figure 15. The same as Fig. 13, but along 15.5° N

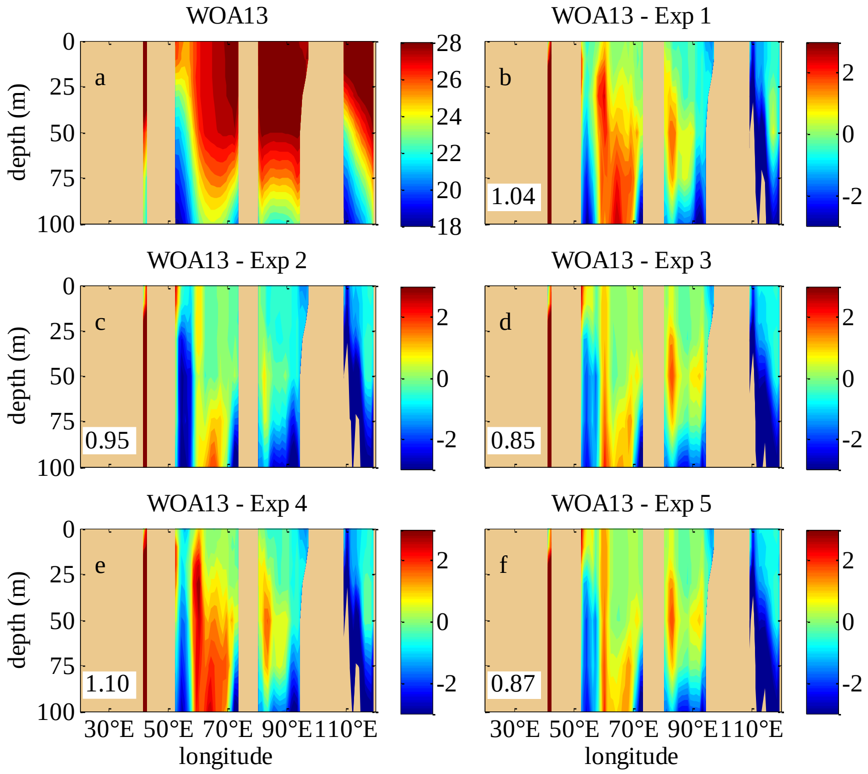


Figure 16. The same as Fig. 13, but along 15.5° N and in July

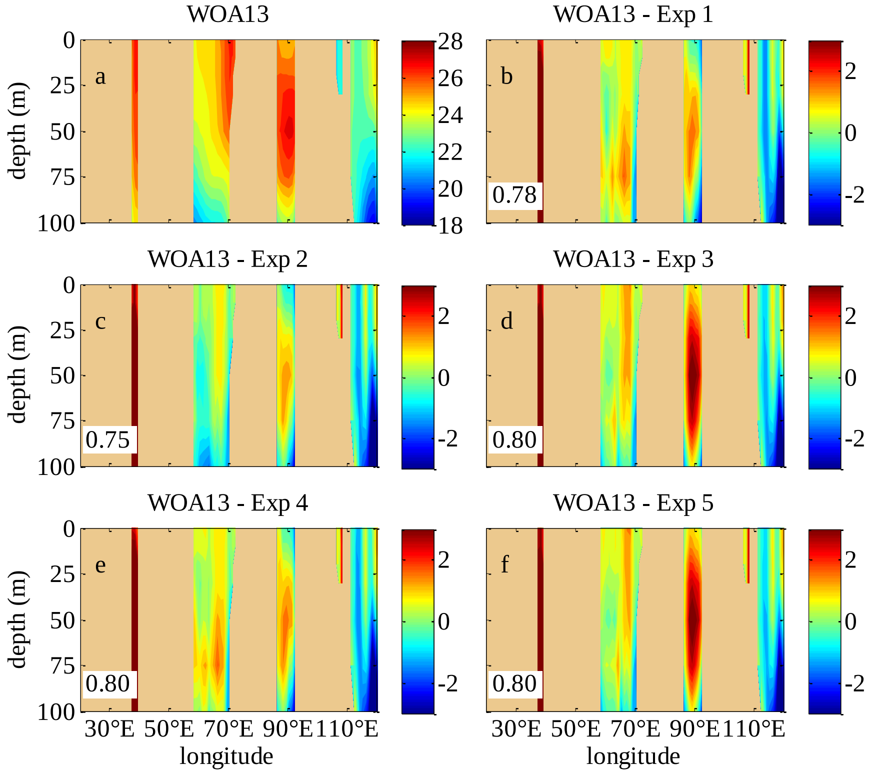


Figure 17. The same as Fig. 13, but along 19.5° N

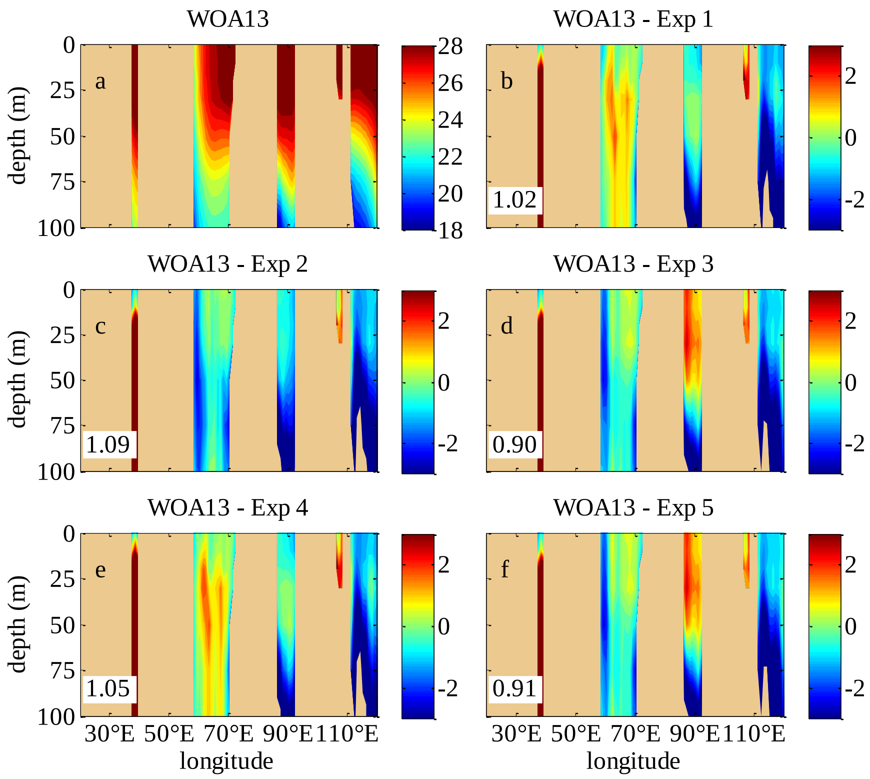


Figure 18. The same as Fig. 13, but along 19.5° N and in July

The discussion has been added in Line 660-664 of the revised manuscript.

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.

Reply: The reason given for cooler thermocline temp in Exp-1 as " The reason for the cooler temperature in the thermocline depth region in Exp 1 should be that the multi-year monthly mean surface forcing fields were smaller than the actual values, which leads to insufficient heat transfer from the atmosphere to the ocean. After 10 climatologic years modeling, the temperature in the ocean interior became cooler obviously than the WOA13 data." This explanation does not show any scientific argument. Not fully convinced. How does multiyear monthly mean surface forcing fields were smaller than the actual values will affect the simulation? The author made the forcing climatology I guess from 1948-2021. Similarly the explanation why the bias reversed in Exp-2 with respect to Exp-1 is also not complete. Author should give scientific evidence may be the complete heat budget for the explanation.

We agree with the reviewer, the explanation has been revised. The simulated temperature along 30.5° S is generally cooler than the WOA13 data in the thermocline depth region (see Figures 4 in the manuscript). We think that in addition to the explanation mentioned in the last response, the comparisons between the Levitus94 and WOA13 data (Figures 5 and 6) can give another better answer. The 30.5° S transect is near the ACC region, in which the Levitus94 temperature is cooler about 3°C than the WOA13 temperature from surface to 200m depth layers, and warmer about 0.5℃ in the NIO and tropics when the depth is deeper than 200 m. The comparisons of the temperature in the upper-300 layers (Figures 1 – 3) indicate that, although the simulated temperature is cooler than the WOA13 data along 30.5° S, the simulated temperature become warmer than the WOA13 data along 0.5° S and 7.5° N, inaccurate simulation of the meridional overturning circulation causes abnormal heat distribution (heat accumulates in the north of the IO and tropic region).

When the NBSW- and IT-induced mixing schemes were adopted, the vertical mixing was enhanced and carried the surface warm water (see upper-50 m regions in Figure 4b of the manuscript) to mix together with the cooler water below, so more heat entered into the ocean interior and the SST became cool. The climatologic surface heat fluxes used in these experiments are often small because of smoothing, the Haney method will ‘bring’ sufficient heat fluxes to make the SST maintain at a normal magnitude.

We agree that the complete heat budget should be analyzed to understand the mechanism, but it is a little inappropriate for the ocean modeling only, because the heat fluxes are not strictly conserved (the climatologic reanalysis product are used and adjusted by the Haney method). The new experiments will be carried out based on the atmosphere-ocean coupling models (also including current-tide-wave coupling) in future, we believe that the heat budget analysis must be one of the most important tasks.

Please refer to Line 389-392 and 445-470 of the revised manuscript.

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.

Reply: In that case there should be a zone in the equatorial region. It will not be good to include ITF in zone 2, zone 2 must be divided into two zones.

Many thanks for the reviewer's suggestion. The ITF forms into a narrow westward flow centered at about 12° S, within the South Equatorial Current (SEC), when it enters into the IO. The SEC carries the ITF waters westward across the IO. There is a complete cyclonic circulation system between the equator and 20° S, consisting of the westward SEC on the south side, the eastward South Equatorial Countercurrent (SECC) on the north side, and a northward western boundary current (East African Coastal Current; EACC). Furthermore, the effects of the M_2 internal tides are produced throughout the whole west region in Zone 2 (northern regions around the Madagascar Island). Therefore, in our opinion, the zone partitioning in this study should be appropriate.

The explanations have been added in Line 411-414 of the revised manuscript.

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.

Reply:"........ In fact, from Figures 10 one can see that, the obviously shallower MLDs are generally in the Antarctic Circumpolar Current (ACC) regions, where the simulated vertical mixing from the original experiment is weak dramatically." What about the Indian Ocean particularly in the Arabian Sea and Bay of Bengal.

In addition to the ACC regions, the obviously shallower MLDs also appear in the east regions of the Arabian Sea because of the weak vertical mixing. Furthermore, the simulated MLDs in most of the tropical and southern regions of the IO are shallower partially than the WOA13 data.

The analysis has been added in Line 553-556 of the revised manuscript.