Author's Response to Reviewer's Comment

Manuscript No. - egusphere-2024-2848

Title: **"**An evaluation of the Arabian Sea Mini Warm Pool's advancement during its mature phase using a coupled atmosphere-ocean numerical model"

Comments to Reviewer - 2

Review of "An evaluation of the Arabian Sea Warm Pool's advancement during its mature phased using a coupled atmosphere-ocean numerical model" by S. P. Lahiri, K. R. Prakash, and V. Pant, submitted to EGUsphere.

This paper aims to investigate the temporal progression of the Warm Pool using observations and a regional coupled model (COAWST but without waves). The simulations are quite short (~ 3 months) and are targeted at just one season. WRF component is run at 20km (inner nest) and ROMS is 1/6deg. The experimental and analysis approaches include control and sensitivity experiments for different years, mixed layer temperature budgets, and potential TKE analysis.
The paper has many interesting points and addresses an important climate topic. The experiments are useful, including the various helpful sensitivity experiments. However, there are some major limitations for which I give a Major Revision.

Reply*:*

We sincerely thank the Reviewer for your time and effort in thoroughly reviewing the manuscript. Your insightful comments have significantly contributed to enhancing the quality of the manuscript. Following your suggestions, we have restructured the manuscript accordingly. Your comments have been addressed individually, as outlined below, and the corresponding revisions are incorporated into the manuscript. Your comments are presented in black, and our responses are provided in blue italic font. For comments containing multiple queries, we have addressed each point as bullet points for clarity.

Major points

1. The simulations are rather short (April 1st to June 20th). My interpretation of lines 72-74 and 131-133 is that the coupled model develops biases when simulations are longer than a season (please clarify this).

It is possible than by April 1st, the mature phase has already started to develop, as a response to surface fluxes. In this case, the ocean initial conditions on April 1st are a function of the prior surface fluxes of the warming period. Your findings that ocean initial conditions on April 1st are important may not be inconsistent with Kurian and Vinayachandran (2007). If you had initialized on February 1st or March 1st, your statement on lines 423-424 ("contradicts… Kurian and Vinayachandran (2007)") would be reasonable, but for your current set up of April 1st initialization I think this line referring to Kurian and Vinayachandran (2007) should be removed.

Further, you analyze the heat budget from early May to June, but the warm pool has already considerably developed by early May. E.g. in your Fig. 8d,e,f there are only very brief periods in May where the temperature tendency is positive, it is mostly negative, a weakening. I realize you want to avoid the spin-up period, but perhaps you could do a heat budget anyway for April 1st to June and see if you capture any development of the warm pool (i.e. longer period of positive temperature tendency).

Reply:

- *In this study, we used a regional ocean-atmosphere coupled model where the WRF model was the atmospheric component. The WRF model is generally used for short-term simulation (ranging from a few days to seasonal scales). Based on previous studies and our own experiments, extending WRF simulations over longer periods tends to increase the biases, which can significantly affect the ocean model outputs. Also, our study was focused on investigating the relative contributions of the atmosphere and ocean to the development of the MWP throughout the mature phase. Due to this, we ran the coupled model from April 1st to June 20th in 2013, 2016, and 2018, respectively. Extending the simulation period beyond this was not feasible due to two primary factors: (1) the high computational cost and (2) the progressive increase in model biases.*

- *We appreciate the Reviewer's comment. The Reviewer brought up an intriguing topic on the origins of the Arabian Sea Mini Warm Pool (MWP). We agree with the Reviewer that* *the process of forming the MWP may begin well before April. However, there is disagreement within the scientific community over the exact timing of the mechanisms that contributed to the establishment of the MWP. Akhil et al. (2023); Kurian & Vinayachandran (2007); Mathew et al. (2018) suggested that the development of the MWP does not depend on the antecedent winter stratification and thus, ocean pre-condition has very little influence on the MWP genesis, whereas Durand et al. (2004); Gopalakrishna et al. (2005); Hareesh Kumar et al. (2009); Masson et al. (2005); Nyadjro et al. (2012) reported that the stratification does have a prominent influence on the MWP formation and without the winter stratification the MWP strength could decrease by 0.5°C (Masson et al., 2005). Thus, the present study aims to decipher the impact of the ocean and atmosphere on the MWP during its mature phase, as well as the elements that contribute to the MWP's dissipation. We have concluded that atmospheric processes, particularly wind patterns, determine the spatial variability of the MWP. Nonetheless, the ocean pre-conditions before April have a major effect on MWP strength.*

- ♦ *We agree with the Reviewer that net surface heat fluxes during pre-monsoon have already been stored in the ocean pre-condition before April, and the influence of the ocean pre-condition on MWP genesis indirectly reflects the influence of surface heat fluxes during pre-monsoon. As a result, we will need a longer simulation to reach this conclusion. Therefore, following the Reviewer's suggestion, we have removed the statement 'This contradicts previous studies, such as Kurian and Vinayachandran (2007), which suggested that MWP development in May was independent of the pre-April ocean conditions.'*

- ♦ *The ocean heat budget allows us to understand the impact of multiple factors (net surface heat flux, horizontal advection, vertical process, etc.) on variations in mixed layer temperature tendency. The mixed layer heat budget from May 1st to June 10th showed that the temperature tendency in the MWP core region was mostly determined by net surface heat flux. Furthermore, the net surface heat flux, together with vertical processes, caused MWP's dissipation.*

  The Reviewer advised showing the mixed layer heat budget from April to June. We can understand the Reviewer's concern regarding the fewer positive temperature tendency days *from May to June. However, if we show the mixed layer heat budget from April to June, the importance of different processes on the MWP development and dissipation does not change. Besides, in this whole study, we have left the April month for spin-up time and analyzed the MWP and associated processes from May to June (we have mentioned it clearly in line 167-168 in the revised manuscript). Thus, including the spin-up time analysis only in the mixed-layer heat budget could break the study's consistency. Subsequently, we have restricted ourselves from showing April's mixed layer heat budget in the main manuscript. As the discussion section is open in EGU Ocean Science, we have shown the figure here and have mentioned this in the Fig. 11 caption of the main manuscript. Interested readers can find it.*

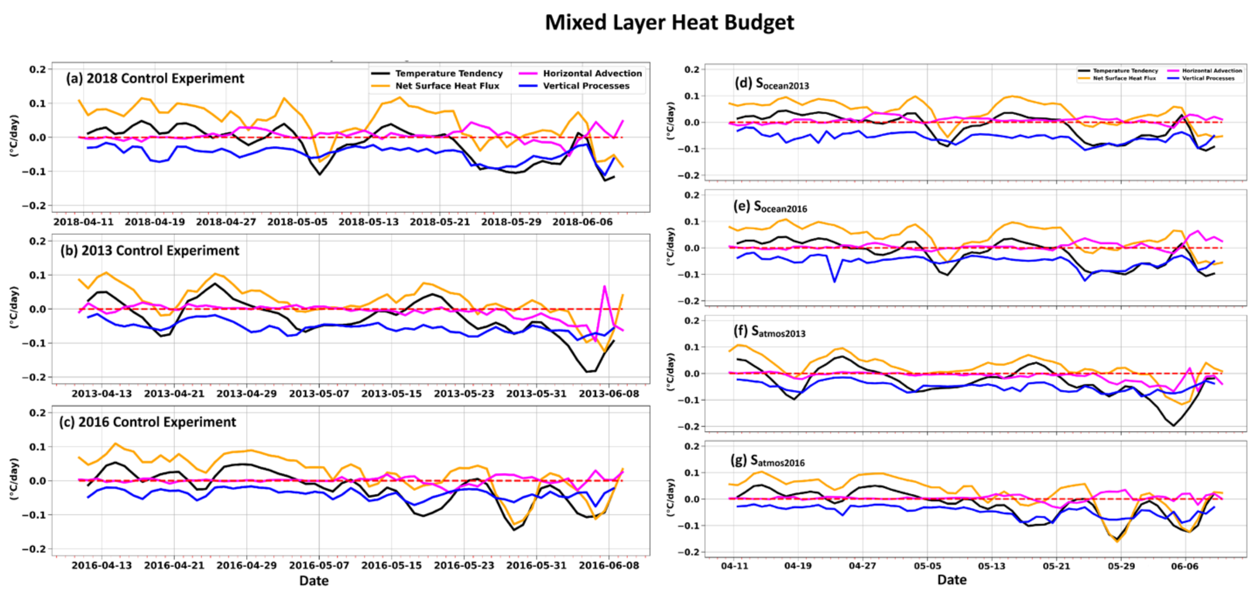

Fig R 1 Area averaged (72- 76°E and 7-13°N) mixed layer heat budget for three control ((a) 2018 control experiment, (b) 2013 control experiment, and (c) 2016 control experiment) and four sensitivity experiments ((d) $S_{ocean2013}$, (e) $S_{ocean2016}$, (f)$S_{atmos2013}$, and (g) $S_{atmos2016}$). In the sensitivity experiments, the oceanic and atmospheric conditions have been changed to various years; thus, only the day and month are kept on the x-axis (d to g).

2. I found the section 3.3.2 on potential TKE production weak, mainly because the figures were too difficult to interpret. A probable improvement would be to provide timeseries for box averages of the terms in Fig. 13-15, for the region boxed in Figs 2c,f,i).

Reply:

We sincerely thank the Reviewer for this comment. The P_{TKE} caused by haline, or thermal buoyancy flux, has less magnitude than the P_{TKE} caused by wind. Due to this reason, we have focused on the P_{TKE} caused by wind. Thus, the P_{TKE} caused by the wind for all the experiments is combined and kept as a single figure in the revised main manuscript (Fig. 14), and the P_{TKE} caused by haline and thermal buoyancy flux is moved to the supplementary section (Fig. S11 and S12). The spatial pattern of wind stirring is very important as it gives us information about the weak wind zone in the southeastern Arabian Sea as the spatial extent of the MWP expands within this weak wind zone.

Following the Reviewer's advice, we have shown the line plot of the P_{TKE} caused by wind stirring, haline, and thermal buoyancy flux over the MWP core (Fig. 13 in the main manuscript) along with the P_{TKE} caused by wind stirring (Fig. 14 in the revised manuscript). This certainly helped to understand the relative importance of wind stirring, haline and thermal buoyancy flux on the P_{TKE}.

3. Map figures, e.g. Fig. 2, 3,4, 6, 7, 9, 10. The figures show a vast area of the Indian Ocean, and it can be hard to see the region of interest. I understand that you sometimes want to display large-scale features, but for some figures you can show a smaller area, e.g. north of 5°N and east of 60° For example, in Fig. 4 it is hard to see coastal currents, and a smaller display region focused on the coasts would be better. For Figs 13-15, replace with timeseries plots as in Major Comment 2.

Reply:

We wholeheartedly thank the Reviewer for this suggestion. The idea behind showing the vast area was to show the large-scale processes, especially the wind flow, as shown in Fig. 6. Besides, we aimed to show the model validation over the whole model domain in Fig. 2 and 3. However, we agree with the Reviewer that the coastal currents are not properly visible in Fig. 4. Hence, we have shown the currents near the west coast of India following the Reviewer's suggestion. We have also marked the domain of interest (the MWP core) in all the figures, which showed a vast area. Fig. 13 in the revised main manuscript shows the time series of the P_{TKE}.

Line-by-line points

1. Line 13-14. See Major Comment 1.

Reply:

We have addressed the Reviewer's concern regarding the simulation time and the longer period of the mixed layer heat budget in the major comment one, where we have shown the temperature tendency from April to June. Please have a look at the comments of the major comment 1.

2. Line 151. An earlier reference is Stevenson and Niiler 1983 - https://doi.org/10.1175/1520-0485(1983)013%3C1894:UOHBDT%3E2.0.CO;2

Reply:

We thank the Reviewer for this suggestion. The above citation is added. Please look at the lines 180-181.

3. Lines 169-170. The expression in Han et al. 2001 does not include the von Karman constant, and I could not find this expression in Rao et al. 2002. Please check, I may have missed it.

Reply:

We have used the Von Kärmän constant from Rao et al. (2002). We request you to look closely at the Section 3 ("Causes for the Absence of Cool SST Anomalies in the Bay of Bengal") of Rao et al. (2002) for the value of Von Kärmän constant (k = 0.42).

4. Lines 208-214. A bit more detail on the analysis – were you looking at daily or monthly variability? How long were the data records? Do you remove any seasonal cycle or trend?

Reply:

We thank the Reviewer for this comment. We have looked at the daily variability at the point location nearest to the AD10 buoy location. We used the AD10 for model validation purposes. Thus, we have kept the original data and have not removed any seasonality or trend. In the main manuscript, we have added a few details regarding the comparison between the AD10 and the model's ability to simulate the vertical temperature and salinity profile. Kindly have a look at lines 230-247.

5. Line 189-190. Please put the time period also in the caption of Fig. 2.

Reply:

We have added the time period in the captions of Fig.2, 3, and Fig. 4. Thank you for this suggestion.

6. Lines 253-257. This process is hard to see in Fig. 6. Please explain more or delete.

Reply:

The manuscript is already quite lengthy, and the south-eastward transport of salinity is not very relevant to the objective of this study. Hence, we have removed this information from the manuscript. Thank you for this advice.

7. Line 281 “enhance”->“allow”.

Reply:

We have incorporated this comment. However, following the Reviewer 1’s suggestion, we have moved it to the section 3.3.1. Please have a look at the lines 358-359.

8. Lines 291-300. See Major Comment 1.

Reply:

We thank the Reviewer for this comment. This particular comment is addressed in detail in the major points 1.

9. Line 303. At this point, remind readers that the focus (control) year is 2018, and sensitivity experiments impose non-2018 conditions.

Reply:

We thank the Reviewer for this comment. The section 3.3 is restructured following the comments of both the Reviewers. In this process, we have included this information in section 3.3.1. We request the Reviewer to have a look at the lines 325-334.

10. Figs 9-10. Please show 2018 control as an additional row for comparison.

Reply:

We thank the Reviewer for this insightful comment. We have added the 2018 control experiment in an additional row in Fig. 9 and 10.

11. Section 3.3.2 See Major Comment 2.

Reply:

We thank the Reviewer for this comment. We have added the time series to this section. Please see Fig. 13.

12. Lines 423-424. See Major Comment 1.

Reply:

Following the Reviewer's Major Comment 1, we have removed this information in the updated manuscript. Thank you for the advice.

Reference:

Akhil, V. P., Lengaigne, M., Krishnamohan, K. S., Keerthi, M. G., & Vialard, J. (2023). Southeastern Arabian Sea Salinity variability: mechanisms and influence on surface temperature. Climate Dynamics, 61(7–8). https://doi.org/10.1007/s00382-023-06765-z

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Nyadjro, E. S., Subrahmanyam, B., Murty, V. S. N., & Shriver, J. F. (2012). The role of salinity on the dynamics of the Arabian Sea mini warm pool. Journal of Geophysical Research: Oceans, 117(9). https://doi.org/10.1029/2012JC007978

Rao, S. A., Gopalakrishna, V. V., Shetye, S. R., & Yamagata, T. (2002). Why were cool SST anomalies absent in the Bay of Bengal during the 1997 Indian Ocean Dipole event? Geophysical Research Letters, 29(11). https://doi.org/10.1029/2001GL014645