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 - 1

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.

I want to recognize the effort the authors have made after the first round of reviews. The manuscript is noticeably improved. However, I do have some additional comments.

Following my original comment #4 and Major point 1 from the other reviewer regarding the short spin-up and how close to the time frame of interest the spin-up window is (particularly considering ocean memory). The authors should make a comment about this in the manuscript much like in their response to reviewer 2. In addition, the point Reviewer #2 made about the ocean pre-conditioning/memory is very important even if the paper focuses on the mature and dissipation phases. I believe these merits further clarification/justification in the manuscript.

Reply:

We sincerely thank the Reviewer for the encouraging words. Your time and effort in thoroughly reviewing the manuscript is much appreciated. These comments and discussion have substantially contributed to enhance the scientific quality of the manuscript. Following your suggestions, we have edited 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.

- *We agree with the reviewer that the spin up time is less in our configured numerical model. However, as we have already reported in our previous comments that in our reanalysis*

feed coupled atmosphere ocean numerical model (the atmospheric and oceanic initial and boundary conditions are given from reanalysis data), one month spin up time is sufficient to solve the mixed layer processes. Besides, the computational cost increases exponentially with an increase in the spin up time. Thus, we have restricted ourselves to one month spin up time. However, following the comment 1 of the Reviewer 2, we now have shown the mixed layer heat budget from mid-April although it is within the spin up time. This further clarifies the impact of the net surface heat flux on the temperature tendency of the mini warm pool.

- *One of the major findings of our study is that the ocean precondition plays a dominant role in the genesis of the mini warm pool. Thus, we are agreeing with the reviewer that the ocean memory (or ocean precondition) indeed plays an important role for the development of the mini warm pool. However, a substantial debate regarding the actual timeline of the ocean warming before the formation of the mini warm pool still persists within the scientific community (please have a look at the introduction especially lines 61 to 69). It is quite possible that a strong mini warm pool could start to develop from the antecedent year. However, the objective of the present study was to investigate the relative role of the atmosphere and the ocean-pre-condition on the mini warm pool's advancement during its mature phase. Since, we have concluded that the ocean pre-condition indeed impacts the mini warm pool formation, it becomes very important to study the timeline of the warming in the southeastern Arabian Sea before a strong mini warm pool formation. We have added this information in lines 475 to 480 in the conclusion section. Please have a look at it.*

Line 92: Please include Bonjean and Lagerloef (2002) for the OSCAR dataset.

Reply:

We thank the reviewer for pointing this out. We have added the citation. Please have a look at the lines 93-94.

In the response to comment 9 on my original review regarding SST biases when comparing the model to AVHRR, the authors say "However, most of these biases are within 1.5-2C." But in the revised manuscript the authors still state "The SST bias remained within 1C in all three experiments except for the northern Arabian Sea along the somalian coast." (line 218). In the SEAS region, Figure 2f shows a signal with a bias over 1oC.

Reply:

We have missed to add the information. It is corrected in the revised manuscript. Please have a look at the lines 219. Thank you for pointing this out.

The authors now include a link to the AD10 buoy data as part of the Data Availability Statement, but the link or some other reference should be included in Line 230 when the buoy is first mentioned. Figure S2 should include the box with the SEAS region of interest.

Reply:

- The link to the AD10 buoy data is added in the lines 94-97 in the Data and Methodology section (the buoy is mentioned here for the first time).
- The SEAS region is marked in the Fig. S2.

We thank the reviewer for pointing this out.

The validation in 2013 with the buoy is questionable (Figure S4). You are missing the top ~10 m in temperature and salinity. The salinity data from AD10 shows a large discrepancy near the data gap during the first third of the time series. Given how important the surface signals are for the purposes of the research, the lack of data in the top 10m and the odd signal in salinity leads me to question the robustness/usefulness of this validation within the context of the manuscript. Furthermore, the validation with the buoy data is a relatively long paragraph in the main text but all figures are relegated to the supplemental (except for the Taylor diagram that is only a couple of sentences.

Reply:

- We can understand the reviewer's concern regarding the model's performance in 2013. AD10 data has a substantial data gap within top 10 m. Hence, we can't compare the vertical temperature and salinity within this depth. But below that the configured coupled model validates well for both temperature and salinity and the bias remains within 1°C for temperature and 1psu for salinity. Besides, the surface salinity and temperature are well represented in the numerical model when compared with the satellite data in 2013 (Fig. 2 and 3). This gave us confidence to further use our configured model for 2013 sensitivity experiments.
- Following the reviewer's advice, we have removed few lines from the vertical temperature and salinity validation section in the main manuscript. We request the reviewer to please go through the lines 231-248.

Line 239: "The MWP does not extend beyond 50m…" Please include a reference for this.

Reply:

We have added a reference here. Thank you for pointing this out. Please have a look at the lines 239-240.

As previously requested by both reviewers, please include the region of interest in all figures and panels (not just the difference panels).

Reply:

We have added the region of interest in all the figures. Thank you for reminding this point.

Line 293-294: Figure 7 panels g, h, I do not seem to support the statement that salinity in the vicinity of the MWP was lower during its mature day but increased during its dissipation day. Figure 7i does not show an overall positive difference which would support the statement. Also, It is not clear to me how low salinity being slightly outside the core MWP area contradicts Kumar et al. 2019 as stated in lines 295-296. Maybe some rewriting with additional details could clarify this better.

Reply:

We sincerely thank the reviewer for this suggestion. We have rewritten the sentences. Please have a look at the lines 293-297.

Lines 446-447: I found these two sentences confusing, please be a bit more specific on what you are trying to say. For example in the second sentence, It is not clear what the authors are trying to say with regards the experiments.

Reply:

We have rewritten the statement here for better clarity. Please have a look at the lines 447-450.

Lines 449-450: I would suggest to rewrite this sentence also to be more precise. For example: "This net surface heat flux drives the dissipation of the MWP with contributions from the vertical processes. This result indicates that atmospheric processes control the MWP's …"

Reply:

We have rewritten the statement here for better clarity. Please have a look at the lines 447-450. Thank you for your suggestion.

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.

The authors have carefully considered my comments and improved the manuscript. The paper is generally, well written, except for minor points, some of which are noted below. However, I still have one major comment, so that I give a Major Revision.

Major points

I have looked again at the heat budget results in Fig. 11 and your Response fig. R1. In Fig. 11 there is hardly any actual warming (positive temperature tendency) in any of the panels. In the extended Response figure there is more sign of warming: in 2018 it is positive from early April towards end of April, in 2016 it is mostly positive in April except for a few days: while in 2013 two warming periods are separated by a ~ week long period of cooling.

Based on this, if you use heat budgets from May onwards, I think the sentence in the Conclusions "(Fig 11) revealed that the net surface heat flux is the primary driver of the MWP development" is hard to justify, as you do not actually show substantial warming (positive tendency) in the heat budget. Even if surface heat flux contributes 0.1deg.C per day, it can be cancelled out by other processes so the net tendency is small (e.g. Fig. 11a, May 13-22).

Your results are better for looking at the dissipation process, where surface fluxes and vertical processes play a large role. If you want to relate it to the maturing phase, I think you have to include April, even though it is spin-up.

Alternatively, it might be useful to compare heat budgets in the mini-warm pool with points

outside the warm pool. From the difference of these two locations, is more surface heat flux leading to more positive tendency in the warm pool?

I request that you re-write the sentence in the Conclusions to reflect the thoughts above.

Reply:

We sincerely thank the Reviewer for the encouraging words. Your time and effort in thoroughly reviewing the manuscript is much appreciated. These comments and discussion have substantially contributed to enhance the scientific quality of the manuscript. Following your suggestions, we have edited 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. The major comments are in blue bold italic color font. For comments containing multiple queries, we have addressed each point as bullet points for clarity.

- ***We agree with the statement that the influence of the net surface heat flux on the temperature tendency of the mini warm pool is much evident from April onwards. However, for the sake of consistency (as we have excluded the spin up time for the analysis in the whole manuscript), we did not intend to keep the longer timeseries of the mixed layer heat budget in the main manuscript. But, following your major comment, we have now replaced the mixed layer heat budget from May with the mixed layer heat budget from April (Fig. 11 in the revised manuscript). This certainly helps to understand the influence of the net surface heat flux on the MWP temperature tendency during the mature phase.***
- ***To better comprehend the influence of the net surface heat flux on the mini warm pool temperature tendency, we have compared the mixed layer heat budget in the mini warm pool (7-12^{o}N & 72-77^{o}E) (Fig. R1 a-c) and 5^o west of the mini warm pool (7-12^{o}N & 60-65^{o}E) (Fig. R1 d-f). A substantial decrease in the temperature tendency is observed in the west of mini warm pool in all the three years. The impact of the net surface heat flux on the temperature tendency is also less here. However, in the vicinity of the mini warm pool (Fig. R1 a-c), the temperature tendency is more and so as the influence of the net surface heat***

flux on the temperature tendency, indicating the importance of the net surface heat flux in the development of the mini warm pool during its mature phase.

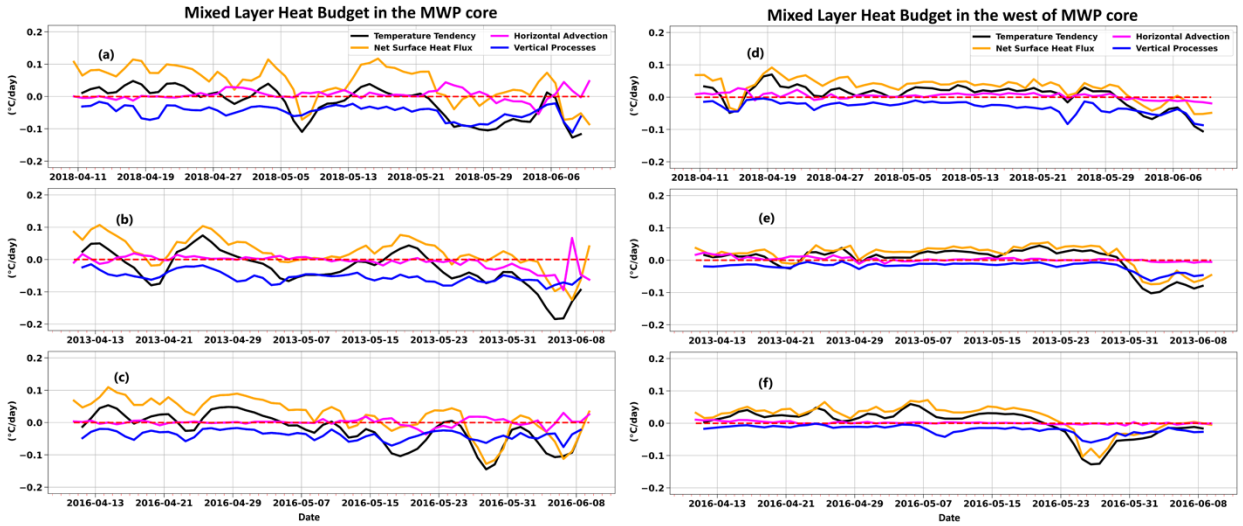

Fig R 1 Area averaged mixed layer heat budget in the mini warm pool core region (72- 76°E and 7-13°N, i.e., the white box shown in Fig. 1) and 5° west of the mini warm pool region (72-76°E and 7-13°N, i.e., the white box shown in Fig. 1) for three control ((a and d) 2018 control experiment, (b and e) 2013 control experiment, and (c and f) 2016 control experiment).

After keeping the mixed layer heat budget from April onwards, the influence of the net surface heat flux on the mini warm pool temperature tendency becomes more evident. Besides, it now supports our conclusion statement i.e., "(Fig 11) revealed that the net surface heat flux is the primary driver of the MWP development".

Minor points

Line 228. The coastal currents are still very hard to distinguish. Perhaps zoom in (leave larger plots for supplemental) and plot arrows more frequently and change color scale.

Reply:

We thank the reviewer for pointing out this. The coastal currents along the west coast of India

remains in the transition phase during the May and late June and it strengthens in July-August (Fig. R2). Thus, we are not seeing any prominent current along the west coast, indicating the model's capability in capturing the current precisely. However, we have now dense the arrow and changed the color scale limit. We hope that the currents are much more visible now.

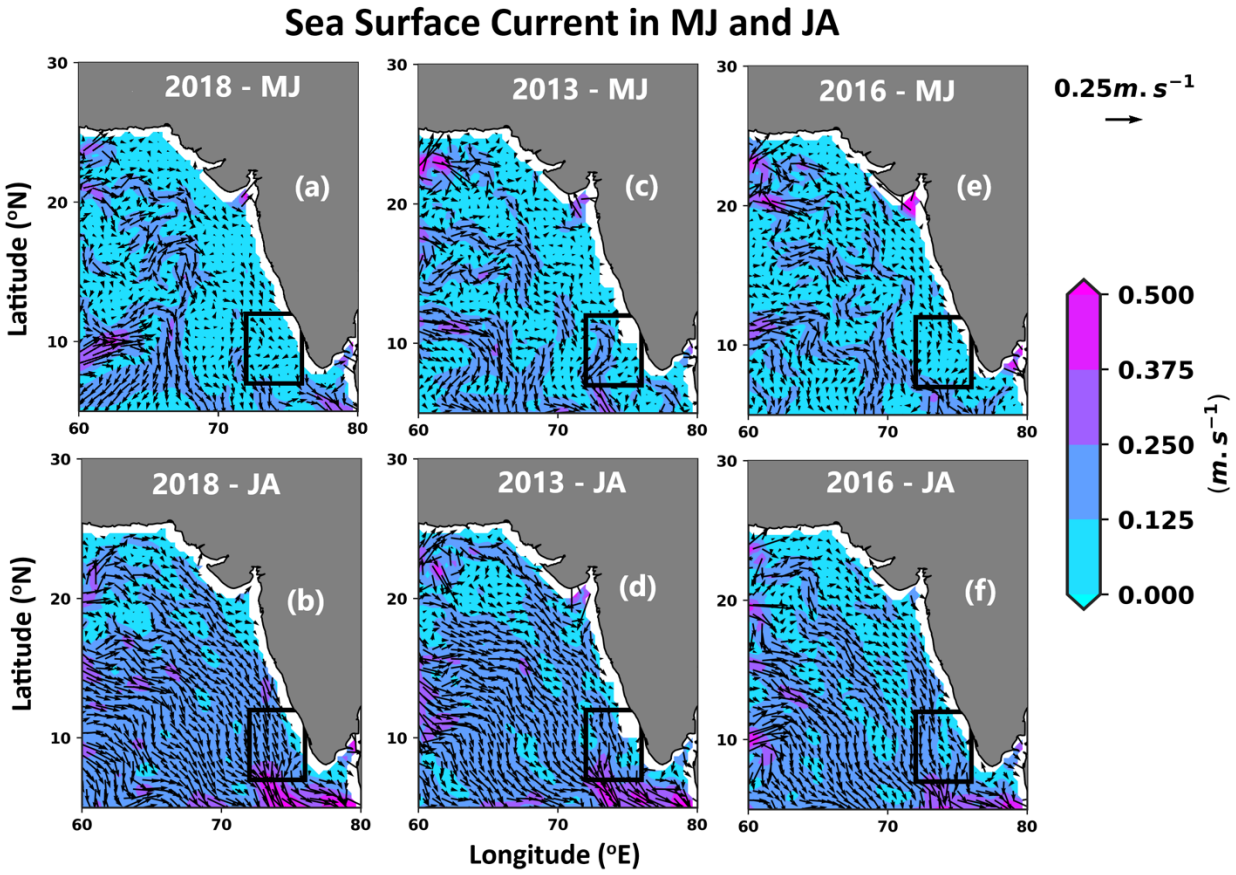

Fig R 2 OSCAR surface current along the west coast of India during May-June(MJ) and July-August (JA) for the year 2018, 2013 and 2016.

Lines 444-445. Net surface heat flux is the primary driver behind dissipation in 2016, but in the other years vertical processes are also very important.

Reply:

We have rewritten this sentence as "Thus, the net surface heat flux along with vertical processes emerges as the primary driver behind the dissipation of the MWP (Fig. 11)." Please have a look at the lines 445-446. Thank you for pointing this out.

Lines 450-451. Vertical processes are influenced by the atmosphere (e.g. wind) and ocean (e.g. stratification, mixing) so I would not say it is all “atmospheric processes”.

Reply:

We thank the reviewer for bringing this point to our attention.

The mini warm pool extends till the mixed layer depth, and it expands following a weak wind zone. However, once the southwesterly monsoon wind strengthens it causes strong vertical mixing that negatively impact the mini warm pool’s temperature and leads to its dissipation.

Besides, the moisture rich southwesterly wind causes cloud formation which restricts the incoming shortwave radiation and at the same time the loss of latent heat flux due to evaporation. This leads to the negative influence of the wind on the net surface heat flux which eventually affect the temperature tendency of the mini warm pool.

Thus, once the ocean pre-condition laid a favorable condition, the mature to the dissipation day of the mini warm pool is driven by the wind. We have modified this in the lines 447-450.

Wording Changes:

1. Line 152. “boundaries were closed” – strictly the boundaries are over land anyway.

   Reply:

   We have re-written the sentence as “Northern and western boundaries were closed in the ROMS model.” See lines 153-154.

2. Line 152. Plural “models”

   Reply:

   This is rectified in the manuscript. Please have a look at the line 154.

3. Line 161 “simulated the model” -> “ran the model for about 80 days”

   Reply:

   This is modified in the lines 163-164.

4. Line 167. Reword to “separately on April 1st and run to June 20 each year”

Reply:

The line is rewritten. Please check line 169.

5. Lines 187-188 “The MLD is the depth h where the following criterion is first met” ?

Reply:

We are not sure what the reviewer wants to mean here. If this is about the mixed layer depth calculation, then we request the reviewer to see the lines 194-198.

6. Line 195. Delete “represented in the”

Reply:

We have reframed this in line 192-193.

7. Move lines 191-196 to after the sentence ending in “h is the mixed layer depth (MLD).” Then move lines 186-190 to the end of the sub-section.

Reply:

We have reframed this section as suggested. Please have a look at the lines 186-195.

8. Line 235 “anticipated”-> “simulated”

Reply:

We have replaced the word. Please have a look at the lines 235-236.

9. Line 276, figure 5 caption. Move “points at 50m… for 2013” to main text instead.

Reply:

We thank the reviewer for this comment. We have added this information in the main text line no 245.

10. Line 330 “till” - > “to”

Reply:

This is incorporated in the text. Please have a look at the lines 329-331.

11. Line 335. “the 2013 atmospheric conditions replaced those from 2018”

Reply:

The lines are reformed in 335-336.

12. Fig. 11 caption. Please remove the last sentence for the print version.

Reply:

We have removed this in the revised manuscript.

13. Line 404. “minimal” - > “opposite sign and smaller magnitude”

Reply:

Please have a look at 404-405 for the updated version.

14. Fig. 13 use same vertical axis scale for all panels. Move “multiplying 10^4” to the vertical axis label.

Reply:

The vertical axis is kept in all the panels and “multiplying 10^4” is moved to the vertical axis label.

15. Line 417. “was unfurled over” is too fancy, I suggest “expand to”

Reply:

We have incorporated this in the lines 418.

16. Lines 446-449 have confusing wording. Please rewrite.

Reply:

These sentences are re-written. Please have a look at the lines 447-450. We thank the reviewer for this suggestion.

17. Line 453. “rise of 41%” -> “decrease of 41%”? See line 393.

Reply:

We have restructured the whole section for clarity. Please have a look at the lines 447-458.

18. Line 457. “S_atmos2016”->”S_ocean2013” ? See line 388.

Reply:

We have restructured the whole section for clarity. Please have a look at the lines 447-458.

19. Line 458. “it’s” to “it is” reads better.

Reply:

20. We have restructured the whole section for clarity. Please have a look at the lines 447-458.

21. Line 480-481. Delete sentence beginning “Given that …”. Reword as “We hypothesize that the wind shadow zone and the corresponding increase…”

Reply:

We have modified this sentence. Please have a look at the lines 481-483.