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

Prasad Lahiri et al. employs the use of a coupled ocean-atmosphere regional model (ROMS + WRF) to examine processes contributing to the mini warm pool in the Arabian Sea. They examine three events of the warm pool in their model and attempt to determine the relative contributions between ocean and atmospheric processes driving the strength and dissipation of these events.

While I find the concept of the study particularly interesting and potentially a good contribution to literature in the future. Nevertheless, significant revisions must be made before this manuscript can be reconsidered for publication.

As detailed throughout the comments below, there is a need to restructure the manuscript text in general to make it clear and concise. There is an extreme overuse of conjunctive adverbs such as "moreover", "however", "nonetheless" and many others that is in several cases is erroneous. There are missing references in several parts. The model description and validation are lacking. There are also several statements throughout the results section that do not appear to be supported by the figures. For example, the provided plots qualitatively comparing with a buoy do not support statements that the model compares well with the buoy data. Second, the statement on line 296: Figure 8f shows that the net surface heat flux is the dominant term driving the heat tendency with contributions from vertical processes remaining largely the same throughout the time series. It is my impression that several of the comments below can be addressed with a thorough revision of the text.

Reply:

We sincerely thank the Reviewer for your tremendous effort and time in reviewing the manuscript. Your insightful feedback has undoubtedly helped to improve the quality and the scientific rigor of the work. In response to your suggestions, we have restructured the manuscript and removed several conjunctive adverbs in the updated manuscript. Also, the interpretation regarding the influence of the net surface heat flux on the mini-warm pool has been corrected. Following your suggestions, we have addressed each of your comments individually, as listed below. The revised *manuscript incorporates these changes. 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.*

1. *Section 2.2: I find the description of the model components lacking and insufficient. For example: no information is given on the time-step of each component, no information is provided on how the open boundary conditions are specified except for the datasets, the horizontal advection scheme used in ROMS, and other important configuration details. Furthermore, the authors jump back and forth describing the model components which is quite confusing. I'm also curious about the selection of nested domains that are not fully overlapping each other between the components (WRF domain 2 does not fully overlap with the ROMS domain). See the references below for examples on complete model descriptions:*
    a. *Olabarrieta, M., Warner, J. C., Armstrong, B., Zambon, J. B., & He, R. (2012). Ocean–atmosphere dynamics during Hurricane Ida and Nor'Ida: An application of the coupled ocean–atmosphere–wave–sediment transport (COAWST) modeling system. Ocean Modelling, 43, 112-137.*
    b. *Castruccio, F. S., Curchitser, E. N., & Kleypas, J. A. (2013). A model for quantifying oceanic transport and mesoscale variability in the Coral Triangle of the Indonesian/Philippines Archipelago. Journal of Geophysical Research: Oceans, 118(11), 6123-6144.*
    c. *Ross, A. C., Stock, C. A., Adcroft, A., Curchitser, E., Hallberg, R., Harrison, M. J., ... & Simkins, J. (2023). A high-resolution physical-biogeochemical model for marine resource applications in the Northwest Atlantic (MOM6-COBALT-NWA12 v1. 0). Geoscientific Model Development Discussions, 2023, 1-65.*
    d. *Seijo-Ellis, G. G., Giglio, D., Marques, G. M., & Bryan, F. O. (2024). CARIB12: A Regional Community Earth System Model/Modular Ocean Model 6 Configuration of the Caribbean Sea. EGUsphere, 2024, 1-48.*

Reply:

- *We sincerely thank the Reviewer for this valuable comment. As suggested, we have restructured the 'Model Details' section. Initially, we provided a general overview of the model, followed by a detailed discussion of the specific configurations and schemes applied* *in our study. Additionally, we have elaborated on the model setup in coupled mode and described the exchange of variables between the atmospheric and oceanic components. These updates can be found in lines 107–116 and 140–154 of the revised manuscript.*

- *The WRF atmospheric model domain was configured such that the parent WRF domain is larger than the ROMS model domain, as recommended by Warner et al. (2010). For the inner nested domain, we specifically focused on covering the region of the mini warm pool. This inner domain was chosen to encompass a slightly larger area of the mini warm pool to better resolve its processes at finer resolution and to account for inflow and boundary reflections. Therefore, there is no compromise in addressing the hypothesis, regardless of whether the innermost WRF domain fully overlaps the ROMS domain. The nested domain adequately spans the entire Arabian Sea and the southern part of the Indian landmass, aligning with our primary area of interest. Furthermore, we conducted sensitivity experiments to evaluate the impact of domain size. While a larger nested domain was tested, the results within the mini warm pool showed no significant differences. Considering this, and to maintain computational efficiency, we opted for the current domain configuration. This coupled model setup is both effective and optimal for our study objectives.*

2) *Table 1 and sensitivity experiments. Are the open boundary conditions for the first time step also replaced along with the initial conditions? If not, wouldn't this generate noticeable discrepancies and noise near the boundaries?*

Reply:

We agree with the Reviewer that if the boundary conditions are not replaced, initial noise at the boundary could travel inside the domain. For this reason, we have also changed the boundary conditions along with the initial condition in the sensitivity experiments. However, we missed to add this information in the previous manuscript. This information is updated in Table 1 in the revised manuscript. Thank you for pointing out this issue.

3) *The authors omit important citations in several places, for example no reference is given to the ERA5 and SODA reanalysis datasets or the ROMS and WRF models. These should be cited in the text, not just the Data availability statement.*
    a. *ERA5: Hersbach, H., and Coauthors, 2020: The ERA5 global reanalysis. Q.J.R. Meteorol. Soc., 146, 1999–2049, https://doi.org/10.1002/qj.3803.*
    b. *SODA: Carton, J.A., G.A. Chepurin, and L. Chen (2018), SODA3: a new ocean climate reanalysis, J. Climate, 31, 6967-6983, https://doi.org/10.1175/JCLI-D-18-0149.1*

Reply:

Thanks for pointing out the important citation that we missed it. Now, we have added these citations in the revised manuscript in the respective places. Please see the lines 138-139 and 144 - 145 in the revised manuscript.

4) *136-137: It would be much clearer and more concise to say: "The first month of each simulation is used for spin-up and not included in the analysis." On that note, is a 1-month spin-up realistically enough for this case? Are the ocean boundaries nudged or sponge layers used to help with initial noise at the ocean boundaries?*

Reply:

In the revised manuscript, we have included this statement in lines 167-168. We used the reanalysis assimilated product of initial and boundary conditions for both ocean and atmospheric models. Therefore, in these conditions, the model was able to quickly reach a steady state, particularly within the upper few hundred meters (0–200 m). The extent of the mini warm pool is confined to the mixed layer depth, which remains within 50 m during its mature phase across all control and sensitivity experiments (refer to the 30.5°C contour in Fig. R1). Thus, a 1-month spin-up is sufficient in the configured numerical model to simulate the mixed layer processes effectively. Additionally, the ROMS model, coupled with WRF, has been shown to perform well for shorter simulation periods. Extending the spin-up time would necessitate initializing the model earlier than April (as the analysis period is May to June), which would not only increase computational costs but also introduce model biases. For these reasons, we have retained the 1-month spin-up period in our study.

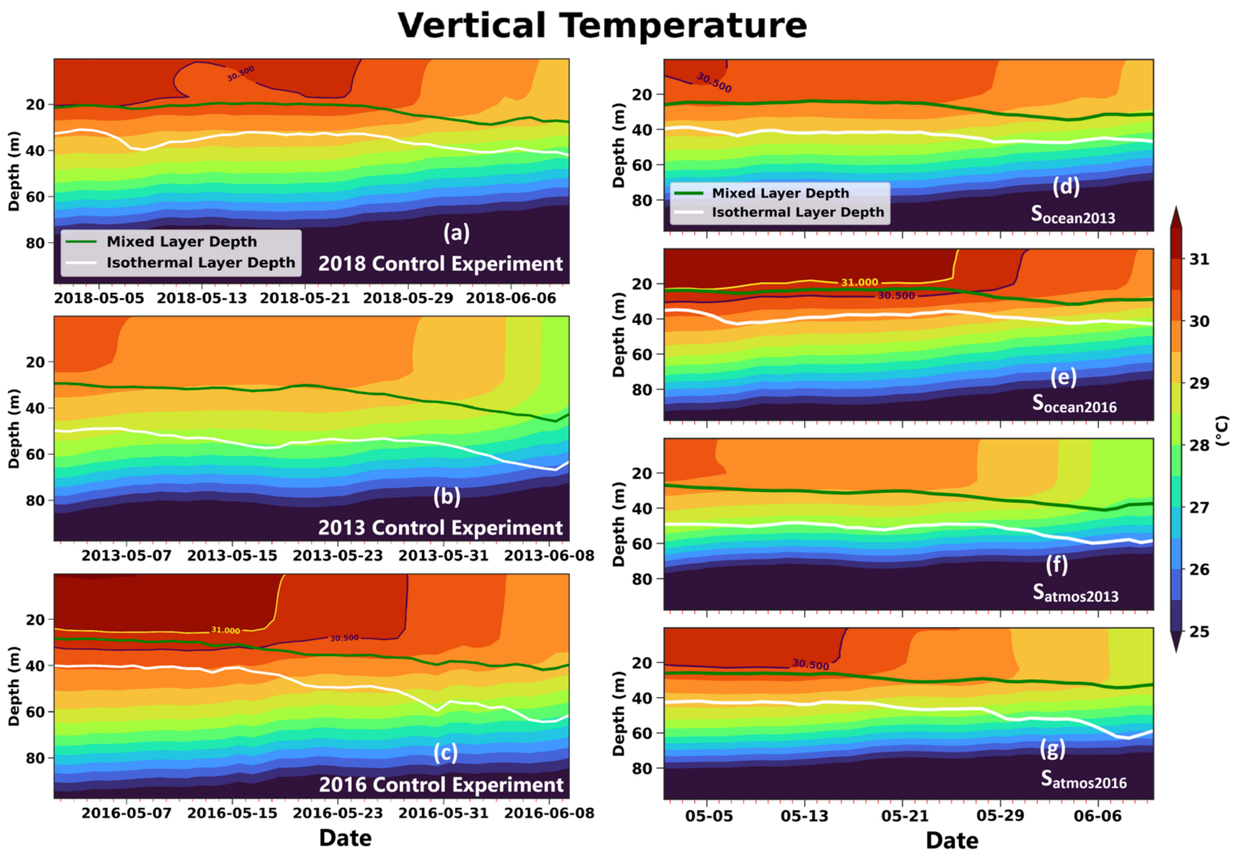

Fig R 1 Area averaged (72- 76ºE and 7-13ºN, i.e., the core MWP region) vertical temperature for three control ((a) 2018 control experiment, (b) 2013 control experiment, and (c) 2016 control experiment) and four sensitivity experiments ((d) $S_{ocean2013}$, (e) $S_{ocean2016}$, (f)$S_{atmos2013}$, and (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.

- *We have not used any sponge layers or nudging at the boundaries. We have used radiation boundary conditions at the open boundary for tracers and momentum. This information is added in lines 150-151.*

5) *Line 139: "We named this set of runs the control experiment (CNTRL)." What set of runs? The ones in Prakash & Pant (2017)? If so, the simulations must be described to some extent here, I have no idea what they did there.*

Reply:

Thank you for pointing out this confusing statement. We didn't refer to Prakash & Pant (2017) for the control experiment, but the variables exchanged between WRF and ROMS. Further, we have removed the citation from here for clarity. This statement is restructured in the revised manuscript. Please see lines 163-169 for clarification.

6) *All datasets used should be described under Section 2. Data and Methodology not in the Results section. That includes the validation datasets and description of any processing done to them for the purposes of the validation comparison.*

Reply:

We sincerely thank the Reviewer for bringing this point to our attention. We have added a new 'Data' sub-section in the 'Data and Methodology' section to discuss the data. Please see lines 85 to 96.

7) *Lines 190-202: Figure 4 results are described before Figure 2 and 3.*

Reply:

Thank you for pointing out this. We have re-arranged the text here and discussed Fig. 2 first and then Fig. 3 and 4. Please see lines 215-229 in the revised manuscript.

8) *Figure 4 should have a differences panel like Figures 2 and 3. This is important because there seems to be biases in the magnitude and spatial extent of some features. Current vectors could also be included in the difference panels similar to Figure 4 in Liu et al. (2015).*

    a *Liu, Y., Lee, S. K., Enfield, D. B., Muhling, B. A., Lamkin, J. T., Muller-Karger, F. E., & Roffer, M. A. (2015). Potential impact of climate change on the Intra-Americas Sea: Part-1. A dynamic downscaling of the CMIP5 model projections. Journal of Marine Systems, 148, 56-69.*

Reply:

We thank the Reviewer for this suggestion. We have updated Figure 4 to closely align with the style of Liu et al. (2015). Additionally, per Reviewer 2's comment, we have focused the plot on a zoomed-in Indian West Coast region rather than displaying the entire domain.

9) *Lines 194-: "The simulated SST effectively captured the cold SST along the Somalia coast across all the examined years, firmly aligning with AVHRR SST data (Fig 2). The SST bias remained within 1°C in all three experiments except in the northern Arabian Sea, where a cold bias patch appeared in the model simulated SST." While the cold tongue is present in the model, the statement as it is currently written is not supported by Figure 2 which shows some of the largest SST biases occur along the Somalian coast (particularly in 2016). Similarly, there are*

biases above 1degC (positive and negative) in other parts of the domain, not just in the Arabian Sea. While these biases are likely acceptable (the authors must convince the reader they are), it is still important to recognize them properly.

Reply:

We thank the Reviewer for pointing out this point.

- *Numerical models have some biases, and they become more evident in the reanalysis data forced regional numerical models. Our configured model is no exception to that. Patches of warm SST bias in the boundary and near the coast in all the years have been observed. However, most of these biases are within 1.5-2^{o}C. Besides, the southeastern Arabian Sea is a very complex region due to the inter-basin transport of water and associated tracers from the Arabian Sea to the Bay of Bengal. This region has a very low bias in temperature and salinity, indicating that our configured numerical model adequately captured the inter-basin transport of water and associated tracers. As our focus is the mini warm pool in the southeastern Arabian Sea, this inter-basin transport becomes more important than the patches of warm and cold biases in the Somalia coast and the northern Arabian Sea.*

- *We also agree with the Reviewer regarding the proper identification of these biases. Hence, the warm SST bias near the Somalia coast has been recognized. Furthermore, a strong cold SST bias in 2016 near the tropical west Indian Ocean is also identified and reported in the revised manuscript. Please look at the lines 215-221 in the revised manuscript.*

10) *Lines 196-198: "This cold bias is attributed to the dry anomalous wind originating from the northwestern region of the South Asian landmass, a pattern also detected in the CMIP models (S. Sandeep & Ajayamohan, 2014)." This doesn't make sense to me. Have you tested this is true in your model? CMIP models are free-running global climate models while you are running a reanalysis forced model. Unless you tested this is true, there is no reason at all to believe the same bias would exist in your forced simulation. Sandeep and Ajayamohan (2014) show that this SST bias results from biases in the representation of large-scale circulation on CMIP models. Those biases would likely not be present in your model and very constrained in the reanalysis forcing.*

Reply:

Thank you for pointing this out. We agree with the Reviewer regarding the difference between the global and reanalysis forced models. Thus, the bias which are in the CMIP6 likely may not be the same in our regional coupled model. Therefore, we have removed this information from the revised manuscript.

11) *Lines 203-206: The figures in the supplemental materials must have a panel with the differences. The current qualitative comparison does not support the statement that the model results aligned well with the Buoy data. This part of the validation is important as the model must represent the temporal and vertical evolution of properties which are important to the processes the authors aim to understand better.*
    (a) *Figure S3 SST: shows noticeable difference in the time extent and magnitude of the warm temperatures and the shallowing of colder waters towards the end of the simulation.*
    (b) *Figure S3 SSS: shows even more noticeable discrepancies between the model and buoy data. The buoy misses a notable high salinity water mass around June 2018. The near-surface distribution of salinity is also quite different.*
    (c) *Similar comments generally apply to Figures S4 and S5.*

Reply:

We have now shown the difference in the temperature and salinity for all three years in S3 to S5. The buoy data has some missing values, which were interpolated in the last version of the manuscript; however, in the revised version, we haven't incorporated such interpolation technique and kept the actual data. In this revised version, the point closest to the AD10 location is used in the numerical model for the comparison, unlike the previous version, where the temperature and salinity of the numerical model are averaged within a 1 X 1^{o} box created around the AD10 location and then compared. Thus, these comparisons are much more precise. In addition, we have revised the text in the manuscript in lines 230-247 to address this validation in detail. Thank you so much for your comments, which improved our results.

12) *The authors do not provide a reference to the buoy data.*

Reply:

We have somehow missed including this information, which have been added in the "Data" subsection as well as in the "Data availability" section. Please see lines 93 to 96 and 486 to 487 in the revised manuscript. Thank you for reminding us of this point.

13) *Lines 206-208: Some additional information would be useful. Is this taking from a single grid point in the model closest to the buoy location? Or is this a horizontal average? If it is a horizontal average, over what region?*

Reply:

We thank the Reviewer for this suggestion. We compared the model data with the nearest 1 X 1^{o} box to the AD10 location in the numerical model vertical temperature and salinity in the previous version of the manuscript; however, in this revised version, the point closest to the AD10 location is used. We have included this information in the revised manuscript. Please see lines 230-232.

14) *Line 138: Be consistent with the terminology, is it dissipation day or phase. One implies several days the other one a specific day. These terms seem to be used interchangeably throughout the manuscript which is confusing.*

Reply:

Thank you for pointing out this error. In the present study, the dissipation day is defined as the day when the SEAS averaged temperature becomes the same as that of its surroundings. Thus, it is a day. On the other hand, the dissipation phase comprises a few days from the mature day to the dissipation day. For this updated information, please see lines 278-281 in the revised manuscript.

15) *The definition for the mature phase in Lines137-138 say "...is characterized by the day when the sea surface temperature (SST) within the MWP core (shown by the white box in Fig. 1) reaches its highest magnitude in May." Why only in May? What if it reaches the highest SST within the MWP on June?*

Reply:

Typically, wind conditions remain weak from late April to early May, accompanied by clear skies during this period. From late May to early June, the Indian Summer Monsoon sets in over the subcontinent. The associated southwesterly winds induce strong upwelling along the west coast of India, leading to surface cooling. Consequently, a strong and intense MWP develops in May and *dissipates as the southwesterly winds strengthen. For reference, the seasonality of SST in the SEAS during strong MWP year is illustrated in Fig. R2.*

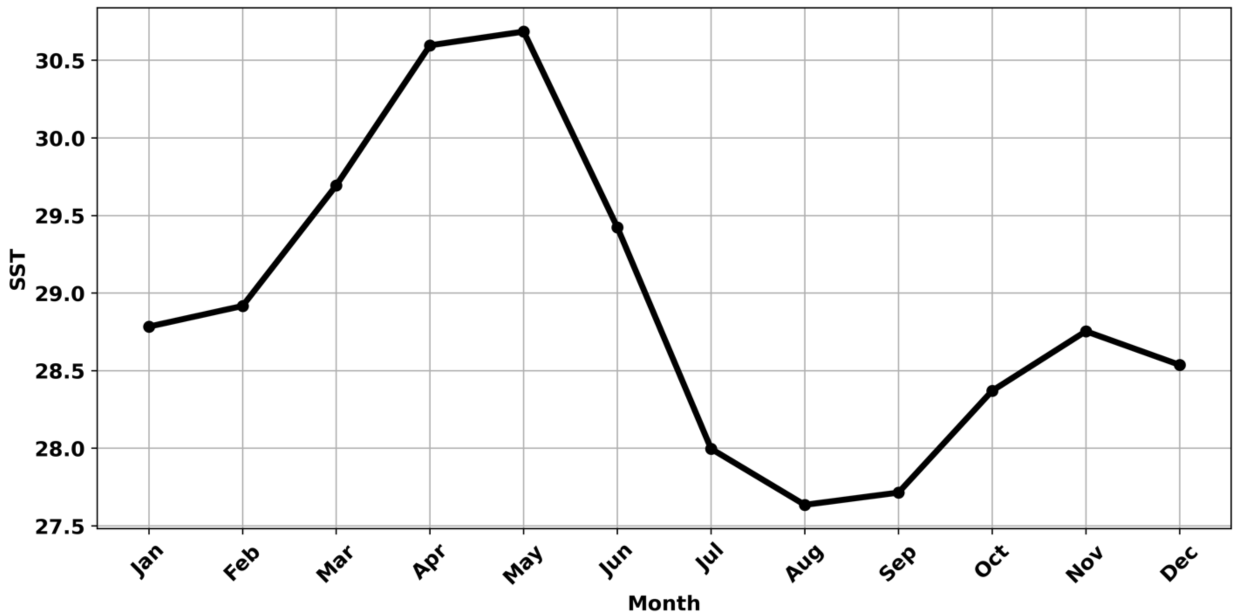

Fig R 2 Seasonality of SST in SEAS (72 - 77ºE & 7-13ºN) averaged over strong MWP years i.e., 1998, 2003, 2005, 2010 and 2016 .

16) *Line 241: the use of "Furthermore" is not correct here.*

Reply:

We are sorry for this error. This sentence is restructured in the revised manuscript. Please see lines 283-284 in the subsection' Ocean surface characteristics during various phases of Arabian Sea Mini Warm Pool'. Apart from this, we have also concise our study by removing unnecessary furthermore, nonetheless, and similar conjunctive adverbs in different places. We thank the Reviewer for pointing this out.

17) *Line 242: "...the wind stress over the SEAS remained less." Less than what? Sentence is incomplete.*

Reply:

In this sentence, we meant that the wind speed over SEAS is comparatively lower than that of the surrounding area. This sentence is reframed in the revised manuscript (see lines 284 – 286 in the updated manuscript).

18) *A table detailing the mature day and dissipation day for each event would help the reader. The table can include the threshold used to define each.*

Reply:

We have added a table detailing the mature and dissipation days for different years. We thank the Reviewer for this valuable suggestion. Please see Table 2 for this information in the revised manuscript.

19) *The authors often omit the year when writing dates which makes the reading more confusing that it already is with the back and forth between the different years.*

Reply:

We sincerely apologize for this confusion. The date and the years are being updated in the respective places in subsection 'Ocean surface characteristics during various phases of Arabian Sea Mini Warm Pool' in lines 277 to 315 in the revised manuscript. However, in the sensitivity experiments, as the atmospheric forcings and oceanic conditions correspond to different years, we mentioned the experiment name rather than the year in the updated manuscript's 'Causative Factors' section.

20) *The domain of interest should be included in ALL figures and panels showing maps like shown in panels c, f and g of Figure 2.*

Reply:

Thanks for the suggestion. We have included the domain of interest in all the figures in the updated manuscript.

21) *Figure 6. Since the description of this figure in the text largely relies on the difference in the conditions between the mature and dissipation phases the authors could consider showing a similar figure with the difference between the time periods (i.e. dissipation – mature). This would be much more informative and relatable to the text in most of Section 3.2.*

Reply:

We agree with the Reviewer that a difference panel will make the interpretation easier for readers. Hence, a panel containing the difference between the dissipation and mature day is included in

Fig. 6, 7, and 8 in the revised main manuscript and Fig. S6, S7, S8, and S9 in the revised supplementary. Thank you for this valuable advice.

22) *Line 255-257: "The signal of the southeastward propagation…" Without the surface currents I can't tell the low salinity signal is propagating southwestward, could as well be that the source of that low salinity is weaker and thus its extent is less.*

Reply:

We concur with the Reviewer regarding the variability of the source of the high-salinity water. Kumar & Prasad (1999) reported that this high salinity water formed in the northern Arabian Sea during winter due to convective mixing. Thoppil et al. (2022) studied and concluded a substantial interannual variability of the source of the high salinity water in the northern Arabian Sea during contrasting monsoon seasons. However, in the aforementioned statement in the manuscript, we meant that this high salinity water is transported southeastward from the mature to dissipation day in each year (Fig. R3). Thus, we are not comparing between the years but between the mature and dissipation days within each year. However, this southeastward transport of the high-salinity water is not the main objective of our study. Hence, to reduce the redundancy, we have removed this statement from the revised manuscript.

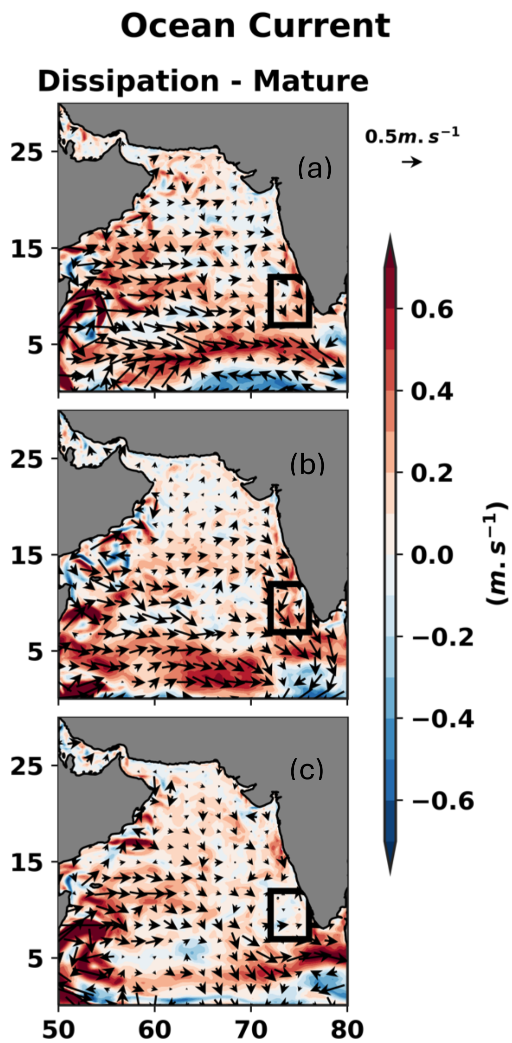

Fig R 3 Anomaly of the current speed along with direction between the dissipation and mature day in (a) 2018, (b) 2013, and (c) 2016. The black box is the MWP core region.

23) *Line 271: "However, these components of the net heat flux could not justify the progress of the MWP in the SEAS." How do the authors reach this conclusion? it seems obvious to me that the latent heat flux is an important contributor to the progression of the MWP: the pattern of the net surface heat flux during the dissipation phase is very similar to the combined patterns of latent and surface heat fluxes. This is not surprising, as wind stress increases it drives evaporation at the surface which acts to cool the ocean surface via release of latent heat. In fact, Figure 8f shows that the heat tendency is largely explained by the net surface heat fluxes.*

Reply:

We strongly agree with the Reviewer on this point. We have misinterpreted the influence of the net surface heat flux on the MWP formation. We have corrected this misinterpretation in the updated manuscript. See lines 358-373 under the subsection 'The Role of the Atmosphere and Ocean in the Formation of MWP' and 438 to 445 and 471 to 478 in the 'Conclusion' section for the corrected interpretation. We apologize for the misinterpretation.

24) *Line 292-294: "In 2018, the net heat flux supplied…" This needs re-writing as it implies this is true for the full time series in Figure 8d which is not.*

Reply:

We thank the Reviewer for the thorough inspection. We have corrected this statement in the revised manuscript (see the line 361-363).

25) *Lines 296-300: While vertical mixing has a negative contribution to the heat tendency (i.e. cooling effect) that is true for the full time series. The vertical mixing curve remains largely flat with no noticeable trend driving the large variations shown for the heat tendency. The statement the authors make is only somewhat true for 2018. In 2013 its clear that contributions from horizontal advection and net surface fluxes drive most of the heat tendency during the dissipation phase. In 2016, the net surface heat fluxes also drive the heat tendency during the dissipation phase with the contribution from vertical mixing largely uniform throughout the time series.*

Reply:

We appreciate the Reviewer's insightful comment regarding the cooling influence of the net surface heat flux. As suggested, we agree that after the MWP matures, increased wind speed leads to enhanced evaporation (latent heat loss) and strong mixing. Additionally, the moisture-rich southwesterly wind reaches the SEAS (after a few days of the MWP mature day) and causes cloudiness, which blocks the incoming shortwave radiation. These factors collectively result in a negative impact of net surface heat flux on the MWP. In response, we have revised the manuscript to include a detailed analysis of the mixed layer heat budget, highlighting this negative influence (see lines 358-373, particularly lines 368–373). Furthermore, we have incorporated this information into the conclusion section (see lines 438-445 and 471-478) for a comprehensive discussion.

26) The panels in Figures 9 and 10 need numbering.

Reply:

We have numbered the panels in Fig. 9 and 10. Thank you for this advice.

27) Line 307: "…the intial pre-April ocean temperature…" Is the author referring to the initial condition mean SST? Please clarify this in the text.

Reply:

The referenced temperature represents the average temperature within the SEAS region (denoted by the white box in Figs. 9 and 10) up to the mixed layer depth (MLD) in the ocean's initial conditions. This temperature is subsequently compared with sensitivity experiments to assess the impact of ocean preconditions. For example, in the 2016 (2013) ocean initial condition, the MLD-averaged SEAS temperature was 0.35°C higher (0.15°C lower) than in 2018. Similarly, when the ocean's initial and boundary conditions were altered to 2016 (2013) in 2018, the MWP became 0.6°C warmer (0.3°C cooler). To enhance clarity, we have revised the relevant statement in lines 329-333 of the updated manuscript. We kindly request the Reviewer to review the changes.

28) Lines 304-315: The description along these lines is extremely confusing and difficult to follow within the context of what the authors aim to describe. I would encourage rewriting. Start by reminding the reader what the first sensitivity experiment was, describe the results of the experiment, then connect with the results of the control simulation and what the results from the sensitivity experiment mean. Then do the same for the second sensitivity experiment.

Reply:

We sincerely thank the Reviewer for this insightful suggestion. In line with the Reviewer's advice, we have simplified the paragraph for better clarity. The revised version can be found in lines 325-342 of the manuscript.

29) Line 314-315 seem to contradict the statements made before about net heat fluxes and vertical mixing in the control experiment, which I already commented about in #24 above.

Reply:

We thank the Reviewer for this comment. Following the suggestion of the Reviewer, we have re-written this whole section, and, in the process, this contradictory information is removed from the revised manuscript.

30) *Figure 11: each panel should identify to which experiment is corresponds to. I shouldn't need to read the caption to find this information.*

Reply:

We have included the information of the experiment in each panel. We have updated this not only in Fig. 11 but also in Fig. S10 in the supplementary. Thank you so much for your suggestion.

31) *It would be more useful and informative if Figures 11 and 8 were combined.*

Reply:

We have merged Figures 11 and 8, as well as Figures 8a–8c and Figure S7 from the supplementary materials. The latter has been relocated to the supplementary section, as Figure S10 in the revised version. The merging of Figures 11 and 8 (now Figure 11 in the revised manuscript) allowed us to rewrite this section, combining the results of the control and sensitivity experiments. We appreciate the Reviewer's suggestion, as it has enhanced the fluency and coherence of the manuscript. Please refer to lines 358–373 for the updated information

32) *Line 333-334: "Moreover, the atmosphere was the primary driver of the vertical processes within the mixed layer that lead to the dissipation of the MWP." I'm not sure what the author means by this. How is the atmosphere the primary driver of vertical mixing processes in the ocean? Furthermore, the net surface heat fluxes are the main contributor to the tendency 11d, with combined contributions from all terms in Figure 11b. Furthermore, as detailed before, the contribution by vertical processes remains mostly the same throughout the time series, so its hard to understand in Fig.11 b and d how vertical processes are the drive the dissipation phase.*

Reply:

We can understand the Reviewer's concern regarding this statement. We meant here that the vertical processes continuously have a detrimental effect on the temperature tendency of the MWP and could facilitate its dissipation. However, the Reviewer fairly pointed out that the net surface heat flux has the dominant influence on the MWP temperature tendency, which we also agree with. Once the wind speed in the southeastern Arabian Sea intensifies, it causes latent heat loss from the surface along with a decrease of incoming shortwave radiation due to the cloudy sky associated with the arrival of the moisture-rich southwesterly wind. This results in a reduction of the net *surface heat flux. Besides, the wind along the southwest coast of India is favorable for upwelling, and once the wind speed increases, so does the upwelling (Rao et al., 2008; Shah et al., 2015). Thus, the atmosphere, especially wind, drives SEAS's vertical processes. Nevertheless, the net surface heat flux is the primary driver behind the variation of the temperature tendency from the mature to the dissipation day in the southeastern Arabian Sea. We have rewritten lines 368-373 in the revised manuscript to add this information. Thank you for your in-depth review.*

33) *Lines 336-338 seem to me repetitions of Lines 330=331?*

Reply:

We thank the Reviewer for the in-depth inspection. In the revised manuscript, we have rewritten the whole section (line 368-372 in the revised manuscript) to add comments 31 and 32. As a result, these lines have been removed.

34) *Figure 12-15. Experiment must be identified in the figure in addition to the caption.*

Reply:

We thank the Reviewer for this insightful suggestion. In the SEAS, the production of turbulent kinetic energy (P_{TKE}) driven by wind has a significantly greater impact compared to P_{TKE} generated by thermal and buoyancy fluxes (Fig. 13). Accordingly, we have retained only the wind induced P_{TKE} in the revised main manuscript (Fig. 14) while the P_{TKE} caused by haline and thermal buoyancy fluxes has been moved to the supplementary material (Figs. S11 and S12). Experiment details are now included in all these figures, and panels have been appropriately numbered. Additionally, the MWP core region is marked with a black box in these figures for clarity.

35) *Lines 373-374: shadow seem like an odd way to describe an area of high Ptke. Keep it simple and call it what it is.*

Reply:

We thank the Reviewer for this query. The MWP expanded in the southeastern Arabian Sea in a region where the P_{TKE} induced by wind is substantially low, which we referred to as the "wind shadow zone." It is important to note that we did not label the region with high P_{TKE} as the shadow zone. Please refer to lines 408-419 in the revised manuscript for the updated information.

36) *Figures 13-15 are all discussed in a single paragraph. I would recommend combining (by eliminating unnecessary panels). The authors do not describe and/or comment on the panels associated with the thermal buoyancy flux or haline buyoyancy flux driven turbulent energy.*

Reply:

Thank you for pointing this out. The P_{TKE} caused by haline, or thermal buoyancy flux, has less magnitude than the P_{TKE} caused by wind (Fig. 13 in the revised manuscript). Due to this reason, we have focused on the P_{TKE} caused by wind. Thus, following the Reviewer's advice, the P_{TKE} caused by the wind for all the experiments is combined and kept in the revised main manuscript (Fig. 14), and the P_{TKE} caused by haline, and thermal buoyancy flux is moved to the supplementary (Fig. S11 and S12 in supplementary).

37) *Line 409: "...revealed that net heat flux is the primary driver of the MWP development.." this statement (which I agree with) is not consistent with statements made in the results section (see comment 24 and others). Specifically line 271.*

Reply:

We thank the Reviewer for pointing this out. The interpretation related to the influence of the net surface heat flux on the MWP temperature tendency was not appropriate in the previous version of the manuscript. We have updated this information in the revised manuscript in the respective places. We request the Reviewer to please have a look at lines 358-373 under the subsection 'The Role of the Atmosphere and Ocean in the Formation of MWP' and 438 to 445 and 471 to 478 in the 'Conclusion' section for the corrected interpretation. We apologize for the misinterpretation.

38) *Lines 411-413: "However, net heat flux alone did not fully account…" You mixed layer budget (Figure 8) does not support the statement that after the mature phase, vertical mixing in the ocean leads to a rapid dissipation of the MWP, except for 2018 to some extent.*

Reply:

We apologize for the misinterpretation. The answer to this comment is similar to the previous one. We have misinterpreted the influence of the net surface heat flux and the vertical processes on the MWP development in the earlier version of the manuscript. However, we have updated this information in the revised manuscript in the respective places. We request the Reviewer to please have a look at lines 358-373 under the subsection 'The Role of the Atmosphere and Ocean in the

Formation of MWP' and 438 to 445 and 471 to 478 in the 'Conclusion' section for the corrected interpretation. We apologize for the misinterpretation.

39) *"Net heat flux" should always be "net surface heat flux".*

Reply:

We thank the Reviewer for pointing this out. We have replaced the Net heat flux with the net surface heat flux in the respective places.

40) *Line 414: "…significantly impacted…" what defines a significant impact in this case? There is no statistical analysis done to determine significance. Furthermore, one resulted in an increase and the other in a decrease. This is rather inconclusive.*

Reply:

- *We sincerely thank the Reviewer for this suggestion. As we have not done any statistical test here, we have replaced the 'significantly impacted' by 'substantially impacted' in the revised manuscript (see lines 435-438) in the 'Conclusion' section.*
- *In the $S_{ocean2013}$ and $S_{ocean2016}$ experiments, the ocean initial and boundary conditions in the 2018 control experiment are replaced by the 2013 and 2016 ocean initial and boundary conditions, respectively. In 2013, the MWP was weak, and by replacing the ocean initial and boundary condition in the 2018 control experiment with the 2013 ($S_{ocean2013}$ sensitivity experiment), we observed a weaker MWP than in the 2018 control experiment. Similarly, in 2016, the MWP was intense, and by substituting the ocean initial and boundary condition in the 2018 control experiment to 2016 ($S_{ocean2016}$ sensitivity experiment), we observed a stronger MWP than in the 2018 control experiment. The strength of the MWP changes following the ocean precondition, and thus, we have concluded that the ocean precondition plays a dominant role in the formation of the MWP. We hope this clears the confusion regarding the influence of the ocean's initial condition on the MWP formation.*

41) *Lines 423-424: "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." This needs further elaboration and comments by the authors. What are the differences between this study and the Kurian 2007 study that may lead to these different results?*

Reply:

We thank the Reviewer for this advice. Following the comment from the other Reviewer, we have removed this statement from the main manuscript.

42) *Figure 16 is potentially a nice way to wrap-up the manuscript. But the author make no effort to summarize and describe the formation and dissipation mechanisms of the MWP as shown in the figure. Describe in 1-2 simple sentences the processes as shown in Figure 16.*

Reply:

Following the Reviewer's suggestion, we have provided a detailed description of the MWP genesis as illustrated in the schematic. This explanation can be found in lines 471-476 of the revised manuscript. We sincerely thank the Reviewer for this insightful suggestion.

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