

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 EGU sphere.

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.

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.

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.

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”.

Wording changes

Some improvement in wording/language-style is required. I list below some examples, but please read to check for more.

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

Line 152. Plural “models”

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

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

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

Line 195. Delete “represented in the”

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.

Line 235 “anticipated” -> “simulated”

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

Line 330 “till” - > “to”

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

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

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

Fig. 13 use same vertical axis scale for all panels.

Caption and text. Move “multiplying 10^4 ” to the vertical axis label.

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

Lines 446-449 have confusing wording. Please rewrite.

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

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

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

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