

Interactive comment on “Mixed layer depth variability in the Red Sea” by Cheriye P. Abdulla et al.

Anonymous Referee #1

Received and published: 29 March 2018

General Comments: The authors have used historical temperature profiles from the Red Sea to develop a monthly climatology of mixed layer depth (MLD) along the seas' central axis, and investigate the importance of wind stress, thermal buoyancy forcing and haline buoyancy forcing in controlling MLD in the northern, central and southern Red Sea. The authors also investigate the relationship between MLD and the presence of cyclonic and anticyclonic mesoscale eddies, as well as the impact of the cross-axis Tokar Gap wind jet on MLD in summer. To my knowledge, this is the first published climatology of MLD in the Red Sea, which will be useful for verifying numerical model simulations of the basin and biogeochemical studies. The analysis of the impact of atmospheric forcing mechanisms as a function of latitude is interesting and worth publishing, in my view. The descriptions of the impacts of mesoscale eddies and the Tokar

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Gap wind jet are less clear and I recommend major revisions in these sections to make them truly convincing.

Specific Comments:

Line 30 – Much of this is elementary physical oceanography, belonging in a textbook, not a scientific paper. I suggest to condense this part of the text significantly.

Line 43 – Bower and Farrar (2015) have direct estimates of evaporation rates that should be mentioned and referenced here.

Line 148 and following – 85 m +/- what? Need to add standard deviations to these mean values, in this line and all the following instances of reporting mean values. This is essential to understanding the statistical significance of the mean values. I realize some statistics are included in the supplementary material, but they should be included in the main document. Similar for lines 152–153, line 175 and line 197 (plotted lines need error bars or similar).

Line 214 – It wasn't obvious to this reviewer how the authors chose the latitudes where there were supposedly reductions in correlation between MLD and all forcing mechanisms. For some of the gray bars, the coincidence of lower correlations is obvious, but not in all. Would be good to define more clearly how these latitudes were chosen, hopefully using some objective criteria.

Line 243 – References are needed here to validate the authors' description of the relationship between mesoscale eddies and MLD.

Line 267 - The authors are implicitly arguing that the upwelling and downwelling associated with the secondary circulation of cyclonic and anticyclonic eddies is more important in determining MLD in the eddies than direct wind forcing and buoyancy forcing. Is there any literature to support this? I'm guessing there is, and the authors need to add some references here to this point.

Line 322 - It is not clear to me here if the region to the south of the jet axis is well-mixed

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because of wind-induced turbulent mixing, or because of the secondary circulation associated with the wind stress curl-induced formation of the anticyclonic eddy, or both. the authors need to clarify this, or, if it is ambiguous, say that they are not sure which mechanism dominates.

Line 326 - If this is a summary sentence, I suggest to start it with “In summary. . .” I’m left with uncertainty about the authors’ claim regarding the role of the TG jet in increasing MLD. As questioned above, does the upwelling and downwelling associated with the eddies overwhelm the direct mixing impact of the wind jet? Presumably the direct impact of the winds would be felt on both sides of the wind jet axis, but it’s not clear if the authors are making this point for the cyclonic as well as anticyclonic eddy. Clarification needed here.

Line 346 – It was not clear to me if the deeper MLD was due to the direct impact of the winds or the formation of the anticyclonic eddy. Needs clarification.

Line 350 - I think this is the best result of the paper.

Line 357 - As remarked on above, why would the winds enhance ML development south of the wind axis but not north? Maybe the deepening to the south is due mostly/only to the formation of the anticyclonic eddy?

Technical comments:

There are numerous English grammatical and syntax errors in the writing. I’ve listed some, but not all, below.

Line 20 – Should read “The surface mixed layer . . .” (i.e., add “the”)

Line 30 – Should read “Oceanic heat loss. . .” (i.e., delete “The”)

Line 30 – “to strong mixing. . .” “Strong”? Compared to what?

Line 35 – “through from. . . .” Extra word here.

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Line 39 – “The Red Sea is a typical. . . .” How is it typical?

Line 45 – “regions in the world. . . .” Referring to the water in the Red Sea? Maybe use “ocean basin” instead of “region.”

Line 48 – What about Yao et al. references? Shouldn’t they be included here? They represent some of the most comprehensive modeling studies of the Red Sea to date (after Sofianos’ papers).

Line 49 – The increase in the number of temperature. . . .” (add “the number of”)

Line 55 – the authors should consider adding reference to Bower and Farrar (2015) paper and Yao et al. papers.

Line 77 – Over what depth range are inversions flagged?

Line 89 – What does “spread” mean? I think the word to be used is “distribution.”

Line 110 – “Traon” Check spelling. I think it’s “LaTraon”.

Line 118 – What is meant by short-range disturbances? A sentence or two more on how the method works will save the reader from having to look it up elsewhere.

Line 121 – Would be helpful to the reader to give an example. How exactly does this work?

Line 149 – Are these numbers from individual profiles? Please clarify.

Line 158 – This sentence is confusing. What is meant by the “other regions”?

Line 168 – I would say April to June is more like the monsoon transition (probably low winds), not summer.

Line 174 – “net heat loss” (loss not lose)

Line 180–181 – Rather than “enhance mixing,” which should be “enhances mixing” to be grammatically correct, I would suggest saying “supports vertical mixing through

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buoyancy loss” or something similar.

Line 181 – “slightly diminishes mixing. . .” And here I would say “opposes vertical mixing due to buoyancy gain.”

Line 184 – Would be helpful to define acronyms in figure caption.

Line 190 – “support vertical mixing” (add “vertical”)

Line 192 – Shouldn’t it be “net buoyancy flux” ?

Line 194 – Isn’t there a Sofianos paper to be referenced here too?

Figures 3 and 4 – It would be helpful to add a zero line on Figs. 3 and 4.

Line 200 – I suggest that the authors mention the wind direction as well as stress amplitude variations through the seasons. Also, shouldn’t wind stress in the winter be negative? All wind stress values are presented as positive. This is okay since it is only the magnitude (not direction) that impacts vertical mixing, but the authors need to say they are showing absolute value only.

Line 206 – I’m not sure what the authors mean here by “phase.” I think they are referring to negative and positive correlation; e.g., MLD and NHF are negatively correlated since as NHF (into the ocean) increases, MLD decreases.

Line 229 - How were eddies identified? If some eddies or sub-gyres are semi-permanent, how do you decide when one ‘dies’ and a new one is formed? If the histogram is from another paper, it should be referenced here.

Line 258 - I think ‘curve’ should be ‘curves,’ because the point is (I think) that at these latitudes, correlations between MLD and all the forcing factors (wind, thermal buoyancy, haline buoyancy) are reduced.

Line 260 - Zhai and Bower 2013 should be added to this list, and Bower and Farrar 2015.

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Line 289 – Authors should indicate data source in figure caption.

Line 291 – What is the data source for the T S profiles?

Line 293 - This year (2001) was also highlighted and described by Zhai and Bower 2013, which should be referenced here.

Line 333 - “slightly lower” than what? Lower than some individual measurements? If that is what is meant, that is obvious and this phrase should be deleted, or replaced with the actual extreme values.

Line 334 - Rather than ‘general picture’, authors should say something more concrete like ‘climatological mean.”

Line 340 - I would say that shallow MLD and increased stratification are the same thing. Authors could consider ‘associated with increased short-wave radiation’ instead.

Line 343 - Suggest to add “...is not linear with increasing latitude.”

Line 345 - This phrase is confusing. Suggest to say “deeper MLD than typical of elsewhere in the Red Sea” or something similar.

REVIEWER COMMENTS – SUPPORTING INFORMATION

Figure S1 – I think “distribution” is a better word than “spread.”

Table S1 – I don’t think it’s necessary to include this table. It was sufficient to describe the end result of the QC in the manuscript.

Table S2 – Could this information be summarized more efficiently with a plot of some kind?

Table S3 – Similar comment for this table. Change to a plot?

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-6>, 2018.

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