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Interactive comment on "Fast local warming of sea-surface is the main factor of recent deoxygenation in the Arabian Sea" *by* Zouhair Lachkar et al.

Anonymous Referee #2

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General comments

The Arabian Sea (AS) Oxygen Minimum Zone (OMZ) has profound consequences on the ecosystem and climate, making it important to understand the evolution of oxygen in the AS. The present study offers to investigate the mechanisms driving the oxygen evolution in the AS using a set of sensitivity experiments performed with an eddy-resolving model. They conclude that the deoxygenation in the northern AS is primarily caused by the reduced ventilation associated with the recent fast warming, particularly in the Gulf, while the summer monsoon winds intensification caused oxygenation in the rest of the AS.

While agreeing that the topic fits well within the scope of the journal, there are several lacunae both in terms of analyses and presentation that do not allow me to recommend the manuscript for acceptance in its present form. Below are my detailed review comments on the manuscript.

Specific comments

1. The title is somewhat overstated/misleading: "fast" warming is still debated in the observations (see for e.g. Gopika et al., 2020); the "deoxygenation" occurs mainly in the northern AS (as their results suggest), unlike what is stated in the title. 2. What is the focus region – AS/northern AS/AS OMZ? The entire paper, including the abstract, switches its discussion between those regions, making it difficult for the reader to comprehend. 3. The importance of Gulf warming for the AS OMZ has already been addressed by the authors in Lachkar et al., GRL, 2019, which is partly the focus of the current manuscript. The authors need to clarify or discuss this in detail. 4. Schmidtko et al. (2017) did not specifically discuss a decline in oxygen in AS nor on the west coast of India. However, this paper is explicitly referenced in the introduction for claiming deoxygenation in the AS. Similarly, the study by do Rośorio Gomes et al. (2014) is largely debated. On the other hand, many relevant references, for e.g. Sandeep and Ajayamohan, 2015, are not cited. 5. The manuscript does not provide important details. For e.g., how the trends are computed? What is O2sat./AOU and how are they estimated? No details provided on O2 budget, ventilation and biological consumption terms, in the methods section. 6. The manuscript is badly written, with careless handling in many places. Almost every paragraph contains sentences which are not easy to follow. I will mention some of them below. There are many typos too. For e.g. E or W in longitudes (P4 L25 and other places). Figures in supplementary do not have longitude labels, figures with vectors have no reference scale vector, and captions do not provide complete details (i.e. the region for average, Fig 6: what is the reference for % computation, etc.). I strongly suggest the authors to improve the presentation, including the abstract. 7. Avoid methodological details in the results. For e.g. P9 L20:

the first two sentences of section 3.1 belongs to methods section. P14 L20: details of experiments should be moved to methods. 8. P4 L22: "major rivers in the northern Indian Ocean". Provide details as to which rivers are considered, particularly in the Arabian Sea. 9. The authors provide validation of their model at the surface level and at the seasonal scale. The model shows a good performance as the fields are restored at the surface. However, as the paper focusses on the subsurface level, it is necessary to validate the model at the subsurface, for e.g. with Argo observations, both in terms of vertical profiles/sections. It is also important to demonstrate that there is no drift in the model at the deeper levels (e.g. ventilation). 10. P9 L28: How do you ascertain that 14% increase in denitrification is due to 10% decline in oxygen? Are these anomalies correlated? 11. The manuscript discusses many processes and the link/flow is somehow missing. I suggest to first provide a discussion of possible causes of oxygen variations and then address each of them. Section 3.2 P10. It may not be clear to the reader the link between AOU and ventilation. The authors should provide substantial details of these parameters in the methods. 12. P10 L5: O2 decline vs O2 saturation. How do you ascertain their link? What about AOU at this depth? 13. P11 Fig. 6: Panel (c) shows O2 section at 65E. Different color scales make it difficult to compare. There seems to be some inconsistency in the deeper levels (below 200 m) when compared to panel (b), especially between 18 and 20N. Moreover, there appears to some discontinuity in the vertical layers (jumps in color shades) in panel c. Does it arise from some mistake from sigma to depth level conversion? Lastly, provide the details of how the % change is calculated in the caption. 14. P12 Fig. 7: What is the region (lat, lon, depth) over which the volume calculation is performed? The O2 trends are different in northern and southern AS, if you considered the entire AS. This figure suggests decadal signals that may alias the linear trend computation. Are these trend estimates hold good even after removing decadal signals? 15. P13 Fig. 8: How is O2sat computed? No mention of region in the caption. As you discuss AOU in the text, why is that AOU not shown on these panels. 16. P14 L12. Inconsistency between figures 9 and S9. Fig S9 presents the vertical and horizontal components of ventilation

term shown in blue in Fig 9. It is hard to see that they add up to the total ventilation, including their scales. 17. P14 Section 3.3: The details of sensitivity experiments may be shifted to a new section under methods. 18. How sensitive are the results to a different forcing set other than the ERA-Interim? How well the oxygen trends are constrained when using different forcing field? Are the decadal signals (say in winds) consistent among different observations? 19. P14: The sensitivity experiments are conducted by repeating the cycles of 1986 to remove interannual variations and is considered to vary climatologically. I am wondering why 1986 repeated cycles are used for forcing or why this particular choice is made. A cleaner approach would be to compute the climatology (of heat fluxes in AS/Gulf or winds) and use it to force the model. 20. I have a major concern regarding the sensitivity experiments. First of all, the authors do not provide much details on the region over which the fields are allowed to vary climatologically (1986 repeated cycles). Moreover, there is an underlying assumption here. How do you ensure that warming results primarily from the heat fluxes. For e.g., by suppressing the heat flux variations in the Gulf, are you able to remove the Gulf warming? Did you check whether the AS heat fluxes affect the Gulf warming? Similarly, does the AS warming results mainly from the heat fluxes over the AS? In the absence of a figure showing warming trends (somewhat like Figure 10, or time series averaged over Gulf/AS for sensitivity experiments), it is not justified to presume that the experiments indeed isolate the respective processes. 21. P15 Figure 9: There appears to be decadal signals, which may potentially alter the trend estimates. No mention of the region in the caption. Panel numbering has gone wrong. The terms have to be described in the methods. 22. P15 L6: "summer upwelling intensification . . ." How do you conclude that there is an upwelling intensification as there is no figure to support this claim? And how upwelling region contributes to the rest of the basin? These claims are mere speculations without any justification. 23. Fig 11 vs S11. The authors show the maps from the sensitivity experiments, but have chosen to move the maps that demonstrate the effect of each process to supplementary figures. For instance, to know the effect of heat fluxes, one has to refer to the supplementary figure (say S11). I would choose the other way, as it is difficult to follow the discussion in the text. I guess, the text can be considerably improved if the authors stick to discuss only the effect of processes, instead of describing the results of sensitivity experiments. 24. P16 Fig. 10. There is no reference vector. I find some inconsistency between background color shade (speed) and the vectors. Negative shades on the west coast of India and the direction of overlaid winds do not agree. Further, the low magnitude vectors south of equator & to the east of 65E correspond to high wind speeds (red shade). I expect the trends in wind stresses and wind speeds are not entirely different. 25. P17 Figure 11: Panels g & h are not consistent with the rest of the panels, when visually averaged over the northern AS. As emphasized before, I would suggest to interchange Fig. 11 with S11 to ease the discussion in the text. 26. P19 L9-10: "... potentially impacting O2 supply to northern AS". How? 27. P19 L15-16: "the shoaling of thermocline ... top 200 m". Not really justified. How well thermocline anomalies are correlated with O2 anomalies? 28. P19 L29-30: The authors point to an increased summer upwelling on the western AS. However, there is no clear evidence for its contribution to northern AS. On the other hand, the northern AS productivity is mainly due to convective mixing during winter. The influence of winter monsoon has not been discussed in the manuscript. The winter monsoonal winds directly contribute to the ventilation as well as through productivity changes to the oxygen in the northern AS. 29. P19 L33-34: The negative feedback of denitrification is merely speculated and not really demonstrated. Thus, its effect is overstated. 30. Figure S10: It is not convincing that there are no strong SST trends in the upwelling region of the western AS during JJAS. 31. Figure S11: The 4% increase marked in panel a is for entire AS, not for northern AS as mentioned in the text (Section 3.4; P16 L6). Stratification shows a clear decadal signal.

Technical corrections (not all)

1. P19 L2-4: "This suggests ... at play". Not obvious to the reader. The entire first paragraph in this page is not readable. 2. Many panels and many of the supplementary figures are not properly referenced in the manuscript. 3. Figures S7 & S8: Not easy

for me to understand. No color bar. Repeating statements in caption.

References Gopika, S., Izumo, T., Vialard, J. et al. Aliasing of the Indian Ocean externally-forced warming spatial pattern by internal climate variability. Clim Dyn 54, 1093–1111 (2020). https://doi.org/10.1007/s00382-019-05049-9

Sandeep, S., Ajayamohan, R.S. Poleward shift in Indian summer monsoon low level jetstream under global warming. Clim Dyn 45, 337–351 (2015). https://doi.org/10.1007/s00382-014-2261-y

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-325/bg-2020-325-RC2-supplement.pdf

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