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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 #1

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Given their ecological and socio-economical impacts, it is a must to better describe and understand the present and future evolution of the major Oxygen Minimum Zones (OMZ). Despite large uncertainties related to their sparse and inhomogeneous coverage, in-situ observations suggest that suboxic conditions in the Arabian Sea (AS) may have expanded during the past decades. The present study uses a high-resolution ocean regional model forced with an atmospheric reanalysis to assess the mechanisms responsible for the oxygen decline over the AS. Their results point towards a major influence of reduced ventilation through enhanced upper ocean stratification on the northern AS desoxygenation, further strengthen by summer monsoon wind changes. While previous studies recognised that these two physical processes (along with bio-

logical ones) may contribute to long-term oxygen evolution in the AS, this study offers for the first time a reliable quantification of the respective influence of each of these processes over the historical period. I hence believe that this is a very interesting and important study that deserves to be published in Biogeosciences. However, there are a number of major issues listed below that need to be addressed before publication.

Major comments: A. Validation: Most of the validation section evaluates the model ability to simulate the seasonal cycle in the Arabian Sea. Because this paper mainly focuses on the oxygen long-term evolution, I would recommend the authors to (1) reduce the validation of the seasonal aspects and (2) expand the validation of the long-term trends as follows: (1) Figure 1 and 2: I would move these Figures in the SI. Given that the authors use a surface restoring the observed salinity and temperature, this is not surprizing that the seasonal cycle of the model SSS and SST agree well with observations. In addition, I feel that Figure 2 is not central to the present study and should not be included in the core of the paper. (2) Figure 3: Keep at least the lower panels (Chl validation) but improve the color scale chosen to emphasize the regional contrasts. (3) Figure 4: Add meridional sections of O2 concentration along with hypoxic/suboxic volume to evaluate the O2 structure at depth in both model and observations. (4) Evaluation of long-term evolution (extra analyses): Given that the reliability of the results presented here relies on the strong assumption that the model reproduces accurately the long-term evolution in the AS, the model ability to simulate this evolution should definitely be assessed in greater details. For instance, this paper should include an evaluation of the long-term evolution of AS maps and yearly time series in the northern/western AS for model and observations of (1) Chl over the common period (1997-2010) where observations and model data are available (using OC-CCI product for instance), (2) SST over the entire period (using ERA5 or HadISST for instance), (3) static stability index (as in Roxy et al. 2016) over the entire period (using an ocean reanalysis such as SODA or ORAS4/5) and (4) wind trends from several .wind products. Finally, I would also like to see an attempt to evaluate the model O2 evolution over wide boxes with in-situ observations (from Ito or Schmidtko et al. 2017 for instance).

This would allow strengthening the reliability of the model results regarding the oxygen decline in the AS and the related mechanisms discussed in the paper. B. Choice of the sensitivity experiments: It is not obvious to be why the authors decided only to investigate the role of the summer monsoon wind changes on the O2 concentration. Looking at Figure S10, it appears to me that winter monsoon wind changes are also significant, with a strong wind decrease in the northern AS. This decrease is expected to reduce convective mixing and hence ventilation. I would recommend the authors to perform an additional experiment to evaluate this impact and report it in the manuscript. C. Summary of the main results: I also believe that the authors should definitely include a schematic summarizing the main processes responsible for the long-term O2 evolution in the AS. This would really be helpful for the reader to grasp the salient results of the present study.

Detailed comments: Abstract: P1 L2-3: Given the strong observational uncertainties, I would recommend the authors to lower down their claim about a decline in AS O2 over the past decades. Use for instance "suggest" instead of "show". More generally, I would recommend the authors to refer to the latest SROCC report on the ocean changes (Bindoff et al. 2019) to refine their statements on O2 trends in observations. This report indeed concludes that: "... the challenges of data sparsity, regional differences and the relatively large uncertainties on the oxygen changes across different studies, but also recognising that oxygen declines are significantly different to zero, leads to medium confidence in the observed oxygen decline. Âż In addition, this report does not mention the AS as one of the regions where the O2 decline is the strongest and best observed (they rather mention Southern Ocean, equatorial regions, North Pacific and South Atlantic). P1 L5: "while summer monsoon winds have intensified" : While this is true in the atmospheric reanalysis used in the present study, this intensification of summer monsoon winds may not be evident in all wind products. Indeed, several studies suggest that decadal wind variations are not well constrained in reanalyses, which may lead to strong uncertainties in long-term wind trends over the AS. In addition, most studies rather report a decrease of the Indian summer monsoon circu-

lation over the historical period (e.g. Swapna et al. 2017). This uncertainty should be discussed in the manuscript. P1 L5 : "reconstruct" I don't like the use of this term here, as their is not assimilation in the model. I would rather use "simulate". P1 L7 : Replace Âń observation-based Âż by Âń forced by an atmospheric reanalysis Âż P1 L12 : Âń in particular in the Gulf Âż : The gulf warming contributes to 1/3 of the O2 decline through stratification if I get it well from the bottom panels of Figure 11. I would rather use Âń including in the Gulf Âż for not over-emphasizing the role of the Gulf warming.

Introduction : P2 L2-9: I would recommend the authors to also refer here to the latest SROCC report on the ocean changes (Bindoff et al. 2019) to refine their statements on the model projections and related uncertainties. P2 L13: "hypoxic events" Do you mean "anoxic events"? P2 L16-35: Please also refer to SROCC 2019 report about the confidence we have these reported changes. P3 L8: Regarding the enhanced warming in the AS, you should refer to Gopika et al. (2020) that investigate this in details. P3 L12-14: Results from Roxy et al. (2016) which report a Chl decline over the western AS appears to be inconsistent with results from the present study. This inconsistency and its implication on the robustness of the authors conclusions should be discussed in Section 4.2. P3 L17-18: I would recommend the authors to remove the reference to Goes et al. (2005) as the period covered by this study is very short (and strongly aliased by the 1997 El Niño) and their analysis very local. Most studies rather report a decrease of the Indian summer monsoon circulation over the historical period (e.g. Swapna et al. 2017) as well as in the future (e.g. Sooraj et al. 2015). Our current knowledge on the summer monsoon long-term trends should be better summarized here. P3 L23: Remove "largely" P3 L29: Change ", in particular in the Gulf" into ", with a significant contribution from the Gulf warming"

Methods: P4 L17: I am wondering why the authors did not extend their simulation until present days as ERA-I forcing is available until 2019. This would have allowed the authors to extend their validation over a period with more observations and monitor natural and anthropogenic decadal variations in more details. I would like the authors to clearly state why they did restrict their simulation to 2010 only. P6 L7-P8 L5: Given the restoring to observed SST and SSS, the good agreement between observations and model is far from surprizing here. A dedicated Figure evaluating the surface circulation is not mandatory either given the scope of the paper. I would move these Figures in the SI to allow more space to validate the long-term evolution of key variables in the model. P8 L14-15: I would add two meridional sections to Figure 4 showing the ability of the model to simulate the oxygen vertical structure in the AS. P9 L5-12: While this paper discusses the mechanisms responsible for the long-term oxygen changes in the AS, this small paragraph is the only place where the authors evaluate the ability of their model to simulate the AS long-term changes. I would recommend the authors to reduce their seasonal validation and expand the validation of the simulated long-term evolution in the AS. First, I find the authors methodology to derive long-term trends very rough (difference between first and last 5 years of the period considered). Why not trying to evaluate a proper linear trend with some significance level? In addition, why not trying to build an average evolution in observations in wide boxes (northern vs/southern AS) rather than a point-wise comparison? The authors also argue that "Contrasting the long-term temperature changes in the top 200 m in the model and in the observations reveals an overall good agreement in terms of the magnitude and the patterns of upper ocean warming in both summer and winter seasons (Fig S7 and Fig S8, SI). Âż I am not sure to agree with this statement as Fig. S7 and S8 do not show any colorbars and are very patchy. I believe the authors can considerably improve this paragraph and strengthen the reliability of their simulation by extending the validation of the model long-term evolution to other parameters, for which observations or reconstructed products are available. Maps of trends and yearly time-series of key parameters could be compared for model and observations for (1) SST, (2) upper ocean stratification, (3) winds (for several products as these products are not well contrained) and (4) maybe Chl at least over the common period where model and observational data from OC-CCI are available (to be able to compare results with Roxy et al. 2016). It would also be worth trying to compare the oxygen trends over key regions (northern/southern AS) in

model and observational products such as those used in Ito et al. and Schmidtko et al. (2017). Indeed, while I find their analyses on the mechanisms driving the oxygen decline in the model very convincing, these results are not supported by observational evidences. This should be done whenever possible.

Results: P9 L26-27 L30-31: How these numbers do compare with observed estimates globally and regionally? Figure 8bc: Plot a frame on Figure 6 to indicate the averaging region. It would also be interesting to add a time series on these panels showing the upper ocean stratification changes (for instance the static stability index). If ventilation dominates, we should expect this physical index to mirror the oxygen changes at interannual and longer timescales. This model stratification index could be further compared with observational estimates. Figure 9: Replace x-time axis by years (as in Figure 8) instead of months as currently done. Indicate over which region the analysis is performed and refer to Figure 6 to show this region. P14 L10-13 and Fig S9: Why does vertical and lateral components evolution mirror each other? Total advection term is actually a small residual between these two large terms. Can you explain? P14 L15-17: This SST trend in the model could be compared with observations (even if we expect a good agreement because of the relaxation term used) P14 L17-18: The authors argue here that wind changes are particularly prominent during the summer monsoon. However, Figure S10 suggests that winter wind changes also strongly contribute to the annual wind changes shown on Figure 10b. First, I don't understand why the authors decided from that point to focus on the impact of summer wind changes and completely disregard winter wind changes. As previously noted, circulation weakening during the winter monsoon should lead to reduce convective mixing and contribute to reduce the ventilation in the northern AS. I recommend the authors to investigate this potential mechanism futher by performing an additional sensitivity experiment. Second, because of the scarcity of atmospheric observations in the Indian Ocean, decadal to multi-decadal wind signals in this region are not well constrained in atmospheric reanalyses. I would suggest the authors to compare summer and winter wind trends shown on Figure S10 to other reanalyses products (JRA55 or NCEP2 for instance) to ascertain if the reported wind changes are a robust feature in all products. Figure 11: As they are shown at different places in the manuscript, it is not easy to compare results from the sensitivity experiments (shown in Figure 11) with those of the control simulation (which are shown earlier in the manuscript, i.e. Figure 6). I would recommend the authors to either re-add Figure 6a to Figure 11 to ease comparison or to directly show the difference between the sensitivity experiments and the control simulation. This would ease the interpretation of the results derived fro mthe sensitivity experiments. P16 L5-8 and Figure 12: In addition to Figure 12a, I would here show yearly time series of the stratification index against O2 evolution in the control simulation. This would allow to give some hints on whether the ventilation mechanism operating at long timescales also operate at interannual and decadal timescales, i.e. a strong anti-correlation between between these two time series would further strengthen the case for a strong control of ventilation on oxygen concentration at different timescales. P19 L17-23: The upward trend in surface Chl simulated in the model is opposite to what observed trends and model projections (see Roxy et al. 2016). This discrepancy and its implication should be addressed in the discussion section.

Discussion: P21 L5-7: Check consistency of wind trends in other atmospheric reanalyses (see above). To me, summer monsoonal winds rather weakened over the past decades in observations. Also discuss also here apparent inconsistency with Roxy et al. (2016) study (decline in PP over recent decades). P21 L18-23: This is true but also mention that these models project a weakening of the summer monsoon circulation (e.g. Sooraj et al. 2015), which is not the case in the present study.

References: Bindoff, N.L., W.W.L. Cheung, J.G. Kairo, J. Arístegui, V.A. Guinder, R. Hallberg, N. Hilmi, N. Jiao, M.S. Karim, L. Levin, S. O'Donoghue, S.R. Purca Cuicapusa, B. Rinkevich, T. Suga, A. Tagliabue, and P. Williamson, 2019: Changing Ocean, Marine Ecosystems, and Dependent Communities. In: IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, A. Alegría, M. Nicolai, A.

Okem, J. Petzold, B. Rama, N.M. Weyer (eds.)]. 2019. Gopika, S., Izumo, T., Vialard, J., Lengaigne, M., Suresh, I., & Kumar, M. R. (2020). Aliasing of the Indian Ocean externally-forced warming spatial pattern by internal climate variability. Climate Dynamics, 54(1-2), 1093-1111. Sooraj, K. P., P. Terray, and M. Mujumdar (2015), Global warming and the weakening of the Asian summer Monsoon circulation: assessments from the CMIP5 models, Clim. Dyn. 45, 1–20.âĂÍ Swapna, P., Jyoti, J., Krishnan, R., Sandeep, N., & Griffies, S. M. (2017). Multidecadal weakening of Indian summer monsoon circulation induces an increasing northern Indian Ocean sea level. Geophysical Research Letters, 44(20), 10-560.

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