We are grateful for the first referee's efforts in reviewing the manuscript and for the constructive and valuable comments. We believe our paper will significantly improve as a result of his/her comments. We highlight our responses to the general and specific comments and explain the revisions we will make to the paper accordingly. The reviewer's comments are shown below in italics writing while our response is marked in red.

Anonymous Referee #1 Received and published: 7 December 2020

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 biological 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.

We thank the reviewer for the positive comment and the encouraging assessment of the paper.

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.*

As we agree with the reviewer assessment, we will follow his/her suggestion and implement those requested changes. More specifically, we will move Figure 1 and Figure 2 to SI and improve the color scale used in Figure 3 (for surface chlorophyll). We will also add meridional and zonal sections of O2 concentration to evaluate the O2 vertical structure in both model and observations (please see our response to comment #17).

(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.

Following the reviewer suggestion, we will substantially expand the evaluation of modeled long-term evolution of key properties. In particular we will evaluate long-term evolution of temperature and salinity at multiple depths as well as upper ocean static stability and surface chlorophyll-a concentration. More specifically, we contrast trends in SST from four SST products: AVHRR (used to force the model), ERA5, HadISST and the NOAA OISST products (see Fig 1). This comparison shows that all SST products agree on the important warming the Arabian Sea has undergone over the study period, despite differences in the magnitude of warming, with the ERA5 (resp. HadISST) displaying the strongest (the weakest) rates of warming.

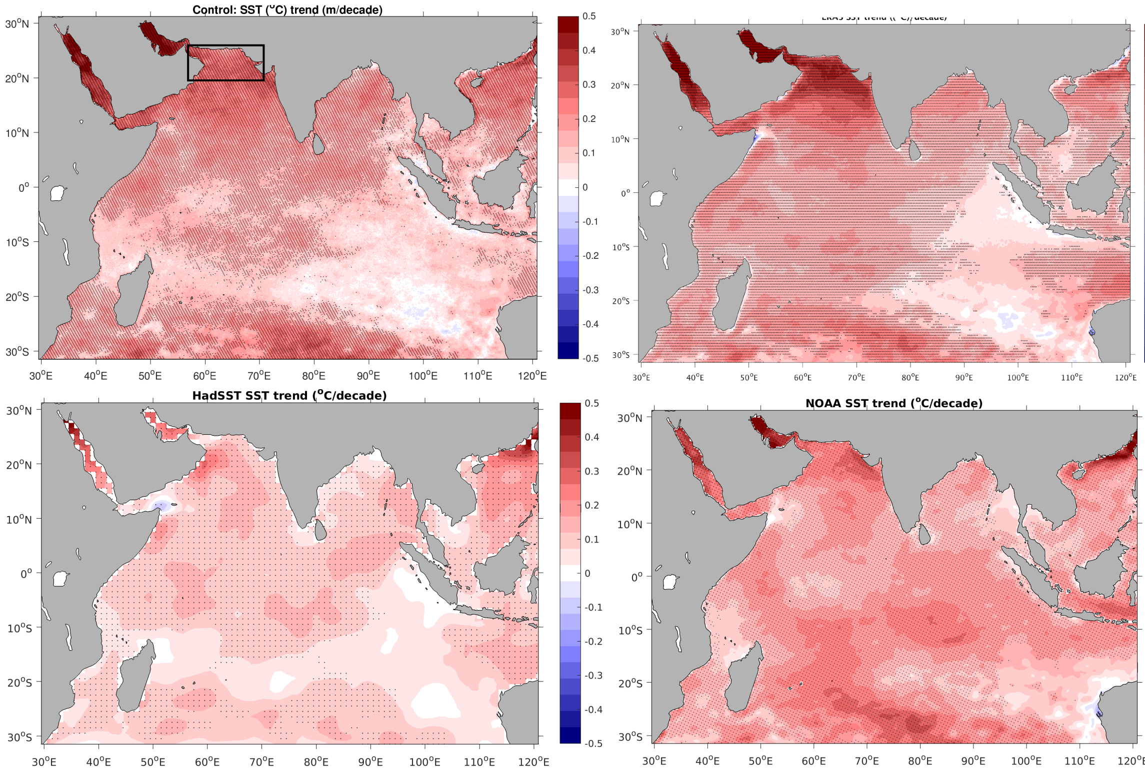

Figure 1: Linear trends in sea surface temperature (in C per decade) from AVHRR Pathfinder (top left), ERA5 (top right), Hadley Center SST (bottom left) and the NOAA OISST product (bottom right). Hatching represents statistically significant trends at 95% confidence interval following a Mann-Kendall (MK) test. The black rectangle in the top left figure indicates the location of northern Arabian Sea box.

Furthermore, we also contrast simulated temperature and salinity interannual anomalies from the northern Arabian Sea to those from WOD2018 observations at different depths (Fig 2 and Fig 3). This comparison reveals a very good agreement between the simulated and observed temperature trends in the upper ocean of the northern Arabian Sea. For salinity, the observational coverage is much sparser with most of the observations coming from the last decade of the simulation (see Fig 4 showing temporal data coverage in the northern Arabian Sea). This does not allow us to use observations to validate long-term changes in salinity in the model.

Additionally, we also evaluate the evolution of vertical stratification and static stability in the Arabian Sea in the model. As salinity observations are very sparse over the region during the study period we contrast simulated static stability to that from reanalysis products such as SODA and ORAS5 (Fig 5). This comparison reveals that overall the simulated increase in vertical stratification in the Arabian Sea is comparable to similar trends observed in the ORAS5 and SODA reanalyses, although with local differences in their magnitude and regional patterns.

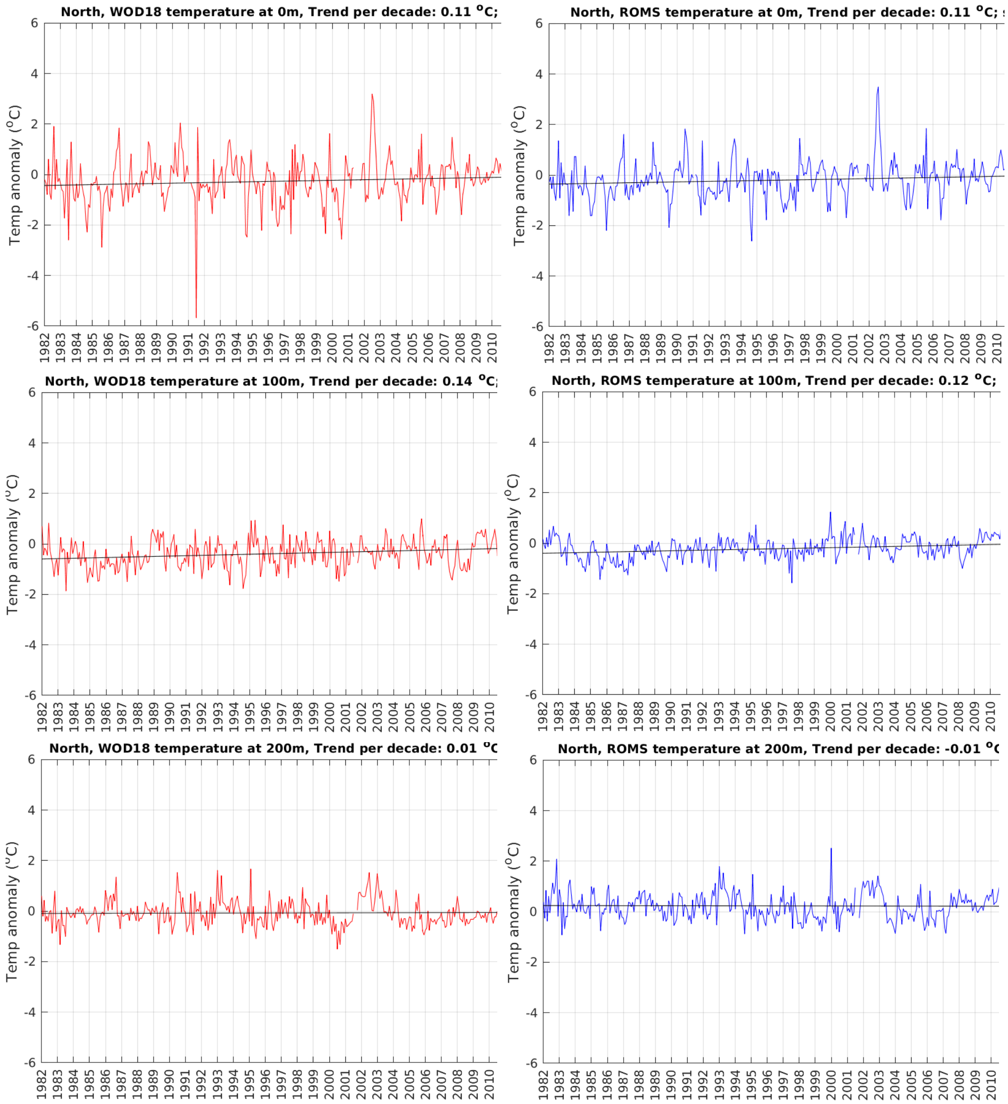

Figure 2: Interannual anomalies and linear trends in northern Arabian Sea temperature from WOD2018 observations (left) and ROMS model (right) at surface (top), 100m (middle) and 200m (bottom). Observed temperature interannual anomalies are estimated by binning in-situ observations in the northern Arabian Sea box (shown in Figure 1) and deseasonalizing the time series. Model time series are evaluated similarly by subsampling the model outputs at observation points.

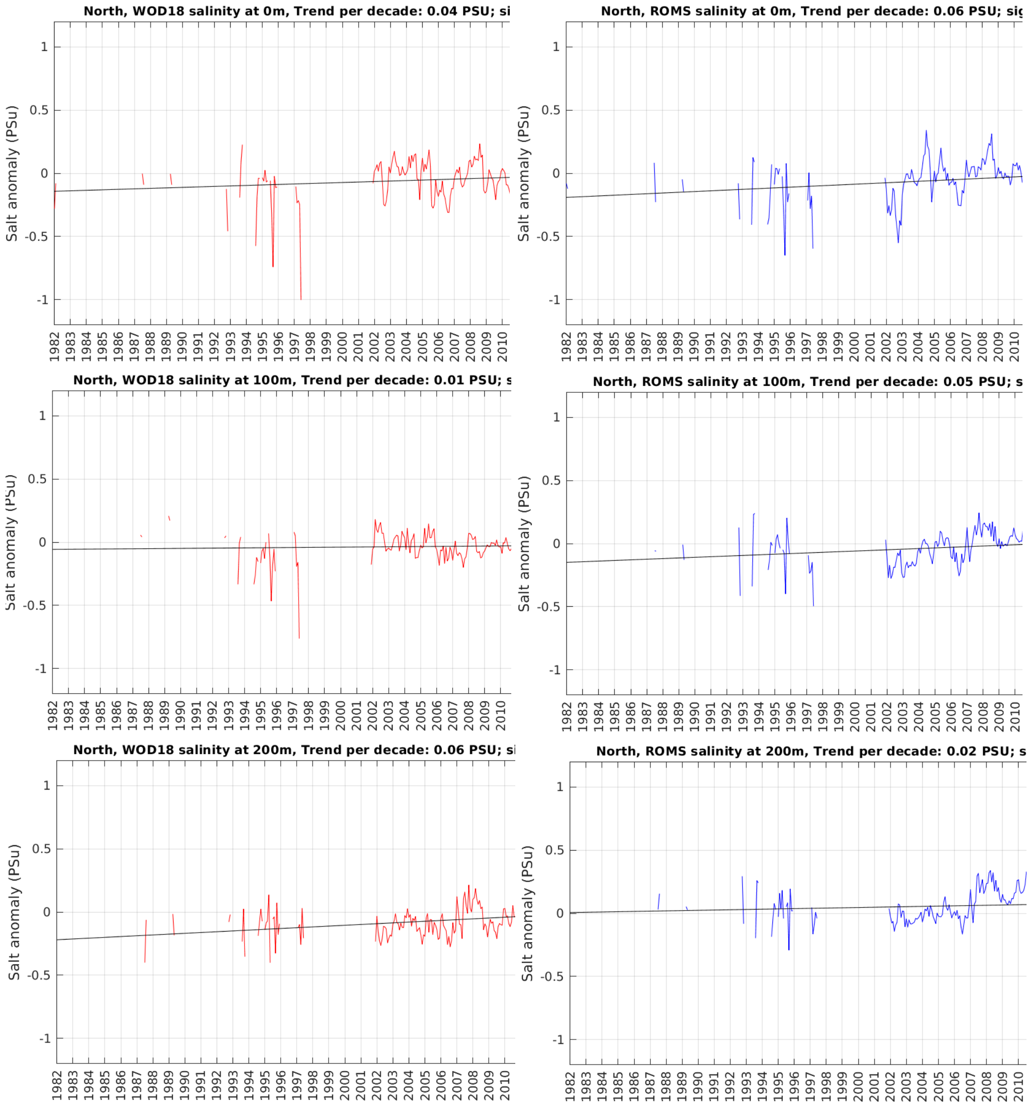

Figure 3: Interannual anomalies and linear trends in northern Arabian Sea salinity from WOD2018 observations (left) and ROMS model (right) at surface (top), 100m (middle) and 200m (bottom). Observed salinity interannual anomalies are estimated by binning in-situ observations in the northern Arabian Sea box (shown in Figure 1) and deseasonalizing the time series. Model time series are evaluated similarly by subsampling the model outputs at observation points.

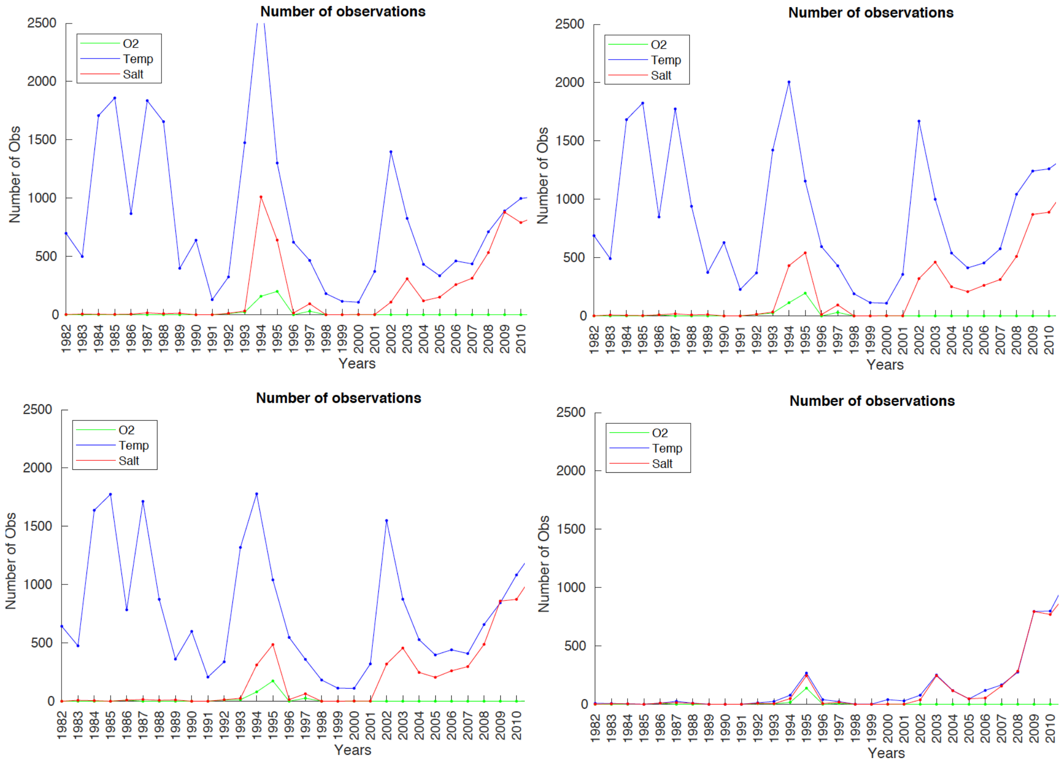

Figure 4: Temporal data coverage over the study period for temperature (blue), salinity (red) and O2 (green). For each of the three variables, temporal data coverage is calculated by counting the number of annually binned observed data (WOD2018) in the northern Arabian Sea box (shown in Figure 1) at surface (top left), 100m (top right), 200m (bottom left) and 1000m (bottom right). The graphs indicate that temperature is the most well observed variable with a decent coverage that spans the study period, while O2 and to a lesser extent salinity show a much sparser coverage that precludes quantifying long-term trends over the simulated period.

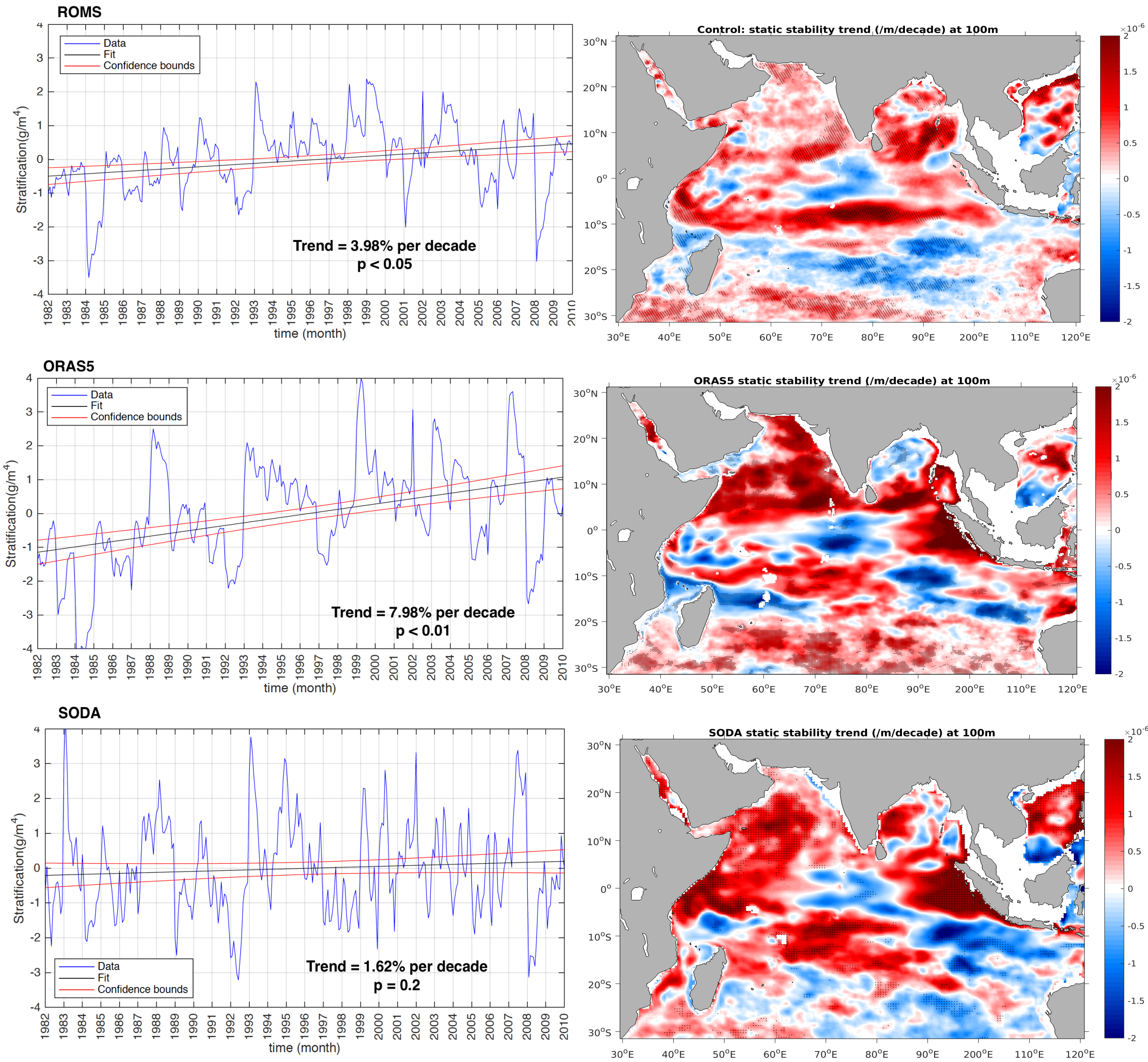

Figure 5: (left) linear trend in vertical stratification at 100m in the northern Arabian Sea box (location in Figure 1) (in g/m4 per decade) and (right) maps of trends in static stability at 100m (in m^{-1}) from the model (top), ORAS5 (middle) and SODA reanalysis (bottom).

For biological variables, we only evaluate long-term changes in surface chlorophyll as O2 (and NO3) observations are extremely limited in the area over the study period (please see Fig 4). We would like to point out that the O2 data analyzed in the Schmidtko et al (2017) study mentioned by the reviewer covers a different and longer period of time (1960-2010).

Finally, as surface chlorophyll satellite data is available only from september 1997, we contrast simulated chlorophyll to observations over the common period from september 1997 to the end of the year 2010. The 13-year period is too short to extract meaningful long-term trends. Yet, this comparison is still useful as it reveals a decent agreement between the model and observations over the study period with a moderate correlation of 0.48 between the modeled and observed interannual anomalies in the northern Arabian Sea (Fig 6).

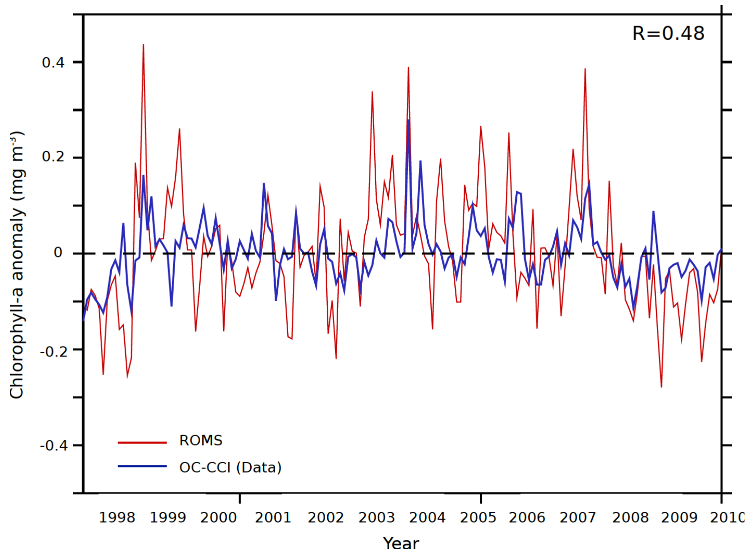

Figure 6: Surface chlorophyll-a interannual anomalies averaged across the northern Arabian Sea (location in Figure 1) as simulated in ROMS (red) and from OC-CCI satellite data product (blue) over the common coverage period from 1997 to 2010. No significant trend can be identified over the period in either of the data or the model. Note the decent correlation (R=0.48) between modeled and observed interannual anomalies.

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.

We decided to focus on summer wind changes because we believe that the Arabian Sea OMZ is much more sensitive to changes in summer winds than winter winds as was shown in Lachkar et al (2018). Indeed, in this previous work we found that the volume of both hypoxic and suboxic waters in the Arabian Sea is much more responsive to perturbations in summer winds than to comparable perturbations in winter winds.

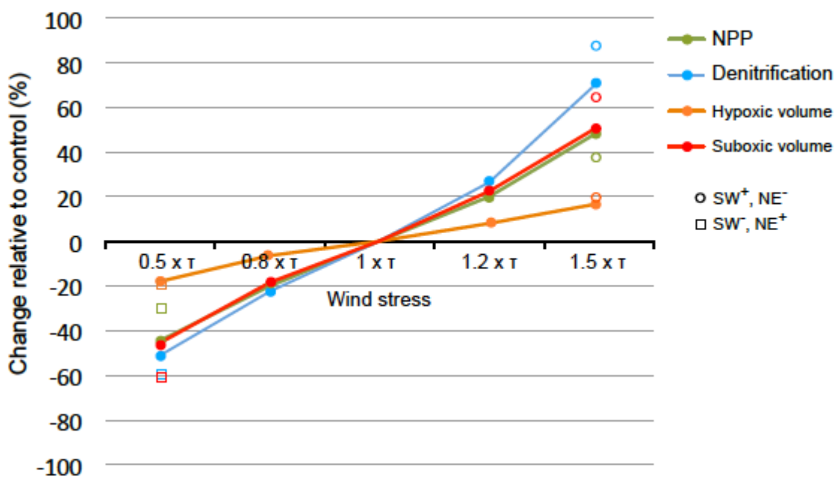

Figure 7: (from Lachkar et al., 2018) Biogeochemical response to monsoon wind intensity changes. Relative changes in response to monsoon wind intensity perturbations in net primary production (green), denitrification (blue), suboxic volume (red) and hypoxic volume (orange). Open circles (respectively, squares) indicate the results from the simulation where summer monsoon wind stress is increased (respectively, decreased) by 50 % and winter monsoon wind stress is decreased (respectively, increased) by 50 %.

This is likely due to the fact that convective mixing is more sensitive to winter cooling than monsoon wind intensity. The dominance of surface buoyancy fluxes over wind mixing in driving convective deepening of mixed layer during winter monsoon season has been established in a couple of previous observational and modeling studies (e.g., Weller et al., 2002, Prasad 2004). Moreover, changes in winter winds are associated with a weaker perturbation in wind stress curl in the Arabian Sea relative to summer wind changes (see Fig 8).

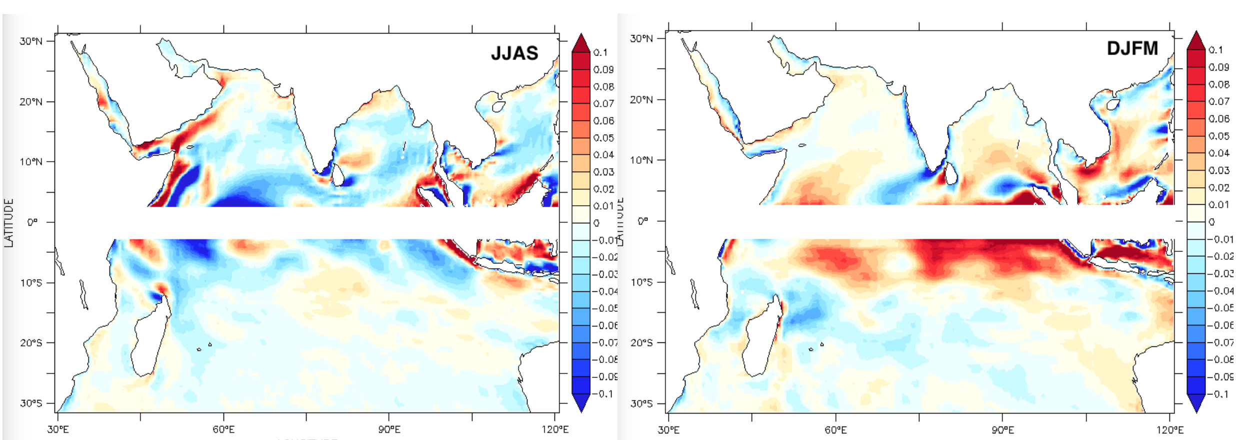

Figure 8: Linear trends in Ekman suction velocity (upwelling) during summer (left) and winter (right) monsoon seasons (in m/day per decade).

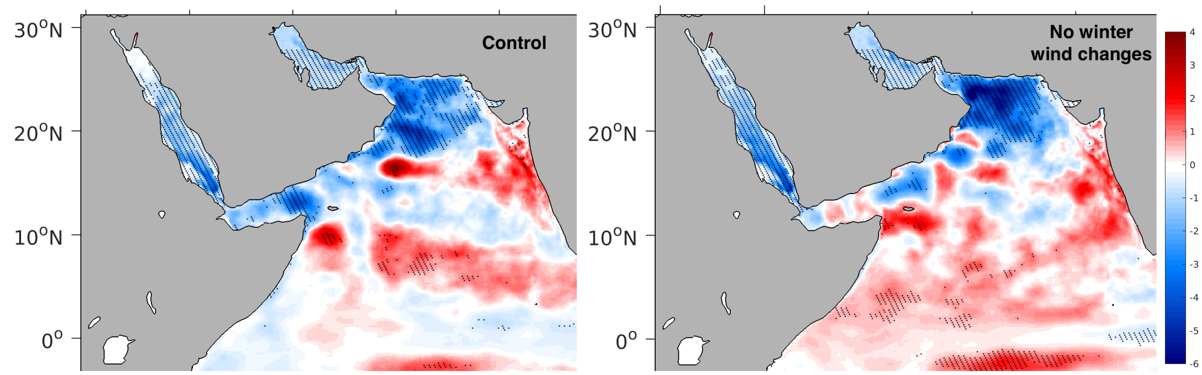

Figure 9: Linear trends in O2 inventories in top 200m (in % per decade) in the control simulation (left) and under climatological winter winds (right). Ekman suction velocity (upwelling) during summer (left) and winter (right) monsoon seasons (in m/day per decade).

However, following the reviewer's suggestion we have run an additional simulation where winter winds (DJFM) were set to be climatological. The results from this additional sensitivity experiment (shown in Fig 9) confirm that changes in winter winds do not contribute to the simulated O2 decline in the northern Arabian Sea. Indeed, in the absence of winter wind changes, deoxygenation in the northern AS is maintained and is only slightly weaker in the western AS. This indicates that changes in winter winds do not contribute to deoxygenation in the northern AS and may only very partially explain deoxygenation in the western AS. Additionally, an increase in vertical stratification over the study period (similar to that obtained in the control) is simulated in the absence of winter wind changes, further confirming that the weakening of winter convective mixing in the northern Arabian Sea is more associated with winter warming (see SST changes in winter, Fig S10 in SI) than changes in the intensity of winter winds.

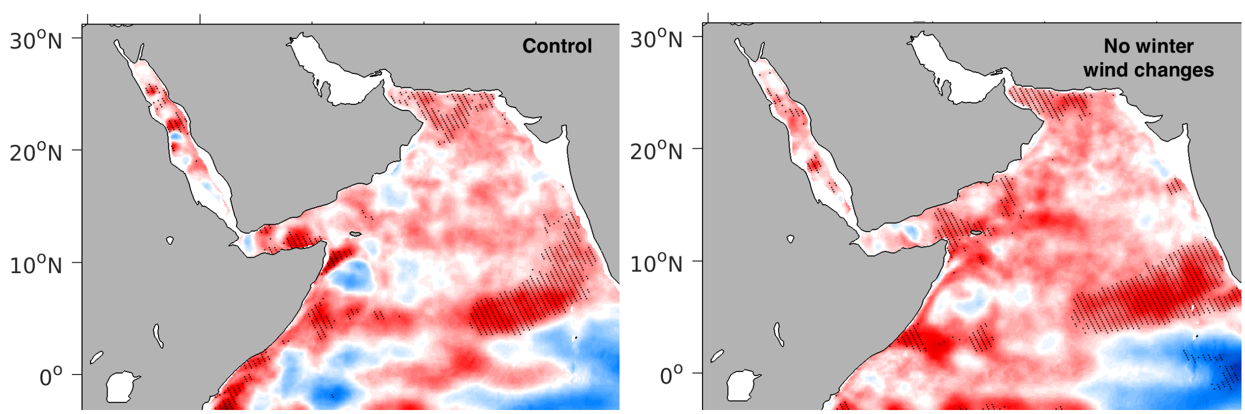

Figure 10: Linear trends in vertical stratification at 100m (in kg/m4 /decade) in the control (left) and in the climatological winter wind run (right).

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.

We thank the reviewer for this suggestion. We will include a schematic summarizing the main findings in the revised version of the manuscript.

Detailed comments:

Abstract:

1) *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”.*

   We will change “show” to “suggest”.

2) *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. Âz 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).*

   We will cite the SROCC report (Bindoff et al., 2019).

3) *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 circulation over the historical period (e.g. Swapna et al. 2017). This uncertainty should be discussed in the manuscript.*

   Following the reviewer suggestion, we have considered the trends in surface winds from different products (Fig 11). As pointed out by the referee, there seem to be large discrepancies in terms of the magnitude and even the direction of the change in surface winds as these reanalyses are not very well constrained. Indeed, while the NCEP2 winds show a modest increase in upwelling-favorable winds in the western Arabian Sea, there seems to be no such an increase (and even a slight weakening) in the JRA55 winds. Therefore, we will discuss this uncertainty in the revised version of the manuscript. While we acknowledge this as one of the caveats of the study, we also believe that this has relatively limited implications for the conclusions of the study as we demonstrate here that the dominant factor in deoxygenation in the northern Arabian Sea is surface warming, with the winds playing only a secondary role.

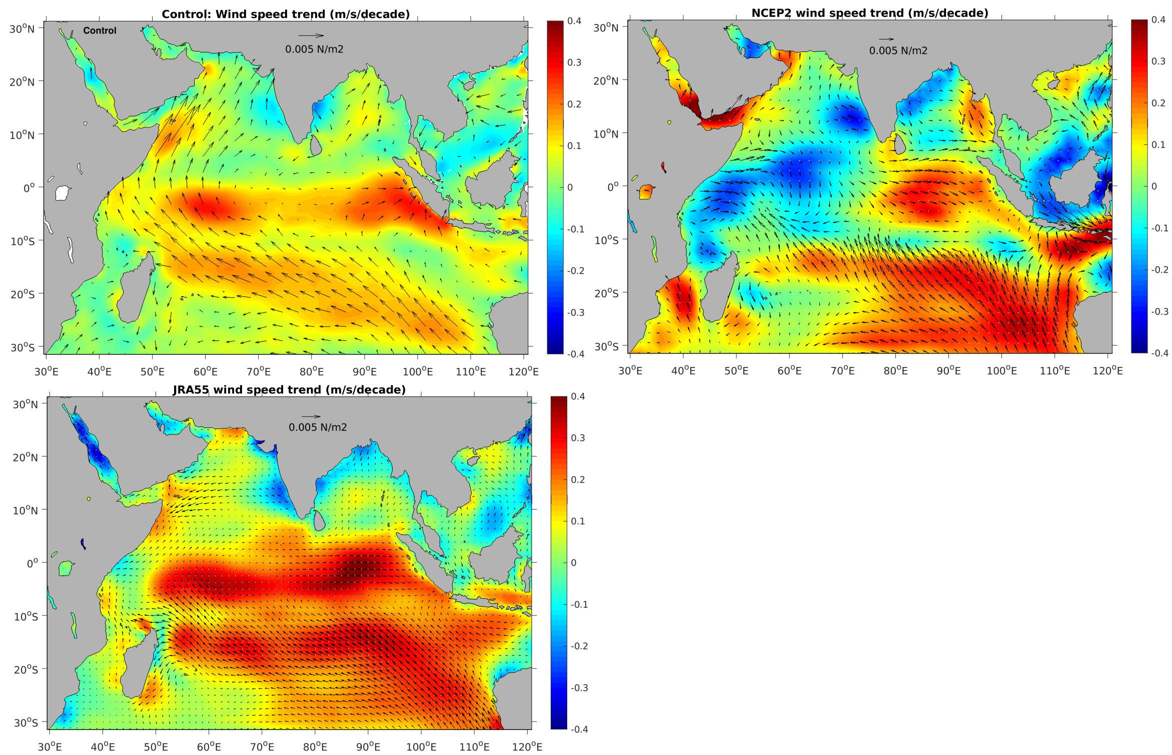

Figure 11: Linear trends in surface winds from ECMWF (top left, here used to force the model), NCEP2 (top right) and JRA55 reanalysis product (bottom left). Color shading indicates trends in wind speed (in ms^{-1} $decade^{-1}$) whereas arrows show trends in wind stress vector.

4) *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".*

Will be changed.

5) *P1 L7 : Replace Ân observation-based Â ´ z by Â ˙ n forced by an atmospheric reanalysis Â ´ z*

Will be changed.

6) *P1 L12 : Â ˙ n in ´ particular in the Gulf Âz : 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 Ân´ including in the Gulf Âz for not over-emphasizing the role of the Gulf warming. ˙*

Will be changed.

Introduction :

7) *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.*

We will refer to this report in the revised version.

8) *P2 L13: "hypoxic events" Do you mean "anoxic events"?*

We refer to low O2 events in the hypoxic range (O2<60-80 mmol/m3) not necessarily anoxic events. These have been shown to stress sensitive organisms and cause loss of marine biodiversity and shifts in the food web structure (Rabalais et al., 2002; Vaquer-Sunyer and Duarte, 2008; Laffoley and Baxter, 2019).

9) *P2 L16-35: Please also refer to SROCC 2019 report about the confidence we have these reported changes.*

We will refer to this report to indicate the confidence we have in these reported changes.

10) *P3 L8: Regarding the enhanced warming in the AS, you should refer to Gopika et al. (2020) that investigate this in details.*

We thank the reviewer for his/her suggestion. We will refer to this paper in the revised manuscript.

11) *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.*

There are two differences between our study and Roxy et al (2016) that may explain this inconsistency. First, in their analysis Roxy and colleagues have considered surface chlorophyll while we are analyzing trends in vertically integrated biological productivity. While these two quantities are usually strongly correlated, they are not always identical, especially in tropical systems where deep chlorophyll maxima are a common feature. Indeed, the trends in surface chlorophyll in our simulation are quite different and are much weaker in the open ocean in comparison to trends in NPP (Fig 12). The second main difference with Roxy et al. (2016) is the study period. Indeed, the satellite chlorophyll data presented in Roxy et al (2016) is based on a different and shorter period (1998-2013) than in the present study (1982-2010). Interestingly, our model also simulates a decline of surface chlorophyll in the western Arabian Sea when the analysis is restricted to the period between 1998 and 2010 (Fig 12). We believe the high sensitivity of the trends to the study period is a consequence of the strong decadal variability in the region. This can also be seen in chlorophyll trends based on CMIP models presented in

Roxy et al (2016). These time series that cover a longer period (1950-2005) reveal strong decadal variability with a decline in surface chlorophyll from the 1950s to the late 1970s but an increase from then on. We believe this can also contribute to this apparent inconsistency.

We will discuss this point thoroughly in section 4.2 (comparison with previous works) of the revised manuscript as suggested by the reviewer.

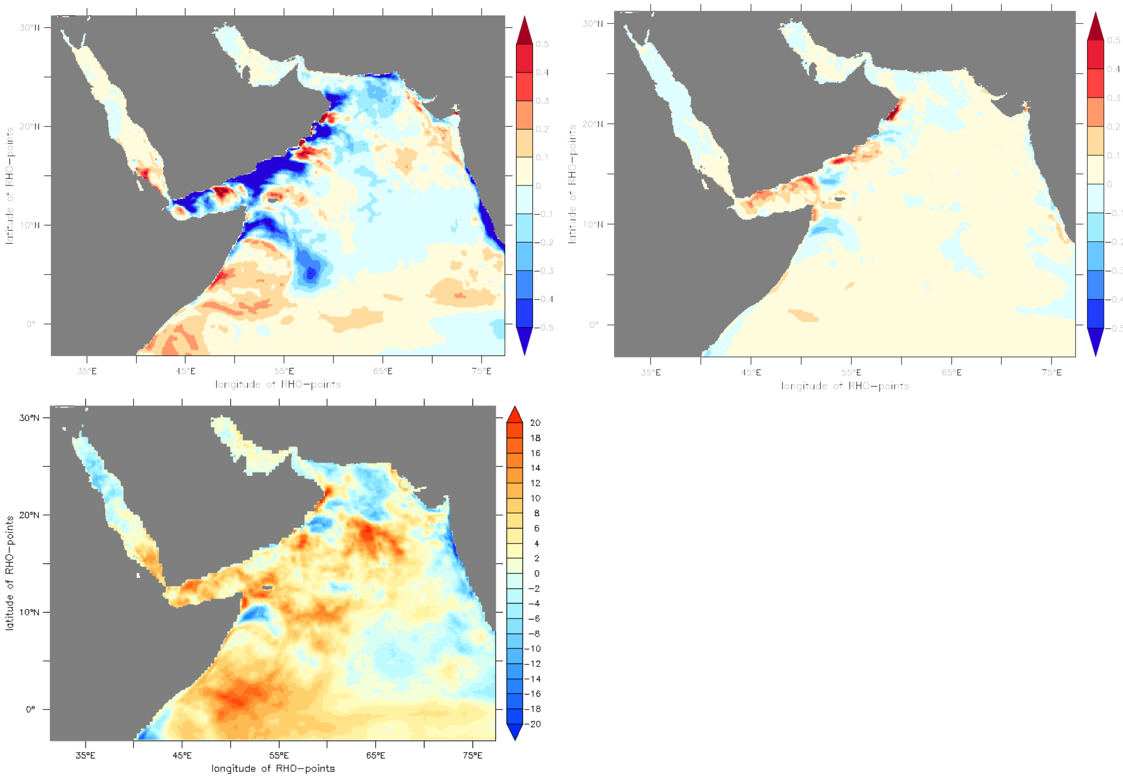

Figure 12: Linear trends in simulated surface chlorophyll (in mg/m3/decade) (top left) over the period (1998-2010) and (top right) between 1982 and 2010. (bottom left) linear trends in biological productivity between 1982 and 2010 (in mol/m2/yr/decade).

12) *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.*

This reference will be removed.

13) *P3 L23: Remove "largely"*

Will be removed.

14) *P3 L29: Change ", in particular in the Gulf" into ", with a significant contribution from the Gulf warming"*

This change will be implemented.

Methods:

15) *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.*

When this research effort was initiated back in 2017, the SODA reanalysis (version 2.2.4) used for boundary conditions was not available past the year 2010. Therefore we decided to run the simulation until 2010 (despite the fact that ERA forcing was available for more recent years). In 2018, a more recent version of SODA (version 3) was made available with coverage extending to recent years. Unfortunately, this version (and the more recent ones) has among other problems a major issue in the Arabian/Persion Gulf and the northern Arabian Sea (our focus area) with surface salinities unrealistically low (in the 33psu) nearly 8 psu below the actual observations (Fig 13). Because we restore our surface salinity to SODA, and as the formation of dense water in the Arabian Gulf is extremely important for the ventilation of the Arabian Sea (Lachkar et al., 2019), we decided against extending the simulations using the most recent SODA version (3.x). We plan in the future to switch to an alternative reanalysis (e.g., ORAS5) to force our future simulations.

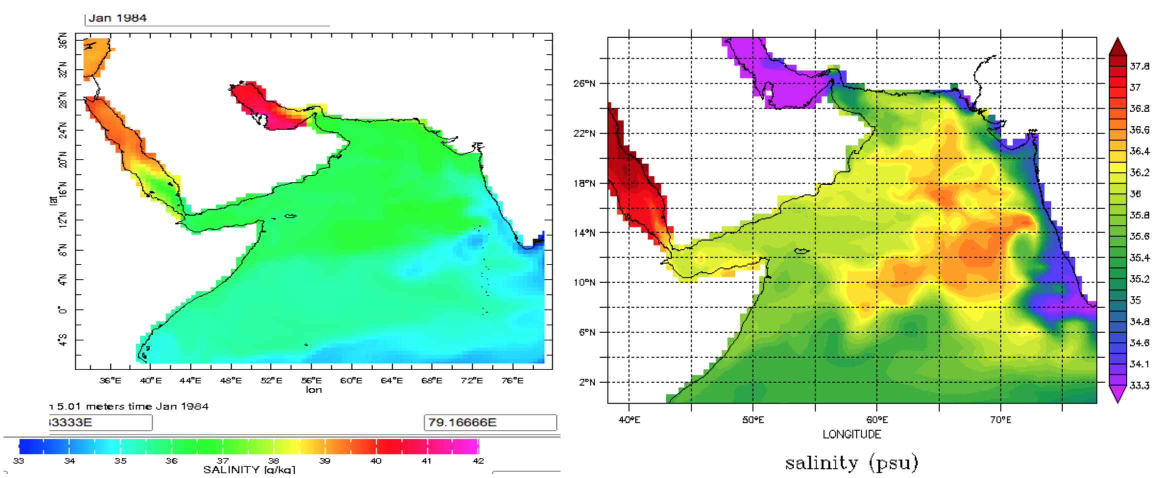

Figure 13: Sea surface salinity (in psu) from SODA version 2.2.4 (left) and SODA version 3.1 (right) in January 1984.

16) *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.*

These changes will be implemented. Please see our response to the previous general comment A.

17) *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.*

Following the reviewer suggestion, we will add an evaluation of the vertical structure of the simulated OMZ by showing O2 across one north-south (65E) and one east-west (18N) sections (Fig 14 shown below).

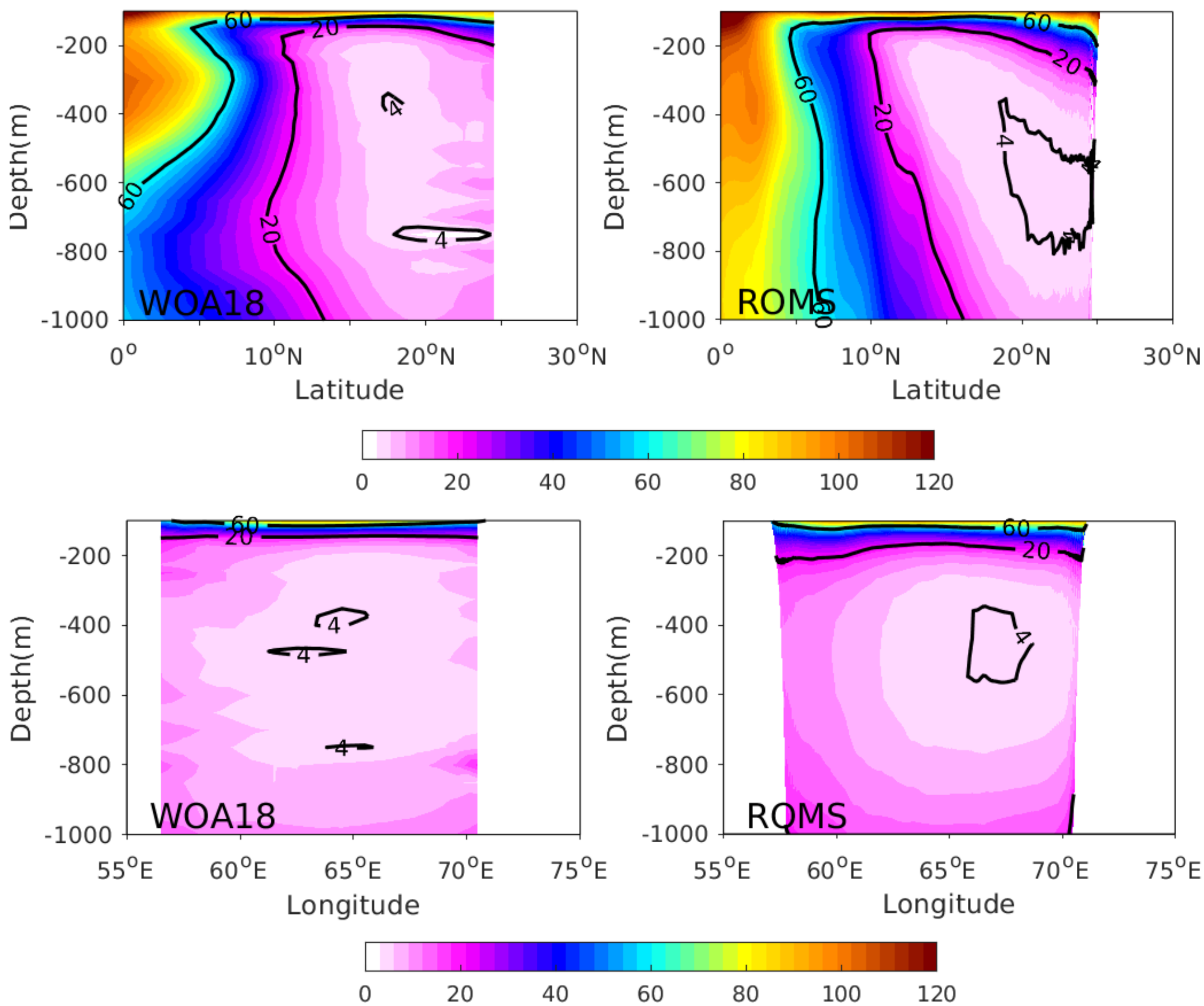

Figure 14: O_2 concentration across 65E (top) and 18N (bottom) from WOA18 (left) and ROMS (right).

18) *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?*

Following the reviewer suggestion, we substantially expand the evaluation of long-term changes in the model. Please see our detailed response to previous general comment A. We will also remove Fig S7 and Fig S8 and replace them by Figs 1, 2, 3, 4, 5 and 6 (in this document).

19) *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). Âz 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).*

We will remove these figures (Fig S7 and S8) and replace them with figures Fig1 to Fig 6 in this document following the reviewer suggestion. Please see our detailed response to General Comment A.

20) *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.*

Unfortunately, O2 observations are extremely limited in the area, especially during the 1982-2010 study period (most available observations were collected in 1994/1995 as a part of the JGOFS Arabian Sea Process Study) and hence cannot be used to estimate trends. Please see Fig 4 and our response to General Comment A.

Results:

21) *P9 L26-27 L30-31: How these numbers do compare with observed estimates globally and regionally?*

Will be done. We will compare these numbers with published regional and global estimates.

22) *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.*

We will show the averaging region in the figure as suggested.
Following the reviewer suggestion, we will also superimpose upper ocean stratification changes on top of O2 changes. Here below (Fig 15), we show an example with vertical stratification at 100m superimposed on O2 anomalies in the top 200m. As can be seen in the graph, the stratification index mirrors relatively well the O2 changes at interannual and longer timescales (correlation coefficient R= -0.65), confirming the dominance of ventilation control over O2 changes.

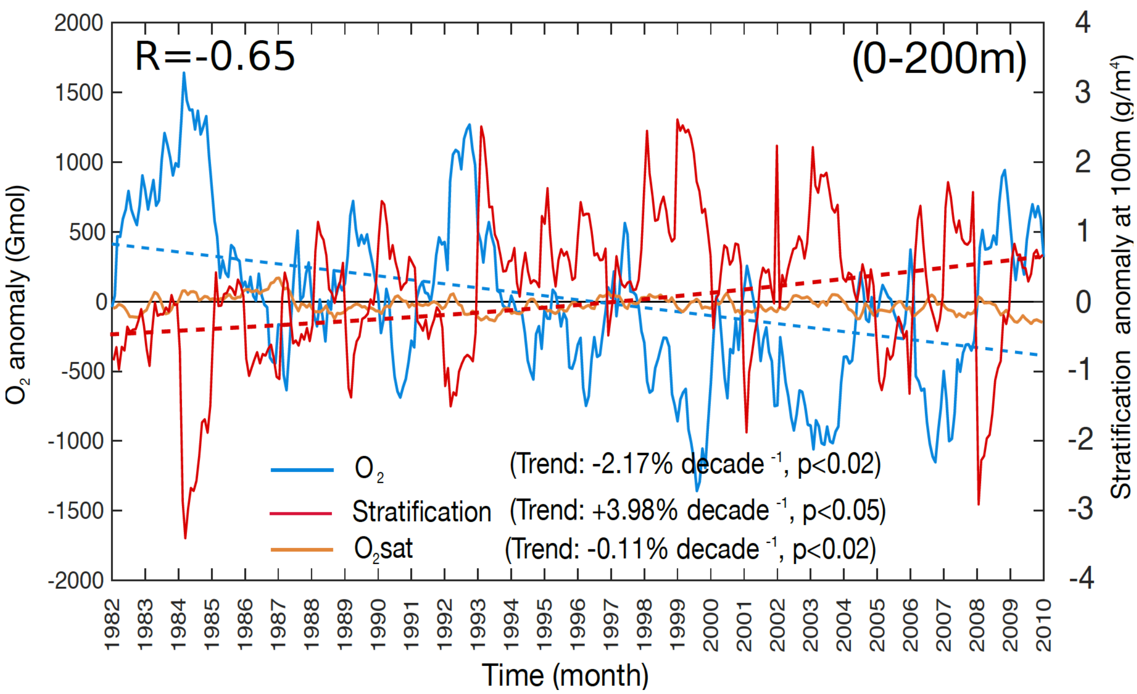

Figure 15: O_2 content (blue) and O_2 saturation (orange) anomalies in the top 200m on top of vertical stratification anomalies (red) at 100m as a function of time. The blue and red dashed lines indicate the trend line in O_2 inventory and vertical stratification at 100m, respectively.

23) *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.*

This change will be implemented.

24) *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?*

This is due to the fact that the net divergence of the transport term is much smaller than the vertical and lateral contributions taken separately (that partially compensate each other).
In the revised manuscript, we will split the total ventilation into advective and diffusive (subgrid mixing) components (see Fig 16 below). This reveals that most of the decline in ventilation seen in the upper ocean is caused by a reduction in the contribution by vertical mixing. This is consistent with the enhanced vertical stratification suppressing vertical mixing in the upper ocean. Below 200m, the contribution of subgrid mixing is negligible and most of the ventilation reduction is caused by a decrease in advection of O2.

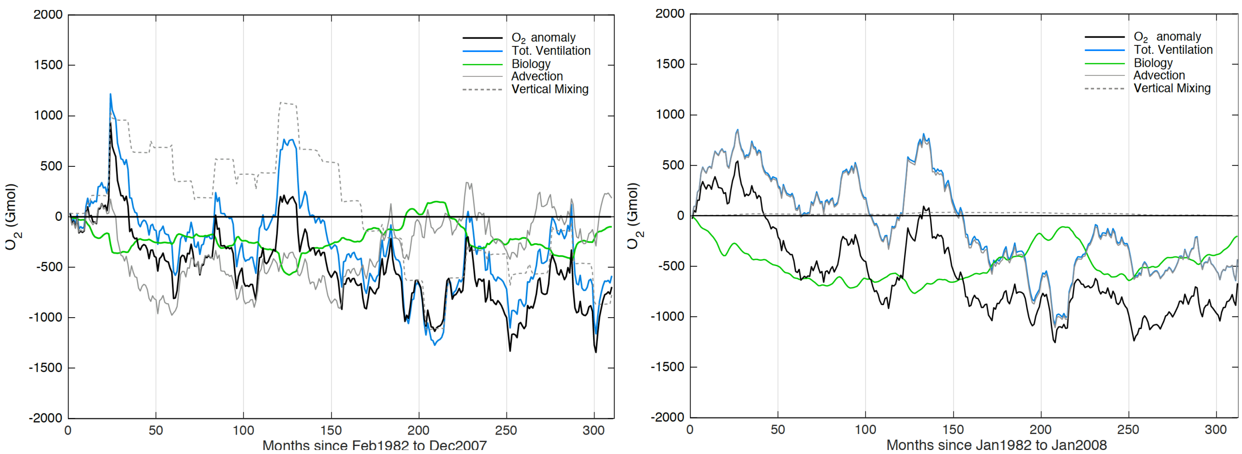

Figure 16: Cumulative O_2 anomalies (black) and their biological (green) and physical (blue) sources in (left) the (100-200m) subsurface and (right) the intermediate (200-1000m) ocean. The contributions of advection and subgrid mixing are shown in thin solid and dashed grey lines, respectively.

25) *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)*

Will be done. The agreement as expected is good (see Fig 17).

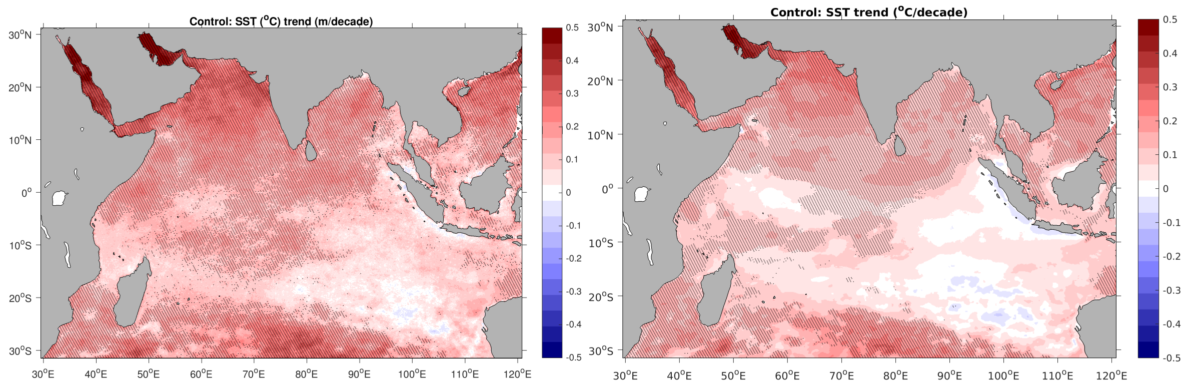

Figure 17: Linear trends in SST (in C per decade) from AVHRR Pathfinder (left), and from the control simulation (right).

26) *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.*

Done. Please see our response to previous General Comment B.

27) *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.*

Done. Please see our response to previous comment #3.

28) *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.*

We will show the differences between the sensitivity experiments (currently in the Supp Info) and move current panels in Fig 11 to the SI.

29) 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.

Done. Please see also our response to previous comment #22.

30) *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.*

Done. Please see our response to comment #11.

Discussion:

31) *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).*

Please see our response to comments #3 and #11.

32) *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.*

We agree that there is a lot of uncertainty around the future changes in summer monsoon winds. Some models suggest a strengthening while others project a weakening.
We will mention this in the revised manuscript, but the current study suggests that most of the ongoing deoxygenation in the northern AS is anyway driven by warming and much less by changes in winds. Therefore, we think that in those models, the impact of warming may dominate the deoxygenation signal in the northern AS.

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. MassonDelmotte, 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.âA˘´l 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.

References:

Lachkar, Z., Lévy, M., and Smith, S.: Intensification and deepening of the Arabian Sea oxygen minimum zone in response to increase in Indian monsoon wind intensity, Biogeosciences, 15, 159–186, https://doi.org/10.5194/bg-15-159-2018, 2018.

Lachkar, Z., Lévy, M., 5 and Smith, K.: Strong intensification of the Arabian Sea oxygen minimum zone in response to Arabian Gulf warming, Geophysical Research Letters, 46, 5420–5429, 2019.

Laffoley, D. D. and Baxter, J.: Ocean Deoxygenation: Everyone's Problem-Causes, Impacts, Consequences and Solutions, IUCN, 2019.

Prasad, T. G. (2004), A comparison of mixed-layer dynamics between the Arabian Sea and Bay of Bengal: One-dimensional model results, *J. Geophys. Res.*, 109, C03035, doi:10.1029/2003JC002000.

Rabalais, N. N., Turner, R. E., and Wiseman Jr, W. J.: Gulf of Mexico hypoxia, aka "The dead zone", Annual Review of ecology and Systematics, 33, 235–263, 2002.

Roxy, M. K., Modi, A., Murtugudde, R., Valsala, V., Panickal, S., Prasanna Kumar, S., Ravichandran, M., Vichi, M., and Lévy, M.: A reduction in marine primary productivity driven by rapid warming over the tropical Indian Ocean, Geophysical Research Letters, 43, 826–833, 2016.

Vaquer-Sunyer, R. and Duarte, C. M.: Thresholds of hypoxia for marine biodiversity, Proceedings of the National Academy of Sciences, 105, 15 452–15 457, 2008.

Weller, R. A., A. S. Fischer, D. L. Rudnick, C. C. Eriksen, T. D. Dickey, J. Marra, C. Fox, and R. Leben (2002), Moored observations of upper-ocean response to the monsoons in the Arabian Sea during 1994–1995, *Deep Sea Res., Part II*, **49**, 2195– 2230.