We would like to thank the second reviewer for the comments that we hope will significantly improve our manuscript. We highlight our responses, point by point, to the reviewer’s general and specific comments and indicate 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 #2

Received and published: 4 January 2021

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.

The focus of the Gopika et al 2020 paper is on the period (1871-2016), which is different from our study period (1982-2010). In particular most of the uncertainties in SST trends referred to in that paper have to do with the quality and the gaps in data prior to the 1960s. Moreover, the uncertainties debated in that paper relate to the large-scale east-west and north-south patterns of the warming Indian Ocean and less about the average Arabian Sea warming itself. Finally, the warming in the Arabian Sea over the study period has been faster than the global average (e.g., Beal et al., 2020).
However, we do agree that most of the deoxygenation we report (and analyze) in this study is located in the northern Arabian Sea. Therefore, we will change the title of the paper to:
“Fast local warming of sea-surface is the main factor of recent deoxygenation in the northern Arabian Sea”.

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.

The focus region is the Arabian Sea OMZ. As deoxygenation (and its implications for the OMZ) is largest in the northern part of the AS, we devote particular attention to the northern AS. Please note that we will change the title to "Fast local warming of sea-surface is the main factor of recent deoxygenation in the <u>northern</u> Arabian Sea" to make this clearer. We will also make this clearer in the abstract and introduction and in the discussion of our figures in the revised manuscript.

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.

It is true that the importance of the Gulf warming for the AS OMZ has been explored in Lachkar et al. 2019 using a set of idealized future warming scenarios of the Gulf. In the present study, however, we explore the drivers of recent changes in O2 and demonstrate that the recent warming in the Gulf contributes to those trends, but is far from entirely explaining them. Indeed, significant deoxygenation is simulated when the AS warms up, even when the Gulf temperature does not change (see Fig11 of the paper and Fig 2 in this document). Therefore, the present study is consistent with the findings of Lachkar et al (2019) but goes beyond in terms of the processes taken into consideration (warming and wind changes over the entire Arabian Sea) and the nature of the perturbation (real observed changes vs. idealistic future scenarios).

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 Rosorio Gomes et al. (2014) ´ is largely debated. On the other hand, many relevant references, for e.g. Sandeep and Ajayamohan, 2015, are not cited.

Although the focus of the Schmidtko et al. (2017) is global, local O2 trends are shown regionally including in the Indian Ocean and the Arabian Sea. Indeed, in their extended Data Figure 1, panel a, O2 trends (in units of percentage of local dissolved oxygen) are shown in the AS. This figure indicates a drop of O2 in the western and northern AS as well as along the west coast of India between 1960 and 2010 that we refer to in our introduction.

This study is consistent with many other observational studies that have reported or suggested deoxygenation in the Arabian Sea and that we cite. Namely:
Ito et al. (2017),
Laffoley and Baxter, 2019,
Piontkovski and Al-Oufi (2015)
Banse et al. (2014)
Queste et al. (2018)
Al-Ansari et al., 2015; Al-Yamani and Naqvi, 2019 with a focus on the Arabian/Persian Gulf.

We also cite do Rosário Gomes et al. (2014) because it points to an important potential link between recent oxygen trends in the region and observed changes in the plankton community composition.

As for the study by Sandeep and Ajayamohan (2015) referred to by the reviewer, it is about potential changes in Indian monsoon atmospheric circulation and is not directly linked to O2 or deoxygenation in the Arabian Sea. However, we will cite this paper in the revised manuscript when we discuss potential changes in regional monsoon winds.

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.

O2 trends:
In the original manuscript we explain the calculation method:
"To investigate long-term changes in O2 time series, data was deseasonalized by removing monthly climatologies from the original time series.
Furthermore, to identify significant trends in O2 we used the non-parametric Mann-Kendall (MK) test (Mann, 1945; Kendall, 1948) that does not assume normality of data distribution and hence is less sensitive to outliers and skewed distributions."
For more clarity, we will add:
"Linear trends were computed from the slope of the least squares regression line."

O2sat/AOU:
We agree with the reviewer that while oxygen saturation (O2 sat) and the apparent oxygen utilization (AOU) may be familiar concepts for marine biogeochemists and the oxygen community, they may indeed need to be explicitly defined for a larger audience not necessarily familiar with these notions. Therefore for more clarity we will add an explicit definition of AOU and O2.

We will include:
"O2sat is the oxygen saturation concentration (in mmol/m3) computed at 1atm pressure from temperature and salinity following Garcia and Gordon (1992). It corresponds to the maximum O2 concentration in seawater at equilibrium, for a given temperature and salinity."

"AOU (apparent oxygen utilization) is the difference between oxygen saturation O2sat and the actual O2 concentration and can be estimated as:
AOU=O2sat-O2
It is a measure of O2 utilization through biological activity since the water parcel was last at equilibrium in contact with the atmosphere. Therefore it is sensitive to both biological productivity and circulation (ventilation)"

O2 budget:

The O2 sources and sinks equations were included in Lachkar et al (2016) that we refer to in the present manuscript. However, for more clarity we will include them in the revised manuscript.

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.

We thank the reviewer for pointing out those typos that will be corrected.
P4 L25: typo will be corrected (78W changed to 78E).
Missing longitude labels in some supplementary figures will be added.
A reference scale vector will be added to Fig 2, Fig 10 and Fig S10.
The area in the northern Arabian Sea (north of 20N) that we average over will be shown explicitly.
The inventories which are vertically integrated O2 concentrations are shown at each grid point. As for change expressed in % units, it is the amount of change as percentage of local dissolved oxygen inventory as routinely presented in many deoxygenation studies (e.g., Schmidtko et al, 2017). We will make this clearer in the revised manuscript.

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.

This will be fixed. We will move these two statements to the methods section.

8. P4 L22: "major rivers in the northern Indian Ocean". Provide details as to which rivers are considered, particularly in the Arabian Sea.

Will be done.

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).

Validation of the model at the subsurface:

Beyond the validation of surface properties (including variables that are not restored such as currents in Fig2, and NO3 and Chla in Fig3) we have validated the model at the subsurface:

1) O2 and NO3 distributions between 250 and 700m from the model are compared to observations (Fig 4 and Fig S6).
2) Additionally, we have shown a quantitative comparison of O2 and NO3 profiles in the top 400m between the model and the observations as a part of the Taylor diagrams presented in Fig 5.

Additionally, in the revised manuscript we will add a thorough validation of long-term changes, including changes in temperature and salinity in the subsurface as well as stratification in the upper ocean. Please see our detailed response to General Comment A by referee #1.

Model drift:
This has already been addressed in the original manuscript. Please see the analysis of model drift in page 1 of the Supplementary. Please also see salinity and oxygen long-term trends shown in Figs S1, S2, S3, S4 and S5. Please note that the model drift is analyzed in two vertical layers: 0-200m and the 200-1000m as clearly stated in these figures captions.

10. P9 L28: How do you ascertain that 14% increase in denitrification is due to 10% decline in oxygen? Are these anomalies correlated?

We would like to stress that we are not referring to "10% decline in O2" in this statement as stated by the reviewer but instead a 10 % increase in the volume of suboxia ($O_2 < 4 mmol\ m^{-3}$) per decade. Denitrification in the model occurs only if O2 drops below this threshold. Therefore, an increase in that volume is naturally expected to lead to an increase in denitrification fluxes. Therefore these anomalies are strongly correlated, but are not necessarily identical in relative terms. This is because denitrification depends also on the amplitude of the organic matter fluxes that enter the suboxic zone, which have not been constant over time as can be seen in NPP trends (Fig 13a).

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.

We will add a short paragraph summarizing what can cause O2 to change (solubility vs. AOU) and that AOU is controlled by ventilation and biology. We will also add a definition of AOU and its link to ventilation as already discussed in our response to previous comment #5.

12. P10 L5: O2 decline vs O2 saturation. How do you ascertain their link? What about AOU at this depth?

O2 declined in the 0-30m layer at a rate of 0.35% per decade. The decline in O2 saturation in the same layer (as can be seen in Fig 8) amounts to 0.25% per decade. Therefore the change in O2 saturation explains around 70% of the total trend in O2 (the reminder is driven by AOU). Please note that O2=O2sat - AOU.

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.

These three panels show slightly different things. (a) and (b) show trends in O2 inventories (O2 vertically integrated) in % per decade in the top 200 m and the 200-1000 m layer, whereas (c) shows trends in O2 concentration (not O2 inventory) in the upper 1000 m across 65◦E (in mmol m^{-3} $decade^{-1}$). (a) and (b) are comparable (trends in O2 inventories) but (c) shows trends in a different variable (O2 concentration) in a different unit (in mmol m^{-3}). As for the discontinuity in colors in (c), it stems from the fact that we show trends computed at each native sigma layer with no vertical interpolation or smoothing.
Finally for more clarity, the % change will be explicitly defined in the caption following the reviewer suggestion.

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?

The volume of hypoxic and suboxic waters is evaluated in the upper 1000m. This will be stated explicitly in the figure caption. In the original manuscript, we integrated the volume horizontally over the entire Arabian Sea. In the revised manuscript, we will show these volumes and denitrification rates integrated over the northern Arabian Sea only for consistency with the focus on the northern AS region (Fig 1 below). However, as can be seen in the figure below, the evolution of the suboxic volume and denitrification is quite similar to when the integration is done for the entire Arabian Sea because suboxia tends to be located mostly in the northern AS (cyan line in Fig 6). However, the positive trend in the hypoxic volume gets stronger and statistically significant ($p<0.05$). We conclude that in the northern Arabian Sea, the OMZ not only gets more intense but also expands. This will be discussed in the revised manuscript.

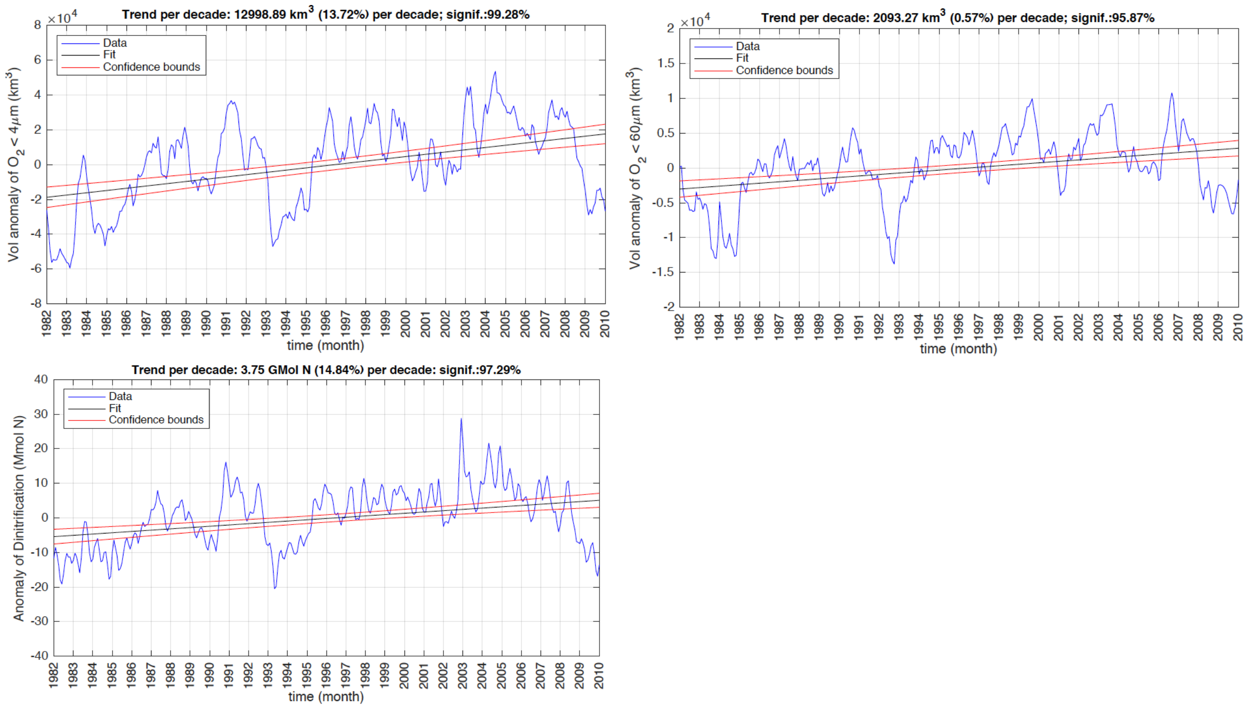

Figure 1: Interannual anomalies in the volume of (top left) suboxia (top right) hypoxia and (bottom) water column denitrification over the study period. The dashed lines indicate the trend lines.

Finally, we do acknowledge that decadal variability is potentially playing an important role in the identified linear trends. However, due to the relatively short simulation period (29 years), it is unfortunately not possible to rigorously filter out that (decadal variability) signal in a statistical sense. We will acknowledge this in the list of the study caveats.

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.

AOU is the difference between O2 and O2sat. We didn't include AOU in the plots because O2 and O2sat are already shown. Please see our responses to previous comments #5 and #12 for more details regarding the computation of O2sat and AOU.

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.

The difference between Fig 9 and Fig S9 is that the ventilation in Fig S9 was not shown on the same scale (the changes in total ventilation are much smaller than the changes in the individual horizontal and vertical components taken separately).
In the revised manuscript we will split total ventilation into advection and vertical mixing components. Please see Fig 16 in our response to comment #24 by referee #1.

17. P14 Section 3.3: The details of sensitivity experiments may be shifted to a new section under methods.

This will be done.

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?

We discuss this in our response to Reviewer 1 comment #3.

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.

The reason we use the repeated normal year approach instead of a monthly climatology (as done routinely to filter out the interannual variability while keeping high-frequency forcing, e.g., Large and Yeager, 2004, Stewart et al., 2020) is that mixing and Ekman circulation are highly sensitive to the high-frequency wind variability (associated with weather systems) as wind stress is estimated using bulk formulas based on wind speed data. As the wind stress is a quadratic function of wind speed, using smoothed wind speed data would result in artificially suppressed Ekman velocities. This approach is commonly used in models using bulk formula forcing. Finally, the choice of 1986 is justified by the fact that this year is neutral with respect to IOD and El Nino/La Nina oscillation. This will be further highlighted in the revised manuscript.

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.

We will show SST trends in all sensitivity experiments in the supplementary information (please see Fig 2 below). This analysis confirms that warming signal is entirely driven by the interannual variability in the forcing.

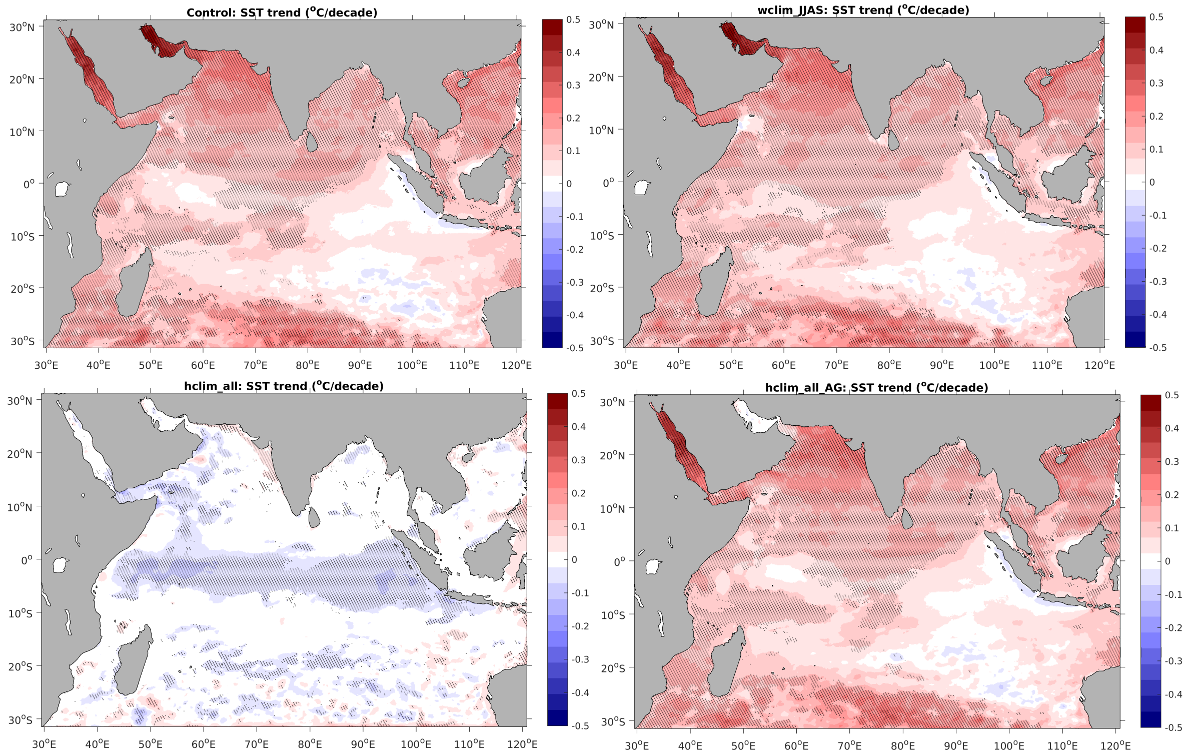

Figure 2: Linear trends in SST (in C per decade) from the control simulation (top left), the no-summer-wind-changes simulation (top right), the no-warming simulation (bottom left), the no-warming-over-the-Gulf (bottom right).

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.

For the decadal variability see our response to previous comment #14.
The region over which the budget is north of 20N. This is indicated in the text but will also be added to the caption. Furthermore, we will show the location of the averaging box in Fig6 of the revised manuscript.

Typos with the panel numbers are fixed.
The terms of the budget will be defined and presented in the Methods section (see our response to previous comment #5).

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.

Summer upwelling intensification can be seen in Fig10b and Fig S10d in the Supp Fig that shows an intensification of upwelling favorable winds off the western AS. We will add references to these two figures in the text. It can also be seen in the positive trends in Ekman suction velocity that we will include in the revised manuscript (see Fig 8 in our response to referee #1).

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.

This will be done.

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.

We will show a reference vector for wind stress trends.
Finally, the apparent inconsistency between the trends in the wind stress vector and the trends in the wind speed is a consequence of the fact that wind stress and wind speed have a non linear relationship, and hence their linear trends are not necessarily proportional.

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.

We would like to note that the trends shown in g and h are trends in total inventory of O2 integrated over the northern AS. They are not computed from spatially averaging the local linear trends in the northern AS as the referee question may suggest. Therefore, trends shown in g and h cannot be directly inferred from visually averaging the local trends shown in other panels (which are expressed in % of local O2 inventory). We will add a note in the caption to clarify this point.

Interchanging Fig 11 with Fig S11: will be done.

26. P19 L9-10: ". . . potentially impacting O2 supply to northern AS". How?

A major pathway of the ventilation of the AS is through the Somali current flowing at depth along the western AS (Rixen et al., 2020). Therefore if O2 decreases there because of an increase in the NPP and hence an increased O2 consumption at depth, one can expect the supply of O2 to the northern AS to diminish as well. We will add a statement to the revised manuscript to clarify this point.

27. P19 L15-16: "the shoaling of thermocline . . . top 200 m". Not really justified. How well thermocline anomalies are correlated with O2 anomalies?

Previous works on Arabian Sea OMZ have shown a strong correlation between thermocline depth and oxycline depth on both seasonal and interannual timescales (e.g., Vallivattathillam et al., 2017). This will be further clarified in the revised manuscript.

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.

While increased productivity in the western AS does not affect O2 concentration in the northern AS directly, it can impact O2 supply from the southern and western AS to the northern AS through the Somali current (please see our response to previous comment #26).
As for the discussion of the role winter monsoon winds, please see our response to General Comment B by referee #1.

29. P19 L33-34: The negative feedback of denitrification is merely speculated and not really demonstrated. Thus, its effect is overstated.

We do not agree with the referee's statement that the denitrification feedback is not demonstrated and just speculation. Fig S19 quantifies this feedback and shows that it is not only statistically significant but also comparable with other biological sources of O2 changes (Fig S17). We will add another panel to the same figure S19 showing the relative contribution of this feedback to O2 trends in the revised manuscript.

30. Figure S10: It is not convincing that there are no strong SST trends in the upwelling region of the western AS during JJAS.

The absence of strong warming (and even local cooling) in the western AS during JJAS is a consequence of the concomitant increase in upwelling favorable winds and upwelling intensification over the study period (Fig S10). Please also see Fig 8 in our response to referee #1 showing the trends in Ekman suction velocity in both seasons and confirming these trends.

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.

This was a typo in the caption of figure S11 as the stratification is shown for the northern AS as mentioned in the text. This will be corrected in the revised manuscript.

In response to the reviewer comment, we will rewrite the first paragraph of section 3.4 to enhance the clarity of the text.

Figs S7 and S8 will be removed and replaced by new evaluation of long-term changes in temperature and salinity (please see our response to General Comment A by referee #1).

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