We would like to deeply thank our colleague Yogesh K. Tiwari for his interest in our work and for his passionate comments and feedback. We highlight our responses to the comments below. Our colleague's comments are shown in italics writing while our response is marked in red.

Authors have used a suit of eddy resolving ocean model simulations to study the decline of O2 in the northern Arabian Sea during recent decades. Authors find that the reduced ventilation caused by the fast warming of ocean surface as the major reason for the decline of O2, with contributions from changes in the summer monsoon winds. Though the topic is of high relevance and the model based study approach is sound, the analysis and presentation are not convincing.
C1 1. The central aspect of this study is reduction in O2 due to reduction in upper ocean ventilation. However, this manuscript do not even offer a basic explanation of the ventilation process and a definition of the ventilation term in the budget calculations. A 2-3 sentence summary of Fig.1 in Oschiles et al. (2018, paper cited in the manuscript) in the introduction will be ideal. Please explain how is ventilation defined in the O2 budget. - Why words like "little", "slight" and "dominates" are used instead of actual values of budget terms while discussing about the O2 budget?

The O2 sources and sinks equations (including the ventilation terms) were described in detail in Lachkar et al (2016) that we refer to here. However, for more clarity we will include them in the revised manuscript.

2. The connection between changes in the upper ocean mixing process and thermocline depth to the change in ventilation is not presented convincingly. How is vertical stratification defined? - A basic discussion of how the increased stratification leads to reduced ventilation is required here. This is a serious omission considering this is the major mechanism used to explain decline in O2.

Vertical stratification at a given depth is given by the vertical density gradient at that depth. This will be defined explicitly in the revised manuscript. We will add a statement to explain that enhanced stratification inhibits vertical mixing and hence reduce ventilation.

- How representative is 20 degree C isotherm as thermocline depth in this region (Fig.12)? Does a temperature gradient/slope based criteria (Fiedler, 2010) offer a better estimate of thermocline depth?

The thermocline depth is typically represented by the depth of isotherm 20C in the region (e.g. Schott et al., 2009).

- What leads to the shoaling of thermocline in the northern AS with summer monsoon wind intensification? - Is there a compensating effect between the surface warming induced vertical stratification and increased vertical mixing from increase in the wind speed (see reference 'c' in item 4 below)? - The impact of increase in stratification on the reduction of ventilation and the resulting reduction in the O2 concentration is not shown quantitatively.

Indeed, there seems to be some partial compensation between the two processes. In the revised manuscript we quantify the impact of changes in vertical mixing on the O2 budget. See our responses to comments by Reviewer #1.

3. Ambiguous or missing description of parameters/features. - What is the vertical resolution of the ocean model for "typical" (say 3000 m depth) water depth in the northern AS? Does it resolve the upper ocean and thermocline depths well?

As stated in the manuscript the model has 32 terrain-following layers with refined resolution near the surface ocean of a few meters on average in top 100m. Therefore, it resolves well the upper ocean physics.

- 58 year Spinup: Which climatological forcing? Was SST and SSS restored? If so to which dataset? - What version of SODA data has been used and what is its resolution? - What frequency was the analyzed model data? Monthly mean? - What is the point in comparing SST and SSS from the model to the same dataset it has been restored to? Is observations used in WOA2013 (salinity) is also used in SODA reanalysis? –

The model spinup is actually made of 58 years forced with a repeated annual cycle (forced with SODA climatology) in addition to four 29-year (1982-2010) repeated cycles. So, in total the spinup phase is 145 year long. We will add more details about the spinup and the SODA version in the revised manuscript.

C2 Fig.2: Drifter measurements are for 15 m depth (examples: Section 1, last paragraph of Lumpkin et al. (2013), Section 2.2 last paragraph in Yu et al. (2019)). What depth are the model fields plotted here? When doing vector plots, please mention at what interval of model grid points (in X and Y directions) the arrows are shown. West India Coastal Current and circulation associated with the Lakshadweep High are very weak in the model compared to that in the drifter data and no explanation has been provided for this. - Fig.3: Large difference (model vs observation) in NO3 at the northern coastal-AS in Winter and at the south-west coast of Indian-Peninsula in Summer are not explained properly.

Velocities are compared between the model and the drifter data at the same level. The goal of this evaluation is to demonstrate the model skill to represent the large-scale patterns of circulation and biology and not to focus on very local details unrelated to the main goal of the study. Finally, model evaluation in the revised manuscript will be expanded to include a more thorough validation of the long-term trends.

- O2 Budget: Please explain the terms included in the budget (grouped as "biology" and "transport") in detail. Is the budget calculated online or offline? How well does the budget balance? - Layers of upper 200 m and below 200 m (Fig.6,7,8 and throughout the manuscript). How can authors justify analyzing data for a selected depth (like 100 m) to describe the properties in a "layer" (like 0-200m, Fig.6a)?. Isn't is appropriate to use a layer averaged (like in Fig.4) or integrated fields (like in Fig.9) instead? Please clearly mention how the data is processed in each of the figures mentioned above and justify the choice.

The O2 sources and sinks equations (including the ventilation terms) were described in detail in Lachkar et al (2016) that we refer to here. However, for more clarity we will include them in the revised manuscript. The O2 budgets are shown for two layers (100-200m and 200-1000m) and not at a particular depth as the comment suggests.

What makes the 200 m depth as a layer separation depth? - Fig.6 panels a and b shows trends in percentage/decade but panel c shows actual values in nmol/m3/decade. It is very difficult to understand the context without visualizing the spatial pattern of actual trend in nmol/m3/decade. - Fig.7: How is the anomaly defined? Difference between monthly-varying model data, and mean of the time series?

The 200 m depth is usually used to separate the epipelagic zone (0-200m) and the mesopelagic zone (200-1000m) and also corresponds roughly to the depth of thermocline in the region. Finally, interannual anomalies are computed by deseasonalizing the data.

According to Fig.4 and definition, hypoxic (O_2 < 60 nmol/m3) region is bigger than suboxic (O_2 < 4 nmol/m3) region in an average (250- 700 m) sense. So, naturally one expect the anomaly in hypoxic should be higher than that in the suboxic case but it is not true according to Fig.7 panels a and b.

Just because the volume of hypoxia is larger than that of suboxia does not imply it should increase more. Actually, it is the opposite that happens (in % terms) as the suboxic waters are mostly concentrated in the northern Arabian Sea where deoxygenation is strongest, whereas hypoxic waters occupy a larger area that experience both oxygenation and deoxygenation trends.

Authors, explain that the small patches of oxygenation in the eastern/central/southern region can make the volume of hypoxic region nearly constant compared to that of suboxic region to the north. But this is very difficult to comprehend from the percentage based trend shown in the panels 6a and 6b. Using percentage trend at two representative depth to explain the anomaly (in actual value/units) over a volume of water is very difficult for a reader to follow and interpret. Why not use the vertically integrated O_2 for 0-200 m range and 200-700 m range in Fig.6.

We will add the absolute changes in O_2 inventories in the revised manuscript (in the SI).

- Sensitivity Experiments: Did any smoothing has been applied while modifying the forcing for a particular region (eg. Gulf) or time (eg. wclim_JJAS). If not, how strong was the impact of sudden jumps in the forcing field resulted from these modifications on the model solution? Labelling of experiments with 1986 fields as "clim" is misleading, instead use either "1986" or "normal" to indicate it is from a specific year. Also explicitly mention what is meant by the "control" run. - SST Warming: What is the mechanism behind widespread SST warming in the AS? Just citing few past studies is not sufficient since this is one of the core process which contributes to the O_2 reduction. Please summarize major reasons for this warming where it is first discussed.

Temporal interpolation is applied to reduce the discontinuity in time in the wclim_JJAS and the new wclim_DJFM. As for the no-Gulf-warming simulation, only forcing SST is modified in the Gulf.
Regarding the repeated normal year approach, we refer to it as climatological because 1) the forcing is repeated every year, so it has no interannual variability and 2) because it is based on a normal year (neutral with respect to major climate variability modes).
As for what is causing the warming in the Arabian Sea, this is far beyond the scope of our study. We prefer to keep the paper focused on our key question: deoxygenation, instead of going in all directions.

4. Missing citations of relevant literature. - Some of the recent studies which are highly relevant for the topics discussed/addressed in this manuscript (a: changes in monsoon winds, b: oceanic impacts of changes in monsoon winds, c:ocean mixing energetics of changes in monsoon winds, d&e: Oxygen minimum zone in the northern AS) are not cited.

We thank our colleague for compiling this selection of papers. We will cite relevant ones.

a) Sandeep, S., and Ajayamohan, R.S., 2015: Poleward shift in Indian summer monsoon low level jetstream under global warming, Clim. Dyn. 45, 337-351; doi:10.1007/s00382-014-2261-y b) Praveen, V., Ajayamohan, R.S., and Valsala, V., 2016: Intensification of upwelling along Oman coast in a warming scenario, Geophys. Res. Lett., doi:10.1002/2016GL069638 c) Praveen, V., Valsala, V., Ajayamohan, R.S., and Balasubramanian, S., 2020: Oceanic mixing over northern Arabian Sea in a warming scenario: Tug of war between wind and buoyancy forces, J. Phys. Oceanogr., 50(4), doi:10.1175/JPO-D-19-0173.1 d) Shenoy, D. M. et al., 2020: Variability of dissolved oxygen in the Arabian Sea Oxygen Minimum Zone and its driving mechanisms, J. Marine Sys., 204, https://doi.org/10.1016/j.jmarsys.2020.103310 C4 e) Sarma et al 2020: Potential mechanisms responsible for occurrence of core oxygen minimum zone in the north-eastern Arabian Sea, Deep Sea Res. PartI, 165, https://doi.org/10.1016/j.dsr.2020.103393 - Add a citation or a statement in the acknowledgements for the SODA data (as shown in the link given below) https://climatedataguide.ucar.edu/climate-data/soda-simpleocean-data-assimilation Carton, J.A. and B. Giese, 2008: A Reanalysis of Ocean Climate Using Simple Ocean Data Assimilation (SODA). Mon. Weath. Rev. , 136, 2999-3017. As detailed above, this manuscript needs to be rewritten focusing on the details of analysis and presentation in order to make it a publishable one. References: ————- Fiedler, P., 2010: Comparison of objective descriptions of the thermocline, Limnology and Oceanogr., https://doi.org/10.4319/lom.2010.8.313 Lumpkin, R., Grodsky, S. A., Centurioni, L., Rio M-H, Carton, J. A., and Lee D., 2013: Removing spurious low-frequency variability in drifter velocities. J. Atmos. Oceanic Technol., 30(2), 353-360, https://doi.org/10.1175/JTECH-D-12-00139.1 Yu, X., Ponte, A. L., Elipot, S., Menemenlis, D., Zaron, E. D., and Abernathey D, 2019: Surface kinetic energy distributions in the global oceans from high-resolution numerical models and surface drifter observations, Geophys. Res. Letters, 46(16), 9757-9766, https://doi.org/10.1029/2019GL083074