General Response

We thank the three reviewers for their constructive and insightful comments. The main concerns among the reviewers fall into 3 categories. We give a brief overview of how we propose to address them and include the proposed new figures here in the general response, and then provide a detailed response below.

1. **Role of changes in stratification and productivity in dissolved oxygen projections.** The reviewers ask for an expanded investigation of potential changes in upper ocean stratification and the biological sink on oxygen changes. Specifically, increased stratification could contribute to deoxygenation in the northern Arabian Sea, and decreased productivity and respiration rates could contribute to oxygenation in the South Equatorial Current (SEC). We propose to add a subsection to our *Results* in which we examine trends in upper ocean stratification (Figure GAC1) and the export of organic carbon at 100 m (Figure GAC2). Both the northern Arabian Sea and SEC regions are projected to experience increases in stratification between 100 - 200 m, as well as decreases in export at 100 m, with opposing effects on oxygen. The spatial correlation of these two trend fields is expected, given the relationship between stratification and nutrient supply via vertical mixing. This suggests that the effects of stratification and export trends, at least, partially compensate each other in both of these regions. We propose to discuss this compensation and how it is consistent with the results of our sections 3.5 and 3.6 (using Optimum Multiparameter (OMP) analysis) which suggests that changes in circulation and water mass fractions related to lateral advective pathways can explain nearly all simulated oxygen changes (direct calculations of mixing-driven oxygen changes will be added in the revised manuscript; Figure GAC3). We have also performed additional sensitivity tests with the OMP method (see new Figure GAC3 and new Method details at end of general response) and propose to add a discussion of the robustness and limitation of the OMP method, and the implications for our results (to be added to *Discussion* section 4.4).
2. **Clarification of approach.** One reviewer is concerned with the definition of Oxygen Minimum Zone (OMZ) used in the manuscript which is a continuum covering a broad range of oxygen values extending beyond the commonly used hypoxic thresholds (e.g., 60 umol/kg). We propose to clarify in the *Methods* section 2.2.1 and throughout the text the rationale for using this broad definition: (1) The relevant thresholds for 'low oxygen' environments can vary significantly depending on the application, such as (in order of increasing threshold) denitrification, mortality of marine organisms, and sublethal stresses on marine organisms. Specifically, thresholds of ~150 umol/kg (and even as high as 200 umol/kg) are commonly used as habitat boundaries for large commercial fish species (eg Vaquer-Sunyer and Duarte, 2008); (2) Using a broad continuum of OMZ thresholds enables us to interpret changes in oxygen volume (including the hypoxic volume) within the context of larger-scale forced changes (contraction, redistribution, expansion).
3. **Robustness of findings across individual Earth System Models (ESM)**. The reviewers note that much of the analysis presented in the manuscript is based on the presentation of Multi-Model Means (MMM) of the ESM ensemble, which may mask

compensating biases and different dynamical responses across models and may be sensitive to model selection. We propose to a) expand the discussion of the maps/sections of individual ESMs already included in the Supplementary Information (SI) of the original manuscript, b) add individual ESMs when presenting OMZ volume and OMZ volume trends in the main text (new Figures GAC4, GAC5, GAC6), and c) compare the effects of water mass fraction changes (from OMP analysis) across individual models (Figure GAC3). Overall, we find that most models exhibit the same regional pattern of oxygen trends and regimes of OMZ volume trends as the MMM, with varying amplitudes of features across models. This will also be discussed in Discussion section 4.4.

We are confident that these revisions will address the concerns of the reviewers. Below, we provide specific details and figures for these revisions, and we address additional reviewer comments.

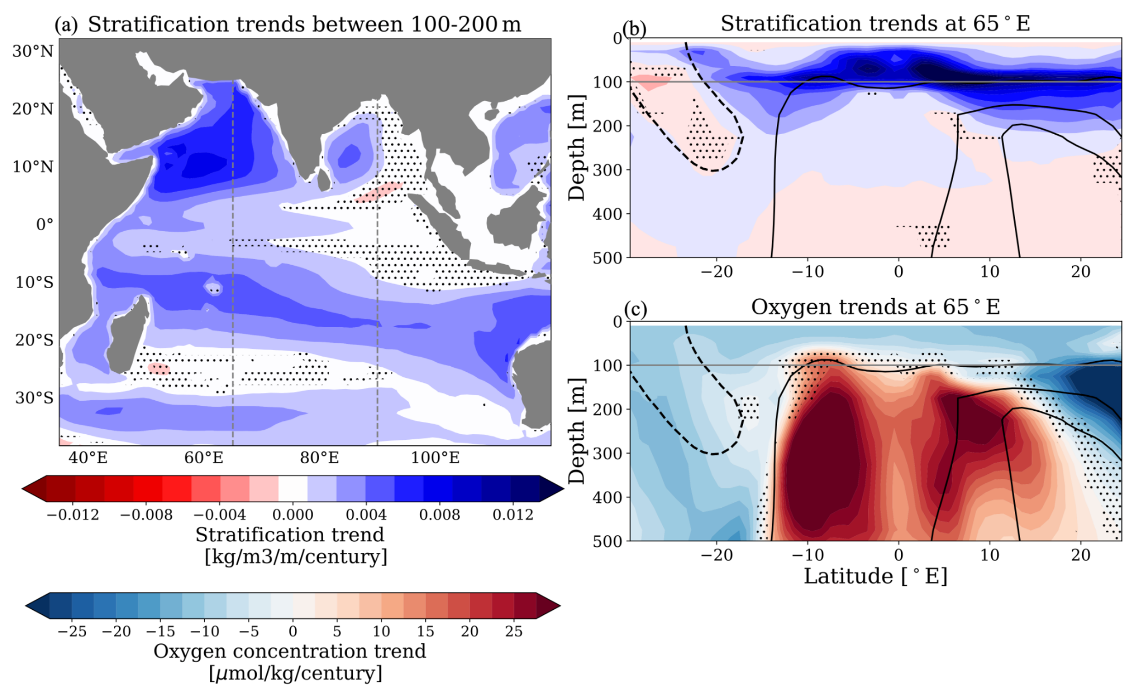

Figure GAC (General Author Comment) 1: Multi-model mean (MMM) stratification trends under SSP5-8.5 scenario forcing (1915-2100). Stratification trends (a) between 100 and 200m, and (b) at 65E. (c) dissolved oxygen trends at 65E. (b,c) Solid black contours represent 20, 60 and 150 µmol/kg oxygen. Dashed black contours highlight salinity signature of Subtropical Underwater (STUW). Results are stippled where less than 75% (6/8) of models agree on sign of trend.

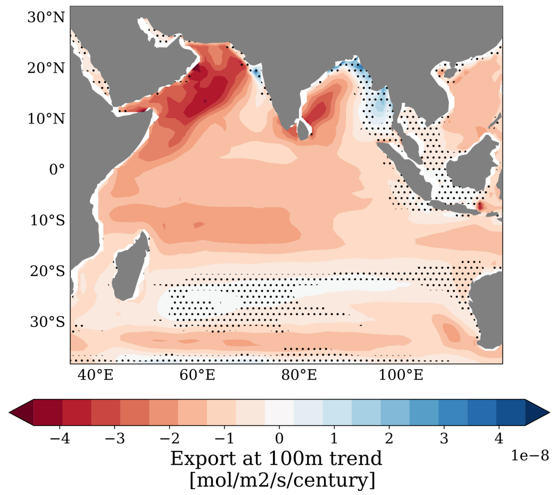

Figure GAC2: Multi-model mean (MMM) export of organic carbon at 100 m trends under SSP5-8.5 scenario forcing (1915-2100). Results are stippled where less than 75% (6/7) of models agree on sign of trend.

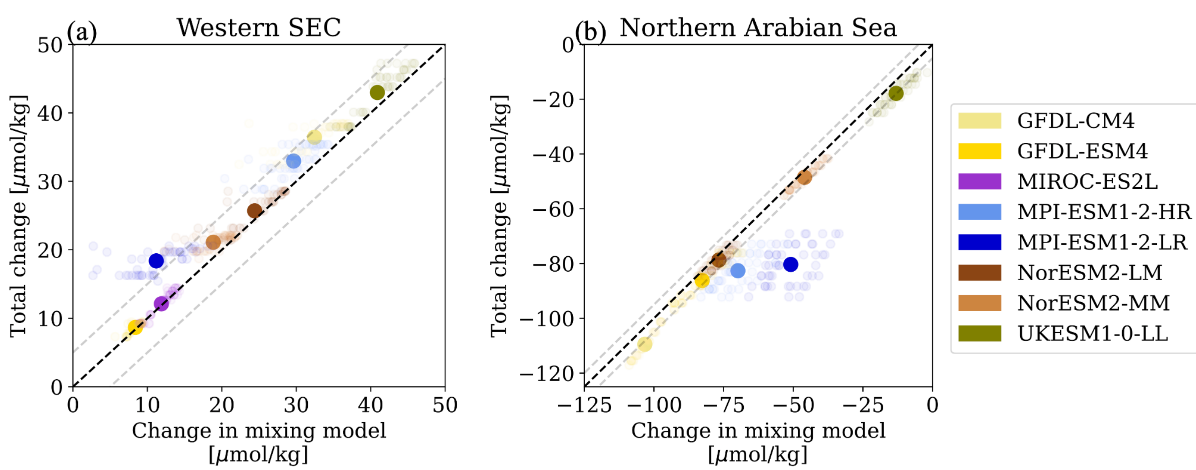

Figure GAC3: Changes in dissolved oxygen accounted for by mixing model analysis. Total oxygen changes (simulated by ESMs) versus oxygen changes attributed to shifts in water mass composition in the OMP mixing mode for the (a) western South Equatorial Current (SEC) and (b) northern Arabian Sea. Results in the western SEC averaged between 40-80E, 5-15S. Results in the northern Arabian Sea averaged between 62-70E, 20-26N. Solid markers are the mean of 50 runs (light markers) of the OMP analysis with perturbed source water properties (See end of general response for details on new methods). Black dashed line marks 1-to-1 relationship, gray dashed lines mark 5 umol/kg deviations from 1-to-1.

New method to be added to Optimum Multiparameter (OMP, section 2.2.4) analysis for sensitivity analysis and used to produce Figure GAC3 above:

“From the mixing model results, we quantify the change in oxygen accounted for by shifts in water mass composition for key regions: $\Delta O_{mixing} = \sum_i (f_{i,1}O_{i,1} - f_{i,0}O_{i,0})$, where f_i and O_i are the fraction and oxygen concentration of source water type i, for future (1) and historical (0) states. Oxygen is allowed to be remineralized, so ΔO_{mixing} need not approximate the total simulated change in oxygen. To test the sensitivity of the mixing model results to small changes in the definition of source water types and potential density layer, we average 50 realizations of the experiment applying random perturbations to source water locations and density values. Source water locations are perturbed by up to 5 degrees in latitude and longitude for the South Equatorial Current region and 2 degrees for the Arabian Sea. For both locations, the value of the potential density layer is perturbed by up to 0.1.”

We note regarding the OMP analysis that we lowered the weight on oxygen in the optimization compared to the submitted manuscript (lowered from 20% the weight of temperature and salinity to 2%) to more appropriately account for the range of oxygen values in the analysis compared to the range and temperature and salinities (see Appendix A of submitted manuscript for technical details of OMP).

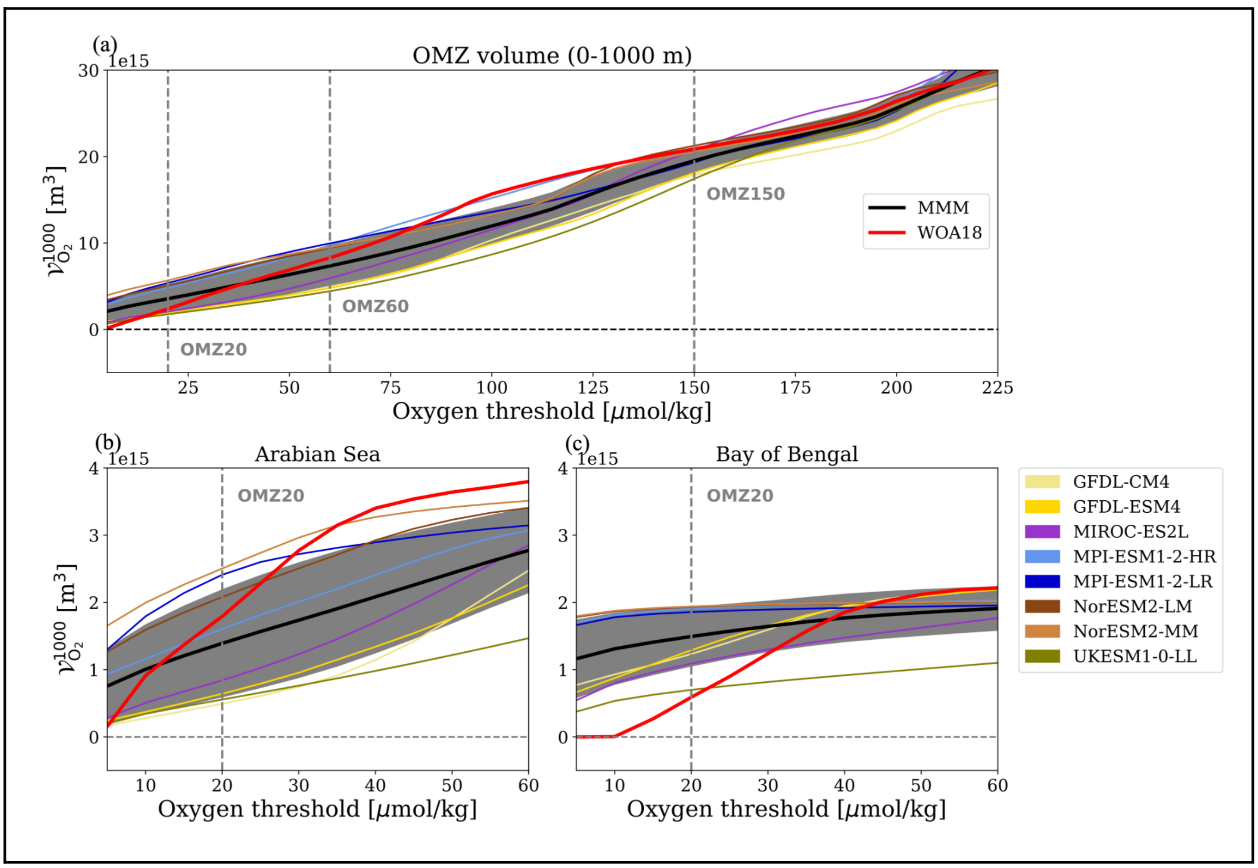

Figure GAC4: (**To replace Figure 2 in main text**) OMZ volume taken between 0-1000 m in the (a) Indian Ocean and sub-basins (b) Arabian Sea and (c) Bay of Bengal for the multi-model mean averaged over 1950-2015 (MMM; black) and observed climatology (WOA18; red). Shading represents one standard deviation of the model spread. Individual ESMs shown in colored curves. Southern boundary of Arabian Sea and Bay of Bengal are at 5N.

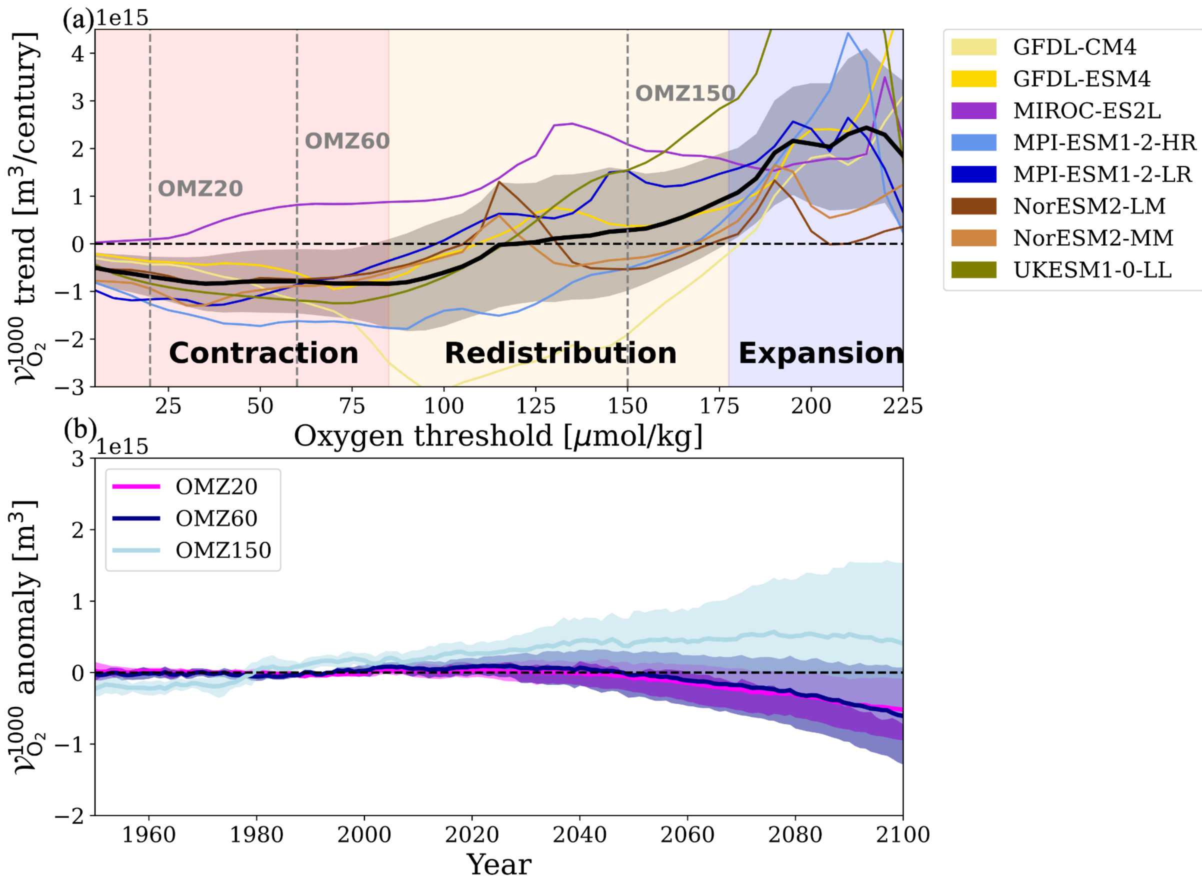

Figure GAC5: (**To replace Figure 3 in main text**) Multi-model mean thermocline OMZ volume changes (between 0 - 1000 m) under SSP5-8.5 scenario forcing (2015-2100). (a) V_{o2}^{1000} trends as a function of oxygen threshold for the Indian Ocean. The 20, 60 and 150 umol/kg thresholds bounding OMZ20, OMZ60 and OMZ150 are indicated with grey dashed lines. (b) Time series of V_{o2}^{1000} anomaly from 1950-2100 (anomaly referenced to 1950-2015 mean) for OMZ60 (dark blue) and OMZ150 (teal). Shading represents one standard deviation of model spread.

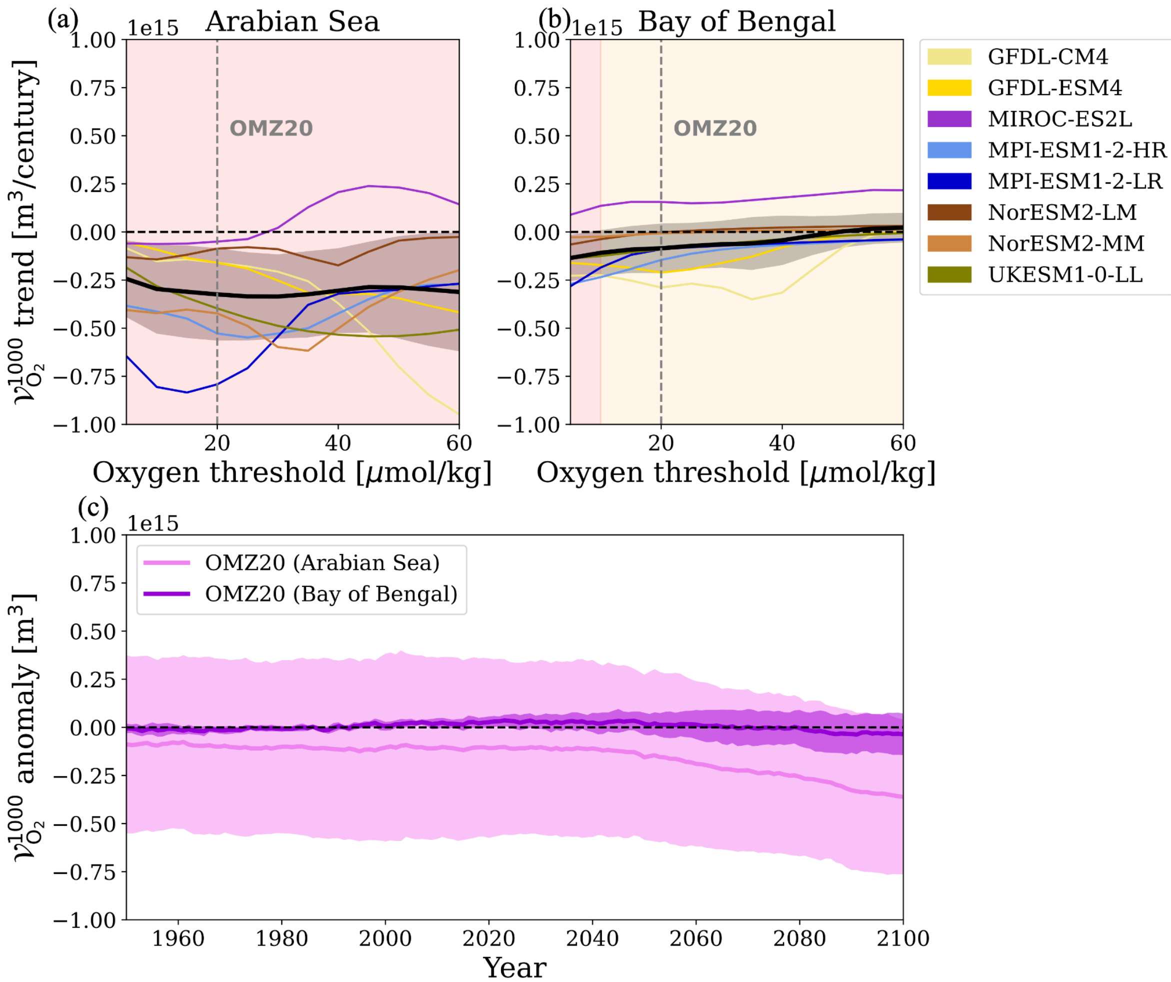

Figure GAC6: (**To be added to supplementary information**) Multi-model mean thermocline OMZ volume changes (between 0 - 1000 m) under SSP5-8.5 scenario forcing (2015-2100). V_{o2}^{1000} trends as a function of oxygen threshold for the (a) Arabian Sea and (b) Bay of Bengal with OMZ20 volumes indicated for each basin. (c) Time series of V_{o2}^{1000} anomaly from 1950-2100 for OMZ20 volumes in the Arabian Sea and Bay of Bengal. Shading represents one standard deviation of model spread.

Reviewer 3:

I very much enjoyed reading this comprehensively written and well structured manuscript by Sam Ditkovsky et al. What is presented is an analysis of the general ventilation pathways in the Indian Ocean and which are “tailored” to the specifics of oxygen, including OMZ regions, putting an emphasis on oxygen concentration thresholds. On the whole, I agree with the approaches, and I like the analysis presented. However, some things are unclear to me and I would like the authors to comment on them. The analysis focusses on section/surfaces – what has been ignored in the study, but I assume is highly relevant when it comes to understanding the drivers of oxygen variability, is the physics of ventilation in particular in the southern source regions. In these regions a coexistence of stratified Central Waters (Ekman pumping driven subduction) with lateral flux dominated Mode waters exists (e.g. apparent in the global assessment of Hanawa & Talley, 2001, or specifically addressed in the Karstensen & Tomczak 1998 paper or). Given the specific role of Mode Waters in thermocline ventilation (in the Indian Ocean formed only in the southern hemisphere). I assume not addressing Mode Water and not contrasting them to Central Water may prompt wrong conclusions on the drivers/sources of variability – also for the Indian Ocean as a whole (because these water masses are a major source) . Can you show that this distinction between Mode and Central water does not matter?

Thank you for the thoughtful comment. As shown in Karstensen & Tomczak (1998), despite having distinct formation mechanisms, Indian Central Waters and Subantarctic Mode Waters have similar temperature and salinity signatures, and thus occupy the same density layer. That study finds that pure Central Waters only exist at midlatitudes, suggesting that by the time these waters reach the subtropical Indian Ocean thermocline (covered in our study), they are mixed together, and together they form the thermocline oxygen maximum layer. Thus we treat them as a mixed water mass of ‘Central and Mode Waters’ in the context of our study. Since we have no reliable way of distinguishing changes in Central Waters versus Mode Waters, we must remain agnostic as to whether the weakened thermocline ventilation in the CMIP6 experiments derives from changes in Ekman pumping or changes in Mode Water formation. We will clarify this in section 4.4 where we discuss the slowdown of southern source waters (line 434).

Within this context I wonder if analyzing the “southern source” by integrating over a vertical section at 30S only is maybe too simple? – for example the winter outcrop (determines area of permanent subduction) is not strictly zonal and thus the full ventilation signal is not accounted for by making use of a zonal section. This may get worse in ESM’s that, with climate trends may show shifts in outcrop density change/trend over time? Have you considered that?

Indeed, we need to be careful in interpreting transport changes across 30S. A zonal section at 30S should comprehensively capture the northward transport of Central Waters and Subantarctic Mode Waters, as well as Antarctic Intermediate Waters, into the Indian Ocean. However, this section will miss the majority of Subtropical Underwater, which is mostly subducted north of 30S. We propose to add a comment on this in methods section 2.2.3. Additionally, any discussion of the transport across 30S in the revised manuscript will reflect its incompleteness of capturing the southern source waters.

A possible consequence of this is that the transport timeseries presented in Figure 7 (submitted manuscript) is incomplete. In particular, the ratio of Southern Pathway to ITF transport should likely increase more than is represented. This may be connected to a point brought up by Reviewer 1 and addressed in Figure R1AC3, which shows that the oxygen supply from the Southern Pathway and ITF collectively does not increase, given how we represent the pathways. To avoid confusion here, we remove Figure 7b from the text.

Also, I wonder if the AOU/ideal age comparison isn't a bit too simple to deduce biology from it? (Line 150 etc.).
This is because AOU is a property that reflects in my view three processes:
1) respiration (biology, depth and eventually region depending)
2) mixing of water properties in the interior
3) mixing of the imprint of the saturation value in the respective outcrop region of the various water masses
To explain what I mean - let's ignore 1) and just look at combinations of 2) & 3). For simplicity assume two water masses are 3 years old and have similar TS in their formation region – a mixing of the two would be invisible and the ideal age equals the oxygen propagation time (3 years). For this case all assumption are OK. However, if two water masses still are 3 years old but start with different TS and thus concentrations (saturation) in their formation region a mixing signal is seen in AOU but no signal in "ideal age" (this will be still 3 years). This change in AOU now is interpreted as a residual and thus "biology" (1)) despite the fact that in this thinking experiment no biology (1)) was considered. Given the complexity of water masses in the Indian Ocean and the wide range of oxygen concentrations and therefore oxygen gradients and therefore oxygen fluxes, the above process I assume may be significant and messes up the correlation and deduction of biology and driver of variability?

This is a good point, and the original language in lines 155-156 was misleading. In our comparison of AOU and ideal age (Figures 5,6), we only interpret an agreement between AOU and ideal age changes as a suggestion of ventilation changes (rather than interpreting any disagreement as biology changes). To avoid confusion, we plan to remove the Pearson Correlation methods and Figure 6 panels d,e,f from the revised manuscript. See our General Response #1 for details on proposed additions to the manuscript which will address the role of changes in the biological sink.

Line 172: note that for an “Optimum” Multiparameter Analysis, at least one parameter more than the number of source water types is needed. This is because the term “Optimum” refers to the “non-negative” constrain which takes away on degree of freedom (see Lawson and Henson algorithm mentioned in Tomczak & Large). Is that a problem for your case? (>3 source water types?

There are perhaps two components here. The first is the allowed number of source water types relative to constraints/parameters. In the application of water masses, conservation of mass adds an additional constraint (eg Tomczak 1981, Prog. Ocean.), so one can actually have up to *n* source water types for *n* non-mass parameters. In our study, we use as parameters potential temperature, salinity, oxygen, and AOU (and mass). One can argue whether or not AOU should count as an independent parameter here, but we will take the conservative stance of claiming to use only 3 parameters (T, S, O2) + mass, allowing for 3 source water types.

The second component here is whether 3 source water types is sufficient for describing the Indian Ocean. We found that each of our evaluation regions (the tropical Indian Ocean and the Arabian Sea) could be well approximated by just 3 source water types. This relies somewhat on the assumption that the system can be explained to first order only by isopycnal transport/mixing. For example, rather than capturing the full linear relationship of the Southern Pathway source waters by using both Antarctic Intermediate Water and Subtropical Underwater, we use a single southern source sampled at the same potential density on which we evaluate the water mass composition of the tropical Indian Ocean.

Line 224: is it known why ESMs simulate higher oxygen levels? Could it be that shallow subduction occurs in the Arabian Sea in the models? (the Ekman pumping during monsoon would support that).
A study by Schmidt et al. (2021, Ocean Science) investigated the Arabian Sea oxygen bias for CMIP5 models (and CMIP6 models likely experience similar issues). They attribute the model biases primarily to excessive ventilation from southern source waters and the Red Sea and Persian Gulf outflows. We find that this explanation is consistent with what we see in the CMIP6 ESMs.

Figure 5: you indicate the expansion of water masses by dashed line. How can I envision this expansion? Say AAIW is defined by the salinity minimum – what does expansion mean? A density range? The 95% contour of AAIW content?

The dashed contours in Figure 5 simply outline the historical locations of AAIW and STUW cores, defined using approximate salinity minimum and maximum values, respectively. We do not suggest in this figure how these salinity features evolve over time, only the evolution of oxygen within and around these salinity features. We will clarify in the caption that these contours represent historical locations of the water masses, not an evolution.

Line 327-329 or Line 353-355: you report about the increase/decrease of water masses overtime. Operating with changing source water mass characteristics is a challenge in water mass mixing analysis – simply because, from a mixing model point of view, each changing source water is introducing an additional water mass to the ocean interior while the water with the former characteristics still exist and contribute to the mixing. How do you deal with that? (maybe I overlooked it but do you list the source waters somewhere?)

We agree that this is a fundamental challenge of the analysis, with perhaps no perfect solution. However, we try to mitigate this effect here by solving the mixing model for climatological fields, rather than evolving fields. In this case, we use 1950-2015 as a historical average. We then compute a climatological year 2100 by perturbing the historical mean using linear trends over the SSP5 experiment.

In the revised manuscript, we propose to present the composite of many runs of the OMP analysis with small perturbations to source water type properties (detailed in the Appendix to the General Response). The results show little sensitivity to these perturbations. While this does not capture the evolution of source water properties in time, it does provide an estimate of the sensitivity of the analysis to small changes in source water properties.

Line 420: An additional fact on the Atlantic OMZ ventilation is the significant source of South Atlantic source waters on ventilating the North Atlantic OMZ (e.g. evident in the TS properties but also from subduction estimates). You may want to also consider this in your "first glance on Atlantic" discussions.

Thank you for the suggestion. Some previous modeling studies suggest that subduction rates of Mode and Intermediate Waters from the South Atlantic hold steady in ESM projections, but that they ventilate at lighter densities (eg Goes et al., 2008, Downes et al., 2009). Given that the Atlantic OMZ is relatively shallow, and that competing pathways (ie deep waters) are likely to slow, this seems to be a plausible hypothesis. We propose to add this to our discussion in section 4.3.