

The authors would like to thank anonymous reviewer #3 for the constructive and valuable comments, which will help to improve the manuscript. A point-by-point reply to the comments follows below. The author responses are marked in red.

Review of bg-2020-82 - Present past and future of the OMZ in the northern Indian Ocean

This review brings a timely update on the state of the knowledge on the OMZ in the Indian Ocean. The value of this review is to bring together a wide range of disciplines covering the influence of bio-physical coupling, insights from paleo-oceanography, pelagic and benthic ecosystems, leveraging present and paleo observations, and models (from early Holocene to future). This is a very valuable exercise for the community. The authors do need to address several issues before it is acceptable for publication.

The authors would like to thank anonymous reviewer #3 for this comment

Specifically, the authors need to clarify how they discuss the balance between biology and circulation throughout the text. The claim that large-scale circulation control long-term changes in the OMZ in the Arabian Sea rather than local changes in biological demand is made several times in the paper (see comment #6, 11, 14 and 21). The authors show data and model suggesting a decline in oxygen during the Holocene. However, the claim that it is due to large scale circulation is a hypothesis. The authors do not show the contribution from physical and biological controls in the model. Options to address this include: i) show the simulated integrated biological production and/or export production (it is usually an output in models), ventilation age would be tremendous but might not be available in the model. If export and biological production do not change, this would substantiate the claim that ocean circulation is controlling the change; ii) use the results from Bopp et al 2017 which show ventilation changes in another model in the Indian Ocean (see comments # 14-15). In any case, the language must be changed throughout the text.

Simulated export production which are constant throughout the Holocene and water mass age in the Arabian Sea have been discussed for an earlier model experiment with the same model setup (but accelerated forcing) in Gaye et al. (2017) and in more detail for the global OMZs including the Indian Ocean for the model experiment analyzed here in Segschneider et al. 2018 (which also discusses the results of Bopp et al. 2017). We will point more prominently to the respective sections and figures in Gaye et al. 2017 and Segschneider et al. 2018.

There are some misleading points that need to be addressed in the abstract and introduction. I also strongly encourage the authors to strengthen sections 3, 4 and 6 (see comments #4-11, 16, 17). Comments on section 2 and 7 are mostly on the form. Finally, please read the paper carefully and double check grammar and spelling. Introduction, Section 3 and conclusion need special attention. We agree that a restructuring and unified writing style would improve the flow and make our view and arguments more obvious. The referees provided extremely constructive and detailed suggestions of how to restructure the manuscript and the native speakers among the authors will unify the writing style.

Detailed major comments

In the following we respond to content issues and with short statements to comments and suggestions regarding grammar, spelling, and unclear explanations.

1. Abstract and introduction. **Abstract will be rewritten**
 - **L18-19**: “OMZ in AS and BoB intensified and expanded.”. This is misleading the readers in the abstract. The main text suggests a much more subtle response with regions of expansion and regions of reduction (section 2). Please clarify.
 - **L 3 and L30**. Mentioning methane is very misleading as this would apply to terrestrial ecosystem but not so much to oceanic systems, which are the focus of this review. Please remove. You might consider mentioning N₂O instead, which is number 3 in the list of GHG.

2. **Section 2.3**- This section on trends in BoB and AS is key to the review and the community. It would benefit some streamlining, specifically rephrase and make clear when the text refers to observed trends vs. when it discusses implications and more general concepts of these trends (e.g. threshold for nitrite oxidation etc.).
This will be done.

3. **In Section 3.1**, the text reads: “data ... implies that the respiration ... causes the low oxygen concentrations in the Arabian Sea ... satellite-derived export production rates were much too low to sustain such a high biological oxygen consumption ... The mismatch between oxygen deficits and the biological consumption reflects uncertainties caused by the poorly constrained physical oxygen supply and export production rates.”
This section will be restructured.

This section should discuss model results mentioned later in the manuscript (e.g. Resplandy et al 2011, 2012; Lachkar et al.), which managed to maintain the OMZ in the Arabian Sea at a quasi steady-state over decadal time-scales. Looking at their balance between biological demand and physical supply would inform how this balance is achieved. Comparing these numbers to the estimates mentioned by the authors would bring valuable information on the “mismatch” and how models manage to achieve the balance (it does not mean the models are right but it is still valuable). Do the models simulate higher productivity than satellite based estimates? Do they have higher ventilation? These papers include information that can be used for this discussion (PP, oxygen physical supply, biological consumption etc.).

This is a very interesting aspect, which will be included.

4. **Section 3.1** discusses the Arabian Sea. What about the Bay of Bengal?
A discussion about the Bay of Bengal will be added.

5. **Sections 3.2**. The points of this section are not well presented I was struggling to guess the links the authors want to make between seasonal thermocline, SNM to ballast effect, zooplankton migration etc.. Please streamline and clarify the following points.
This section will be restructured.
 - L253: “In contrast to the BoB, nitrite accumulates in the seasonal thermocline of the Arabian Sea”. Link to rest of paragraph is unclear. Clarify the link with export production. Again this sentence probably belongs to the next paragraph.
 - L267-271: this is not comprehensible. Please clarify grammar and meaning.
 - L285-290: facts about the collocation of remineralization, zooplankton migration and upwelling source waters but unclear what the implications are. Please explain and clarify how this relates to the main point here.

6. **Section 3.3 L307-310:** “suggesting that physical supply rather than biological demand are drivers controlling the intensity of the OMZ.” This sentence needs clarifying. Without biological demand there is no OMZ, and the OMZ can be considered at “quasi steady-state” $BIO + PHY \sim 0$ [of course there are small trends but the OMZ has been relatively stable on decadal and century time-scales]. Maybe what the authors mean is that temporal variations in the intensity of the OMZ are controlled by physical supply? Clarify and specify what time-scale you are talking about (seasonal only? Decadal, centennial etc?).

Yes, we will clarify this point.

7. **Section 4. L312-323. Intro on eddies.** You mention the role of eddies on biological production and oxygen mixing but could add the influence on export. The literature has progressed a lot since Oschlies et al 1998 and “eddy pumping” is not considered as the only mechanisms at work anymore. Relevant publications for biological production are reviews by McGillicuddy 2016 (mesoscale eddies) and Mahadevan 2016 (submesoscale, includes Arabian Sea example) and refs therein. For eddy-driven export production: Omand et al 2015 and Resplandy et al 2019 (eddy-driven export), Boyd et al 2019 (all export pathways including eddy-driven). On the role of eddies in oxygen mixing, I would also consider adding Bahl et al 2019.

These references will be included into the discussion.

8. **Section 4. L330:** “due to the semiannual reversal of the mean circulation and a resulting reduced oxygen supply”. It is not clear how this fits in the sentence. Eddies enhance the oxygen supply to the OMZ, while the mean circulation partly offsets this supply by eddies. Please clarify text.

This statement refers to the fact that according to Resplandy et al (2012) the Ekman pumping contribution to O₂ supply is nearly of comparable magnitude but of opposite sign between the summer and winter monsoon seasons, which hence leads to these effects compensating each other on an annual timescale, resulting in a weak contribution of the mean circulation to vertical oxygen supply, thus dominated by mesoscale eddies (on an annual timescale).

For more clarity, we will change that statement to: “Furthermore, mesoscale eddies and filaments were shown to dominate, on an annual timescale, the vertical supply of oxygen to the OMZ in the Arabian Sea (Resplandy et al., 2012). This study also showed that eddy-driven horizontal advection substantially contributes to the lateral transport of ventilated waters into the central and northern Arabian Sea.”

9. **Section 4. L336:** note that both the work of McCreary and Resplandy suggest that “this mechanism strongly contributes to the eastward shift...”. As the authors pointed out earlier in this paragraph eddy-driven ventilation supplies oxygen to the western Arabian Sea in both studies. Please rephrase so it is clear that both studies converge here.

We agree with the referee. For more clarity we will change that statement to:

“In a process study aiming to explore the dynamics of the Indian Ocean OMZs, McCreary et al (2013) also highlighted the important role of vertical eddy mixing in the ventilation of the western Arabian Sea in addition to the inflow of ICW. Their work further stresses the importance of this mechanism in the eastward shift of the upper OMZ relative to the region of highest productivity located along the western part of the Arabian Sea.”

10. **Section 4. L374-376:** Does the Chen et al paper mentions a decline in eddy activity? This should be clarified. If it is interannual variability and not a long term decline, then you would expect interannual variability in the OMZ ventilation and denitrification but not necessarily a deoxygenation.

We agree with the referee. We will change this statement to:

“In the Bay of Bengal, strong interannual variations in the intensity of the eddy activity have been reported (Chen et al., 2012). These are expected to cause strong variations in the subsurface ventilation that may eventually lead to episodic suboxia and onset of denitrification at the core of the OMZ (Johnson et al., 2019).”

11. **Section 4. L385-389.** The links here are not clear. The bio/eddy-driven ventilation balance identified in present day models does not suggest that remotely forced changes in physical supply cause long-term changes. The supply of oxygen to the OMZ has to be through mixing and is promoted by eddy-driven circulation, because there are no direct advective pathways into the OMZ shadow zone (by definition). The authors are right however that large scale circulation is important because it regulates the oxygen gradients at the OMZ edges. However, I don't see why Holocene changes could not be tied to changes in biological demand? I would remove these sentences here and keep this discussion for the Holocene section 5 (see comment #14).

Following the referee suggestion, this statement will be removed.

12. **Section 5 L425-430** should point to Figure 6 to help reader follow. I suggest the authors slightly reorganize the text between L425 and 449. Starting with early Holocene before 6000 BP (move L433-439 up), then transition with the increased in productivity and enhanced OMZ after 6000BP (combine L425-432 and L439-450).

We will rephrase this section to make the line of events more clear.

13. **Section 5.2 L488-490:** “a data-model comparison ... in both basins”. I thought the data-model comparison was only for the Arabian Sea. Please clarify. Note that adding an insert map of core location and model regions on figure 6 or would help the reader locate things. At least provide lon/lat of cores.

The wording comes from an earlier version of the ms. which included a section on the BoB in Section 5 which was removed due to excessive length of the ms. We will rephrase this section.

14. **Section 5.2 L495-498:** I am not sure I follow how the match between model and data in oxygen suggests that it is due to oceanic circulation rather than local biological processes. The authors state “it is assumed that ..” in L490 but it seems neither the authors nor prior work has actually showed that circulation controls the simulate change in oxygen in this region in the model. This is an important point because that claim is repeated several times in the manuscript (see L385 and comment #11, L307 and comment #6 and conclusion). The author should either look at the biological and/or circulation changes in the model they present here or use models from others such as Bopp et al 2017 to make the claim (see comment #15 below).

See authors' response to second paragraph of review with ref to Segsneider et al., 2018, where all these points are addressed.

15. **Section 5.** Please consider adding the study of Bopp et al 2017, which compares simulations at the LGM and mid-holocene, linking to the changes from Pleistocene to Holocene mentioned by the authors. The paper includes a qualitative comparison of simulated O₂ with O₂ proxies (Fig 3) and

shows model ventilation changes between LGM and mid-holocene (Fig S3) in the Indian Ocean. Note that this model is not a transient run.

See response to #14.

16. **Section 6.** Authors should discuss their Figure 7 here. It is only mention in passing in L512. Something like “as shown in Figure 7...”. Indeed, most prior work on ESM’s OMZ was not targeting the Indian Ocean. Figure 7 would be a good opportunity to present specifically the results in the Indian Ocean.

That is correct. We will discuss Figure 7 and problems associated with ESMs in more detail.

17. **Section 6.** Authors should consider folding in this section the following recent papers looking at global OMZ and oxygen in ESMs. Models agree on the sign of warming-driven (O₂sat) and biological-circulation (AOU) changes, but uncertainties arise from the subtle balance between these two opposing terms (Bopp et al 2017, Resplandy 2018). Papers highlighting the influence of circulation changes and non-resolved processes such as eddy-driven circulation and mixing (Duteil and Oschlies, 2011, Duteil et al 2014, Lachkar et al 2016, Palter and Trossman 2018, Fu et al 2018, Busecke et al 2019, Bahl et al 2019, Couespel et al 2020).

It is true that we missed out some of the recent model results in this section and we thank the reviewer for pointing them out. We will include them in section 6 by discussing more in detail, why the future predictions have such great uncertainties.

18. **Section 7.1.1** The text is well written but it is much more detailed than the rest of the sections in the review. Authors might consider summarizing/emphasizing the take home messages, the links with oxygen and the OMZ and the implications for trophic webs which are quickly mention at the end of section L640. If there are there other groups than the co-authors that worked on this topic (I am not a specialist of this subtopic), it might be worth including some of their work here.

We will shorten this section by deleting some of the details and make a better effort to summarize our work especially the implications from and for the OMZ. Very few researchers have looked at the reasons for the recent and sudden appearance of mixotrophic blooms in winter in the Arabian Sea and none has delved into its connection to the OMZ. However their results will be discussed.

19. **Section 7.1.2.** The OMZ control migration but please also consider adding the fact that zooplankton vertical migration influence the oxygen consumption vertical patterns. This effect is missing from most ocean bio models and from all ESMs. The following studies are global but include maps showing the impact in the Indian Ocean. Bianchi et al. 2013 (their Fig 3) Aumont et al 2018 (their Fig 9) show simulated oxygen decline due to DVM. Note that most references in this section are relatively old. The authors could consider checking for newer results on this topic, maybe including references from other OMZs to fuel their discussion if not available in Indian Ocean.

Ok

20. **Section 7.1.3** in implications discusses zooplankton but not the DVM aspects. It might be missing because part of the section is missing (see unfinished sentences L 682).

This will be clarified

21. **Section 8.** The conclusion is too vague and too speculative. It tries to blend mesoscale eddies to paleo-changes but this is a difficult task (see my comments #6, 11 and 14). “This was caused by .. changes in circulation”. Again this has not been demonstrated by the Authors (note that the paper

by Bopp et al 2017 which shows ventilation changes between LGM and mid-holocene might help the authors to make the case).

This will be clarified.

22. **Figure 4:** specify O₂ threshold used to compute OMZ thickness and how this “maximum thickness” is evaluated. Also briefly describe the data used here: How many cruises or from a database? What are the years during which these data were taken? Label seasons on plot (e.g. change symbols, add labels or colors).

The depth of the OMZ and mean oxygen concentration were obtained by Acharya and Panigrahi (2016) from the World Ocean Atlas 2013 and the Global Ocean Data Analysis Project. These authors applied the 20 μM threshold to define the OMZ. We will clarify this in the revised version of the ms.

23. **Figure 5** could be improved so the difference between the two panels with/without eddies is more obvious and consistent with the text, i.e. eddies influence oxygen supply, nutrient supply and production. Oxygen does not seem to change between the two. Production and nutrient supply do not seem to change either.

Ok, this will be improved.

24. **Figure 6:** Nice and interesting figure.

- It would be great to add the WOA present day oxygen concentration at 0 ka to compare to the model results on panel b. done in Segschneider et al., 2018
- Please specify the model depth range for the oxygen values in caption.
- 200 -800m, specified in Fig
- Please provide an insert map with core locations and model regions and/or provide lon/lat of cores and model regions in caption. Please remove “sinking” from caption, this is confusing (It sounds like subduction of oxygen).

Ok

Other comments:

L27: check grammar Ok

L112-114: move to next section? Unclear why it is here. Ok, this will be removed.

L150-151: Add other refs about filaments and eddies.

The given reference refers to organic carbon fluxes measured along a filament that developed during the JGOFS expeditions. This will be clarified and filament and eddies are discussed in a different chapter.

L183: typo on conational? Ok

L195: drops? dropped? Check tense (past/present) in section 2.3. Ok

L202: “the in comparison”. Remove the? Ok

L203: “less intense ... than” Ok

L223: should “since than” read “since then”? Ok

L226-227: add ref for the mixing analyses. Ok

L233: (to 75%). Is this up to 75%? It is the average – It is the average. The ‘to’ will be deleted.

L239: last sentence of section should be clarified and better linked to the rest of the paragraph. Why are they linked if the seasonal thermocline is hypoxic? Do you mean if the oxygen content of the seasonal thermocline remains stable through seasonal changes? This sentence probably belongs to the next section which defines the seasonal thermocline and make the link between bio production and physical supply of oxygen. The sentence will be deleted.

L240: Note that you define seasonal thermocline in L 241 but use it already in L239. **This part will be changed.**

L246: “the season thermoclines” > seasonal thermocline? **Ok**

L255: upper part **of** the thermocline? Upper thermocline? **Ok**

L266: remove “upper part” and “lower part” as depth are specified. **Ok**

L267: “the base of the SNM is located.... In contrast to the SNM...” the base of the SNM is in the SNM.

This sentence doesn’t make sense. Please rewrite. **Ok**

L272: which suggests. **Ok**

L306 “preventing the development of anoxic conditions”. As noted by the authors in section 2.3 anoxic conditions already occur in the northern IO. Clarify the sentence. **This will be changed into ‘persistent anoxic conditions’.**

L341-342: please rephrase sentence. “This leads” what leads? Clarify the links between oxygen change, denitrification, nutrient supply, production and feedback on oxygen change. **Ok**

L345: Thus eddies “would/could” affect..... this model results present a very interesting hypothesis but it does not make the link to fish habitat. At least modulate the link to fish except if you have a reference that makes this link in this region. **Ok**

L351: “Using YY It could be shown that XX (Lachkar et al)” replace by Lachkar et al () showed that XX using YY. **This will be clarified.**

L360 and 363: remove “and hence weaken the OMZ” and “weakening the OMZ”. Here the authors discuss the mean state of the OMZ not a trend. As mentioned above Bio + Phy ~ 0 in OMZs, hence eddy ventilation does not weaken the OMZ, it contributes to the supply of oxygen that balance the biological demand. Note that your section 4.3 discusses how variations in eddy activity could indeed result in variations in the oxygen supply and OMZ volume. **Ok, this will be clarified.**

L364-365: link to denitrification – cite paper(s) showing that denitrification inhibition occurs here. **Ok**

L385. Not clear why there is a “However” to start the sentence here. Is this sentence incompatible with the previous one? **‘However’ will be deleted.**

L439: could you please clarify the link between enhanced upwelling and ventilation by ICW? Is it through reduced residence time? **The monsoon drives both upwelling and the inflow of ICW. The ICW improves the ventilation by supplying oxygen to the OMZ and reducing the residence time of water within the OMZ. This will be clarified.**

L448: “matches results from model...” I don’t think this model has been presented yet. Please provide reference here or reference to section 5.3 which comes after. Also add reference to Figure 6b here. **Ok**

L524 please clarify that the increase in hypoxic waters is global scale not in the Indian Ocean. **Ok**

L582: does the journal authorize “in review” citations? **This will be changed.**

L682: missing text? **Ok**

L715: define OM. **(organic matter, Ok)**

L796-797: check sentence and grammar. **Ok**

L798: “The in comparison”? **Ok**

References;

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