Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-82-RC3, 2020

Biogeosciences Discussions

Interactive comment on "Present past and future of the OMZ in the northern Indian Ocean" *by* Tim Rixen et al.

Anonymous Referee #3

Received and published: 10 June 2020

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.

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.

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.

Detailed major comments

1. Abstract and introduction. - 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.).

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 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 that 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.).

4. Section 3.1 discusses the Arabian Sea. What about the Bay of Bengal?

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.

- 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 colocation 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 ae 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?).

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.

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.

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.

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.

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).

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).

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.

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)

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

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.

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).

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 metion 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.

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.

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).

21. Section 8. The conclusion is vague and 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).

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).

23. Figure 5 could be improved so the difference between the two panels with/without eddies is more obvious and consistent with the text, ie.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.

24. Figure 6: Nice and interesting figure. - It would be great to add the WOA present day oxygen concentration at 0 ka on the plot to compare to the model results on panel b. - Please specify the model depth range for the oxygen values in caption. - 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).

Other comments:

L27: check grammar

L112-114: move to next section? Unclear why it is here.

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

L183: typo on conational?

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

L202: "the in comparison". Remove the?

L203: "less intense . . . than"

L223: should "since than" read "since then"?

L226-227: add ref for the mixing analyses.

L233: (to 75%). Is this up to 75%?

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.

L240: Note that you define seasonal thermocline in L 241 but use it already in L239.

L246: "the season thermoclines" > seasonal thermocline?

L255: upper part of the thermocline? Upper thermocline?

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

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.

L272: which suggests

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.

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

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.

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

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 tremd. As mentioned above Bio + Phy $\sim$ 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.

L364-365: link to denitrification – cite paper(s) showing that denitrification inhibition occurs here. L385. Not clear why there is a “However” to start the sentence here. Is this sentence incompatible with the previous one?

L439: could you please clarify the link between enhanced upwelling and ventilation by ICW? Is it through reduced residence time?

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.

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

L582: does the journal authorize “in review” citations?

L682: missing text?

L715: define OM.

L796-797: check sentence and grammar.

L798: “The in comparison”?

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