Note to the editor:

Dear Editor,

Thank you for your report and feedback.
As requested, we provide point-by-point response to the reviewer's comments in the attached document.

Please, note that the role of ventilation changes resulting from monsoon wind perturbations is well taken into account in our simulations (please see our response to reviewer's comment #1).
The study does not consider only the ventilation changes that are independent of monsoon wind intensity (for instance associated with circulation changes at the global or basin scales).

We do recognize however that the concomitant occurrence of such additional perturbations in conjunction with monsoon wind changes may affect the overall response of the OMZ. Thus, we revised the abstract and conclusion to make this clearer as you and the reviewer suggested.

We hope that with these additional clarifications and final revisions you will find our manuscript suitable to publication in Biogeosciences.

Thank you for your time and dedication.

Best regards,

Zouhair Lachkar and co-authors

Response to the reviewer

We thank the reviewer for his final feedback. Please find below point-by-point replies to the reviewer's comments. Reviewer comments are highlighted in black and author responses are in blue.

1) Link of the model result to future projections and the past evolution of the OMZ.

Yes, it is clearly stated in the ms that the simulations are highly idealized and are not intended to reproduce the past evolution of the OMZ or to predict its future trajectory and that they rather aim at exploring the sensitivity of the Arabian Sea OMZ to monsoon wind intensity changes but this is not reflected in the abstract and conclusion.

In the conclusion we can read : A set of coupled physical biogeochemical simulations of the Arabian Sea ecosystem reveals a tight coupling between the intensity of the

summer monsoon wind and the size and intensity of the Arabian Sea OMZ. We find that the OMZ and ecosystem responses are largely determined by the perturbation of the summer SW monsoon ….

In the abstract the authors wrote that the model results show that the Arabian Sea productivity increases and its OMZ expands and deepens in response to monsoon wind intensification and that this lead to a strong intensification of denitrification at depth, resulting in a substantial amplification of fixed nitrogen depletion in the Arabian Sea.

This is misleading without mentioning the limitation of their simulation and that ventilation change in response to varying monsoon strength can cause the opposite.

The changes in the ventilation that are driven by monsoon wind changes are already represented in the model and are taken into account. Additionally, their effects on oxygen are quantified as can clearly be seen in the oxygen budget presented in Fig. 9 and Fig A9. While enhanced ventilation opposes the effect of increased biological consumption of oxygen, its effect is smaller in magnitude than the effect of increased productivity, except in the upper 200m. This is discussed in detail in section 4.1.

What we did not cover in the study are the changes in large-scale ventilation (and circulation) that are independent of local wind changes (e.g., changes in the global thermohaline circulation). For more clarity and following the reviewer suggestion, we have added statements in the abstract and the conclusion to further highlight that we are considering the effect of wind changes in isolation and that the overall OMZ response may also depend on changes in large-scale ventilation and stratification.

In the abstract, we have added: “Additional potential changes in large-scale ocean ventilation and stratification may affect the sensitivity of the Arabian Sea OMZ to monsoon intensification.” (see lines 20-21 in the revised abstract).

In the conclusion, we have added: “These results are obtained while considering the effects of monsoon wind changes in isolation. The response of the OMZ to wind increase may differ however in the presence of other concomitant perturbations such as potential changes in large-scale circulation and ventilation or additional surface warming.” (see lines 30-32, p23).

2) The presented idea that large-scale ventilation changes act on longer and changes in wind intensity on shorter time scales is not supported by sediment tarp results. They indicate that increased winds speeds intensify upwelling but at a certain level decrease carbon export because of associated ventilation changes (Rixen et al 1996). This should be considered in the discussion.

The study by Rixen et al (1996) compares export fluxes at 3000m at three stations in the Arabian Sea with the intensity of SW monsoon winds at the trap locations between 1986 and 1992. There is nothing in this study that concerns the changes in the large-scale (e.g., basin-scale) circulation or ventilation or the OMZ response timescales. Therefore, we do not think this is relevant to our work nor to the discussion of the timescales of large-scale ventilation changes.

3) That in the Holocene, large-scale ventilation changes may have played an important role together with fluctuations in monsoon intensity was not only suggested by Gaye et al and Das at 2017 but already in 2014 by Rixen et al 2014 (Biogeosciences) which should accordingly be cited.

Done. Citation added.

4) However my main critics it that the authors present conclusion drawn form simulations (see abstract and conclusion) without mentioning the limitation of their simulation and that ventilation change in response to varying monsoon strength caused the opposite. This is misleading but can be solved by modifying the abstract and conclusion.

Done. See our response to previous comment #1.

2) First of all I have suggested to discuss not to ignore the deep denitrification peak. That the response of low denitrification rates at depth could be more important as those of at shallower depth for the total denitrification is an interesting result. It suggests to pay more attention to denitrification at greater water-depth in future field studies.

Thanks for the clarification. We agree with the reviewer's conclusion.

5) The third point refers to the comment on sinking speed and respiration rates: That previous studies did it same is not an scientific argument.

As we have shown in our previous response, using the historical parameters of the model resulted in a good agreement with observations. Changing the sinking speed and remineralization rate in such a way that the remineralization depth remains constant will not lead to any change in the results as explained in our previous response to a similar comment.