

Comment on gmd-2020-431

Anonymous Referee #1

Referee comment on "Sensitivity of asymmetric Oxygen Minimum Zones to remineralization rate and mixing intensity in the tropical Pacific using a basin-scale model (OGCM-DMEC V1.2)" by Kai Wang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-431-RC1>, 2021

General comments:

Wang and co-workers address in their paper "Sensitivity of asymmetric Oxygen Minimum Zones to remineralization rate and mixing intensity in the tropical Pacific using a basinscale model (OGCM-DMEC V1.2)" one of the still open issues on understanding the interplay between the physical ocean and the marine biogeochemistry in shaping OMZs. Based on a basin-scale model with a high horizontal resolution they perform sensitivity studies with a set of vertical mixing parameters and a reduced DON remineralisation rate. With the final parameter set they state that the model successfully reproduces the observed asymmetric OMZ. Unfortunately, there is no new scientific finding in this paper. The results are very descriptive without any critical assessment. Furthermore, the "improved model" setup is only evaluated wrt. oxygen distributions. Potential effects on other biological components and/or processes due to the new parametrisation are not analysed, even so, changes in the OMZ might feedback onto the net community production. The authors state in their conclusion that a "reduced remineralization rate leads to remarkable decrease of biological consumption over 200- 400 m". This is a rather trivial finding.

Response: Thanks for the constructive comments. We have made major revisions by taking into consideration of all reviewers' comments and suggestions. In particular, we have added more analyses on responses of biological consumption (equivalent to net community production in the OMZ) to the new parameterizations. In addition, we have assessed the responses of physical supply to changes in both physical and biological parameterizations, with in-depth analyses on the interactions/feedbacks and their impacts on the asymmetry of OMZs. We have rewritten the discussion and conclusion sections.

The physical and biological component of the applied OGCM are not sufficiently introduced. Only after reading previous papers of the authors I could gain a rudimentary understanding of the physical model setup. It would be useful to describe at least the major characteristics of the physical model. I also find that it is not a sufficient introduction of the biogeochemical component to only provide its equations in an Appendix. A more detailed description might be "boring" to the authors but it is very useful for the readers to get the basic concept of their model assumptions. Moreover the oxygen cycle, the core topic of this paper, seems to be newly implemented into the biogeochemical module. However, a comprehensive introduction is missing. It would be interesting to know: 1) how is guaranteed that oxygen consumption does not exceed available oxygen? 2) are there any restrictions to remineralization depending on oxygen levels? Oxygen consumption from NH_4 oxidation seems to be missing or is neglected. Oxygen production/consumption is calculated with a fixed ratio from NCP. However, photosynthesis based on nitrate produces a higher amount of oxygen than on NH_4 . Similarly, remineralization to NH_4 needs less O_2 .

Response: Thanks for the constructive comments. Since this is a model evaluation paper (not model development paper), we only provided a brief description on the OGCM and biogeochemical model in the previous version. But, we have added some details in the

revised manuscript, particularly for the biogeochemical model (line 104-107 and 113-120). Dissolved oxygen (DO) has been a state variable (just like dissolved inorganic carbon) in the basin-scale biogeochemical model. Most parameters used to compute the sources/sinks of oxygen are the same as those used to compute the sources/sinks of nitrogen and carbon. We have analyzed/validated many biogeochemical variables in our previous studies, e.g., chlorophyll (Wang *et al.*, 2009a; Wang *et al.*, 2013b), nitrogen uptake and regeneration (Wang *et al.*, 2009b) and carbon cycling (Wang *et al.*, 2015b).

Our sensitivity experiments include new simulations that have “restrictions to remineralization depending on oxygen levels”, which guarantees oxygen consumption not exceeding available oxygen. The impacts of nitrogen cycle on oxygen consumption/production or the interactions between oxygen cycle and nitrogen cycle are complex, according to some studies (Sun *et al.*, 2021; Kalvelage *et al.*, 2013; Oschlies *et al.*, 2019). But there is limited information available for the parameterizations of relevant processes. In order to model these processes, we need more field data that allow us to quantify oxygen consumption/production in associated processes and to derive relevant parameters.

Furthermore, there is no sentence on nitrogen reduction processes such as denitrification, whose activity is highly correlated with export production in the ETSP (Kalvelage *et al.*, 2013). As no values are given for NCP, export production, and also the distributions of nutrients or detritus are not provided it is impossible for the reader to judge the quality of the model performance. As far as I know, there has been previously no assessment of their biological component for the depth range below 200 m. In view of the extensive degree of revision, I refrain from specific and technical comments.

Response: Thanks for the constructive comments. Because there is limited information available for calibration and validation of some nitrogen reduction processes including denitrification, the regional model does not simulate them at this stage but will include these processes in the future once information becomes available. We have reported/validated many biogeochemical fields, including PP & NCP (Wang *et al.*, 2006b), new production (Wang *et al.*, 2006a) in the euphotic zone, and nitrate, iron, POC/detritus and export production below 200 m (Yu *et al.*, 2021). We have added these references in the revised manuscript.

Anonymous Referee #2

Referee comment on "Sensitivity of asymmetric Oxygen Minimum Zones to remineralization rate and mixing intensity in the tropical Pacific using a basin-scale model (OGCM-DMEC V1.2)" by Kai Wang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-431-RC2>, 2021

The manuscript, "Sensitivity of asymmetric Oxygen Minimum Zones to remineralization rate and mixing intensity in the tropical Pacific using a basin-scale model (OGCM-DMEC V1.2)" by Wang et al. conduct a suite of model parameter sensitivity experiments with a very old, coarse resolution regional physical ocean model. While using an older model is not necessarily a disadvantage, it is only an advantage if the relative strengths and weaknesses of the model are provided such that the reader can integrate the current analysis to other current understanding. That context is not currently provided. For example, focusing on this region has the advantage that the sponge resets the source O₂ (a major weakness of global models) to observations (the authors should note this strength of the current approach). Unfortunately, the comparability of the physical formulation to other models is missing. For example, it is unclear whether the Indonesian Throughflow is represented which is an important part of the advective ventilation in the Western part of the basin and the partitioning of lateral oxygen source waters into the Eastern part of the basin.

Response: Thanks for the constructive comments. We have added one sentence about the strengths and weaknesses of the model: "Such model configuration may have a disadvantage for longer simulations and analyses, but has the advantage in reproducing the spatial patterns of most physical and biogeochemical fields". We have also clarified that the model closes the western boundary and no representation of the Indonesian throughflow is included. Consistent with our previous publications (Yu *et al.*, 2021; Wang *et al.*, 2015a; Wang *et al.*, 2008) and numerous other studies (Duteil *et al.*, 2020; Duteil, 2019; Llanillo *et al.*, 2018; Ito and Deutsch, 2013) which focus on the tropical Pacific without the ITF, we rely on the imposed meridional boundary relaxation to constrain our regional solution. Clearly this is inadequate in the strictest sense of the processes mentioned by the reviewer. We posit that the validations presented support our contention that the model is reasonably constrained for the timescales we are considering in this study. Further studies will include the Indonesian-Pacific configuration with an explicit representation of the ITF and DO ventilation into the domain as reported in Rodgers *et al.* (1999). The current focus is on the Pacific processes which we deem are adequately represented in the current configuration.

The analysis uses an inappropriate definition of "suboxic" (see below). Throughout the manuscript the word "rates" is used when "rate constant" is intended (e.g. on line 204 "Reducing remineralization rate by 50% (Cd0.5 minus reference) leads to large decrease...") making it difficult to interpret the result since it is unclear whether the "rate" is proportionally reduced by 50% with fixed concentration or whether there are compensating responses/increases in concentration that result in a change in the remineralization locations. While the result of the combined need to reduce the remineralization rate constant and increase the vertical diffusivity to better match oxygen distributions is encouraging, the manuscript oddly stops there without coming to any implications of the work for our understanding of the oxygen and nitrogen cycles or the past or future of the OMZ. What was learned that wasn't known before? Most importantly, the final sentence of the conclusions, "Future studies utilizing advanced models are needed to better understand the impacts of physical and biological interactions on the variability and drivers of the tropical OMZs."

Suggests the authors themselves are unclear as to the significance of the present work to current ocean biogeochemical modeling. As such, I recommend the authors work to clarify there descriptions and the implications and limitations of the current work in revision.

Response: Thanks for the constructive comments. Regarding the definition of “suboxic”, previous studies have used a wide range of DO as a criterion, e.g., $<5 \text{ mmol m}^{-3}$ (Yakushev and Neretin, 1997; Bianchi *et al.*, 2012; Karstensen *et al.*, 2008) and $<20 \text{ mmol m}^{-3}$ (Helly and Levin, 2004; Babbin *et al.*, 2015; Wright *et al.*, 2012; Oguz *et al.*, 2000). Some researchers selected $\text{DO} < 20 \text{ mmol m}^{-3}$ as the boundary of OMZs (Paulmier and Ruiz-Pino, 2009; Fuenzalida *et al.*, 2009; Bettencourt *et al.*, 2015). Accordingly, we adopt the criterion of $<20 \text{ mmol m}^{-3}$ for both suboxic water and OMZ volume.

We have made major revisions with an improved approach, i.e., applying a varying constant for OM remineralization that is restricted by oxygen level. We have rewritten the related sections, thus, the questions/comments related 50% are not applicable any more. We have made major revisions with more assessments and in-depth analyses, and rewritten the discussion sections that include the implications and limitations of the current work.

Technical comments:

Line 26 –“which made significant progresses” needs rephrasing.

Response: we have reworted as “with significant progress”.

Line 40 – The authors are misinformed as to the definition of “suboxic”, quoting a value of 20 mmol m^{-3} ... suboxia is defined as an oxygen level at or below the detection limit, typically $2\text{-}10 \text{ mmol m}^{-3}$ where interesting nitrogen redox chemistry such as N_2O production, denitrification and annamox occur. The current definition of <20 is rather “strongly hypoxic” as it is well within the detectible range and well above the region of interesting redox chemistry. I would note that the reference the authors cite, Paulmier and Ruiz-Pinu (2009), use a suboxic level of 4.5 umol/gk . Also, if the authors want to describe the truly “suboxic” volume, they should be aware that while Table 3 notes a volume of “suboxic” waters from WOA13, it has been demonstrated that these mapped products strongly underestimate the volume of suboxia at the $<5 \text{ mmol/m}^3$ definition (Bianchi *et al.*, 2012; <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2011GB004209>)

Response: Thanks for the constructive comments. There is a wide range of DO value used to define suboxic water. In the paper of Paulmier and Ruiz-Pino (2009), they cited $\text{DO} < 4.5 \text{ mmol m}^{-3}$ from an earlier study (Karstensen *et al.*, 2008), but also stated “ $\text{DO} < 20 \text{ mmol m}^{-3}$ corresponds to a usual suboxic condition used to separate the aerobic (O_2 -respiration) from the denitrifying (NO_3 -respiration) activity (Oguz *et al.*, 2000)”. In addition, Wright *et al.* (2012) also defined $0\text{-}20 \text{ mmol m}^{-3}$ as suboxic water. We have added some explanation regarding the definition of “suboxic” in the revised manuscript (line 37-39).

We were aware that Fuenzalida *et al.* (2009) and Bianchi *et al.* (2012) used the WOA2005 to estimate OMZ volume, and reported similar values at $<20 \text{ mmol m}^{-3}$ definition. In our study, we derived similar OMZ volume ($5.97 \times 10^6 \text{ km}^3$ to the north and $1.43 \times 10^6 \text{ km}^3$ to the south) using WOA2013.

Line 45-47 – There is an underlying assertion here that data alone provides understanding, and that more availability of data will resolve the underlying mechanisms. This is a false

premise. Only by contextualizing the observations in a theoretical framework can mechanistic understanding be achieved. Also, “our understanding is uncompleted in terms” should be rephrased.

Response: We have reworded as “our understanding is limited on the underlying mechanisms that regulate DO dynamics at mid-depth”.

Line 54 – “often” seems unnecessary here given that if the OMZ stretches across the equator it would seem to always lead to an overestimate of the OMZ area... unless there is a concomitant decline in area elsewhere in some models. If the latter is indeed the case, it would be worth mentioning. If the intent is just to point out the overestimate, then remove “often”.

Response: We have removed “often”.

Line 57 – “Apparently, it’s necessary to...” this is an odd way of saying this, making it sound like the authors are annoyed at the idea.

Response: We have removed “Apparently”.

Line 73 – This is a really old, coarse resolution model. A lot of advance has occurred over the last 25 years.

Response: While this is an old model, a non-high-resolution model, our previous studies have shown that this model can reproduce mesoscale and sub-mesoscale structures such as the tropical instability wave (TIW) (Zhang *et al.*, 2018; Tian *et al.*, 2018). Indeed, there had been a lot of advance in regional and global model development. We have also made significant improvements in the DMEC sub-model, including the implementation of a phytoplankton dynamic module with a non-steady C:Chl ratio (Wang *et al.*, 2009a), and refined parameterizations of detritus decomposition and DON remineralization (Yu *et al.*, 2021).

Line 74-75 – What is the vertical grid? The stated 10-50m +20*10m layers = 210-250 m... this is not deep enough to represent the OMZ...?

Response: The vertical resolution varies over depth, with ~30-50 m in the core OMZ (at ~300-500 m) and the total depth of ~1200 m. We have clarified this in the revised manuscript (line 78-81).

Line 75 – What is the longitudinal grid? 150W-80E? Are the walls open to admit the Indonesian throughflow? This would seem critical for representation of O₂ ventilation flow into the domain (e.g. Rodgers *et al.*, 1999; <https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/1998JC900094>). Is the Indonesian throughflow prescribed? Both factors should also be explicit.

Response: The longitude of our model is from 124°E to 76°W. In this basin-scale model, the western boundary is closed, without representation of the Indonesian throughflow. We have clarified this in the revised manuscript (line 84-85). Consistent with our previous publications and numerous other studies which focus on the tropical Pacific without the ITF, we rely on the imposed meridional boundary relaxation to constrain our regional solution. We

posit that the validations presented support our contention that the model is reasonably constrained for the timescales we are considering in this study.

Line 96 – the parameters here described as “rates” are actually “rate constants”, e.g. r is the rate constant for zooplankton respiration.

Response: We have made corrections.

Line 116 – What does “DON poor” mean?

Response: We have corrected “DON poor” as “DON pool”.

Line 128 – This implies that the model domain extends to 1000 m or more, suggesting line 74-75 is incorrect.

Response: Yes, the model domain extends to ~1200 m. We have added such information in model description (line 78-79).

Line 140 – It is important to note that “underestimation of supply” is complex and can be from either O₂ being too low in the waters that supply or the physical supply mechanisms being either too sluggish or out of balance (e.g. lateral versus vertical and advective versus diffusive”

Response: Thank you for the constructive comments. We have made major revisions with more/new analyses on the response of physical supply, including lateral and vertical advection and vertical mixing. In addition, we have also assessed the impacts of biological and physical processes and their interactions/feedbacks on net flux.

Line 145-149 – How did these perturbations influence the fidelity of T and S?

Response: Enhanced vertical mixing (i.e., the addition of background diffusion) only applies to the most important variables in this study (i.e., DO and DON). Thus, T and S are not influenced. We have clarified this in the revised manuscript (line 172-173).

Line 152 – What is the reference value of C_d ? What does it do? There is no parameter called “ C_d ” in the appendix, only “ $CDON_0$ ” the remineralization rate constant at 10 °C, but its reference value, 0.001, is very different from 0.5. Looking at Table 1, I see that “ $C_{d0.5}$ ” is actually “ $CDON_0 \cdot 0.5$ ”. However, it is not clear what the 100-600 m range of “0.0005-0.00025” means... is this the role of temperature on $CDON_0$? This parameter needs a sentence or two of introduction, definition, and contextualization here to avoid confusion.

Response: We have made major revisions with an improved approach (i.e., applying a varying value for the constant of OM remineralization). Thus, the questions/comments related $C_{d0.5}$ are not relevant any more. We have added “ C_{DON} decreases with depth over 100-1000 m, following an exponential function in this study”, and also other information in model description section.

Line 204 - the word “rates” is used when “rate constant” is intended (e.g. on line 204 “Reducing remineralization rate by 50% ($C_{d0.5}$ minus reference) leads to large decrease...”)

making it difficult to interpret the result since it is unclear whether the “rate” is proportionally reduced by 50% with fixed concentration or whether there are compensating responses/increases in concentration that result in a change in the remineralization locations.

Response: We have made major revisions with an improved approach to compute the reduced and also varying rates of remineralization. Thus, the above questions/comments are not relevant anymore.

Line 211 – “there is somehow a small decrease...” The use of “somehow” is an insufficient explanation... what is causing this decrease? Is it a response to the remineralization constant decrease?

Response: We have made major revisions with new model simulations, new analyses and figures. We have rewritten most paragraphs in Model evaluation and discussions section. Thus, the above questions/comments are not relevant anymore.

Line 224 – Only here is it explained that there was no response in temperature to the diffusivity change. This should have been noted earlier in the results as requested above, as well as the salinity response.

Response: Yes, there was no response in temperature. We have clarified this point in the revised manuscript (line 172-173).

Line 228 – “Limited field studies” – why is the defining feature of these studies that they were “limited”? Is the evidence derived from them inconclusive? More explanation of context would be helpful.

Response: We have made major revisions, and “Limited field studies” are not in the revised manuscript.

Anonymous Referee #3

Referee comment on "Sensitivity of asymmetric Oxygen Minimum Zones to remineralization rate and mixing intensity in the tropical Pacific using a basin-scale model (OGCM-DMEC V1.2)" by Kai Wang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-431-RC3>, 2021

This paper examines the sensitivity of the oxygen minimum zones (OMZs) to a change in the remineralization rate and changes in the vertical diffusion coefficient in the tropical Pacific. The goal of this study is to present a calibrated model, to evaluate this model and to identify the mechanisms that explain the asymmetric shape of the OMZ in the tropical Pacific.

Unfortunately this paper only shows the impact of two parameter changes, change in the remineralization rate and the vertical background diffusivity, mainly on the oxygen fields and DON distribution without providing an explanation about the driving mechanisms and a thorough discussion of the results. With this there is so far no scientifically new finding in the current state of the manuscript. In particular an explanation about the mechanisms that drive the asymmetric shape of the OMZs in the tropical Pacific, that is already present in the reference simulation, is missing due to the mainly descriptive nature of the paper. In addition the language that is used is in many places imprecise. E.g. there is no differentiation between vertical and horizontal transport processes as both are termed physical transport.

Response: Thank you for the constructive comments. We have made major revisions with more in-depth analyses and thorough discussion of the results, and with explanation about the mechanisms that drive the asymmetry of the OMZs in the tropical Pacific. In particular, we have analyzed the differentiations of physical transport, i.e., horizontal advection, and vertical advection and vertical mixing; we have also analyzed the relative influences of biological and physical processes on sources and sinks of DO, and compared the differences between the northern OMZ and southern OMZ. Our analyses indicate that physical supply has a major influence on the asymmetric OMZs, in which vertical mixing plays a dominant role.

I think there is potential, that this model and the performed sensitivity simulations, could be used to explain the mechanisms that drive the asymmetry in the oxygen fields, although the current version leaves the reader with too many open questions. The model description is not sufficient. There is e.g. no information given about the biogeochemical boundary conditions. What does it mean: All biological components are computed in a manner similar to physical variables. What is the reason to average the model output for the period of 1981-2000 and do not include the last 18 years. Specially, as later in the manuscript some months of year 2009 are compared to some cruise data that are not introduced in the manuscript, they just appear. Are the model data detrended before averaging? Is there a remaining model drift? Why not simply using climatological simulations? What kind of inter-annual forcing is used?

Response: Thank you for the constructive comments. We have added more details in the model description section, including biogeochemical boundary conditions (line 81-88), and parameterizations of DON remineralization and detritus decomposition (line 113-120). "All biological components are computed in a manner similar to physical variables" means that all biological variables are influenced by physical processes (e.g., advection and diffusion). We have rephrased that sentence as "All biological components use nitrogen as their unit, in which sources/sinks are determined by biological and chemical processes in

addition to the physical processes (circulation and vertical mixing) that are computed by the OGCM”.

We used model output from the period of 1981-2000 for the comparison with WOA2005 dataset because most oxygen data were obtained prior to 2000. Since we did not find much difference between WOA2005 and WOA2013, we did not change the period of 1981-2000 from model simulation in the previous version. But, we use model outputs from 1981-2010 in the revised manuscript. We consider that it is necessary to use cruise data for further model validation, thus we do not using climatological simulations. We have provided more information about the datasets during the revisions.

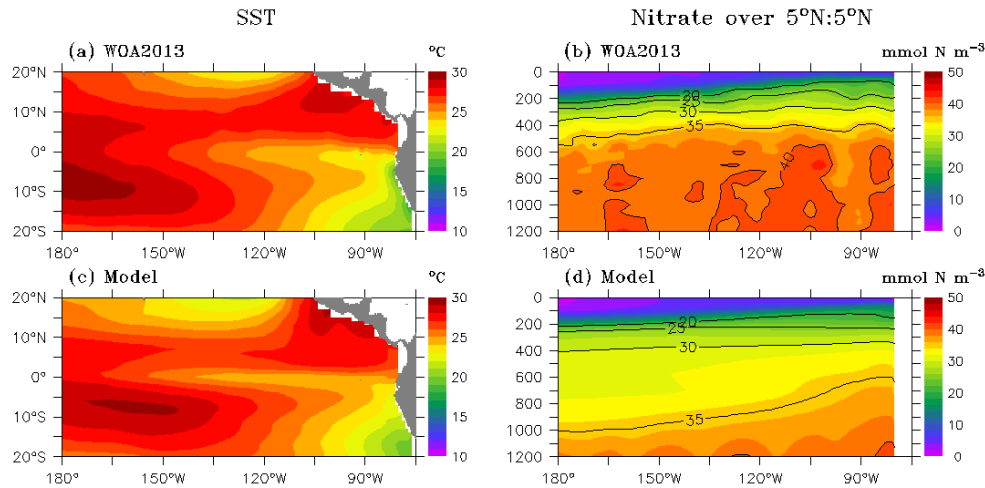
The model data were not detrended before averaging, and we did not find a model drift. The inter-annual model forcing includes interannual 6-day means of precipitation (<ftp://ftp.cdc.noaa.gov/Datasets/gpcp>) and surface wind stress (from NCEP reanalysis).

Although it is mentioned at several places (abstract and introduction) that the model is calibrated - there is no further information given how. Is it calibrated only against the oxygen fields? As the reference simulation already represents the asymmetric shape of OMZ, I assume that this is at least partly a result of the model calibration. The only information that is in the paper: Some parameters have been changed compared to an earlier version of the model. This is however not sufficient.

Response: Thank you for the constructive comments. The basin-scale model was calibrated against many biogeochemical fields, including chlorophyll (Wang *et al.*, 2009a), nitrogen cycle (Wang *et al.*, 2009c) and carbon cycle (Wang *et al.*, 2015a). In this study, we further calibrated against DON remineralization and oxygen fields. Although the reference simulation represents the asymmetric OMZ, it is not a result of the model calibration against biogeochemical fields. Our analyses indicate that it is largely determined by physical processes. We have added more information regarding model calibration, and in-depth analyses of physical transport during the revision. We have also provided more details on model description, including parameterizations of relevant biogeochemical processes (line 102-120).

The model evaluation and validation is lacking. The oxygen fields look fine, but there is no information about, e.g. the circulation. How good is the current system represented in this model? What about nitrogen and phosphate - is there a bias in the east west gradient (one of the problems as shown e.g. by Dietze and Loeptien, 2013, doi:10.1002/gbc.20029) or is that reduced etc.

Response: Our previous studies have provided model validation for chlorophyll (Wang *et al.*, 2009a; Wang *et al.*, 2013a), nitrogen cycling (Wang *et al.*, 2009b), and carbon fields (Wang *et al.*, 2015a; Yu *et al.*, 2021). The model does a good job in representing the current system thus physical fields such as sea surface temperature (Wang *et al.*, 2008; Zhang *et al.*, 2018) (also see Figure a & c below). The model can also reproduce the west-to-east gradient of nitrate along the equator (see Figure b & d below). We have made a lot of improvement in model evaluation during the major revisions, including (1) the responses of biological consumption and physical supply to reduced O:C utilization ratio and enhanced vertical mixing, (2) interactive effects of physical and biological processes on source and sink of mid-depth DO, (3) impacts of physical and biological processes on asymmetric OMZs.



Regarding the structure of the paper - I would suggest to reorganize the paper: There should be a clear separation between the model set up as well as the set up of the sensitivity simulations and the results. In addition, regarding the comparison with the cruise data - these simply appear, where do they come from? Same happens with the DON data from HOT.

Response: Thank you for your suggestion. We have made major revisions with some reorganization by taking into account all reviewers' comments and suggestions. We have made changes in the Model experiments and validation section, and added the source of cruise data (line 201-202).

The sensitivity simulations show an improved representation of the OMZ in Fig4. The description of this improvement in the text is somewhat incorrect. The asymmetric shape is present in all simulations. It seems that the changes in the parameters result in an overall increase of DO and not necessarily an increase of the asymmetry. As at the southern hemisphere the DO concentrations are lower (in fact higher), one might get the impression that this increase might be slightly larger, but there is no evidence that this is the case at this stage. In addition, in the text it is stated that between 2°S-2°N the values of DO are relatively high (~30-40 mmol m⁻³) in Cd0.5Kb0.5. Fig 4f does not support this. Around 400m depth the DO concentrations are below 20 mmol m⁻³.

Response: Thank you for the constructive comments. We realize the incorrect text (i.e., ~30-40 mmol m⁻³), and have made corrections. We have made major revisions, with in-depth analyses and discussion of changes/responses of sources and sinks between the south and north OMZs. Our analyses demonstrate that net flux (i.e., increase of DO) is greater to the south than to the north, and the asymmetry of OMZs is largely determined by physical supply, in which vertical mixing plays a dominant role. We have rewritten the relevant sections in the revised manuscript.

Fig 10 clearly shows differences - but there is no explanation given about the choice of the region shown as well as what have been done with the data - I guess they have been averaged. As the vertical oxygen gradients are different in these two regions, I would have expected a difference in the oxygen response, as the vertical diffusive transport depends on the gradient. Unfortunately the explanation of the results are again left to the reader.

Response: Thank you for the constructive comments. We have made major revisions, with new analyses (and figures) showing the difference in DO response, and changes in consumption, total supply and horizontal and vertical advections and vertical mixing (Figures 9-11). We have rewritten the relevant section with thorough discussion and explanation on the differences in biological consumption and physical supply between the southern and northern OMZs.

In addition there is no clear explanation given about the choice of the parameter change for the sensitivity simulations and with this it seems rather arbitrary. Also why is an increase of the vertical background diffusivity of 0.5 cm²/s optimal, what about higher rates?

Response: Thank you for the constructive comments. We have added explanation about the choice of the parameter changes in the revised manuscript. New biological parameterizations were derived based on field measurement (line 163-166). Regarding the parameter for background diffusion, there is a large range (~0.01-0.5 cm²/s) in other modeling studies. Most studies reported improvements in mid-depth DO, using a value <0.5 cm²/s, e.g., Duteil and Oschlies (2011). While stronger vertical mixing could also lead to improved oxygen at mid-depth, a too-high value for background diffusion (mainly representing molecular diffusion) may not be acceptable.

A thorough discussion of the results is missing. How are the results related to other modeling studies? The sensitivity studies show that the major changes occur along the equator - so this indicates that somehow the representation of the current system is important and needs to be shown and discussed. There are several physical processes in addition to the known impact of vertical mixing that seem to be capable to reduce the oxygen model bias along the equator, e.g. enhancing zonal diffusion or enhancing viscosity.

Response: Thank you for the constructive comments. We have made major revisions with more in-depth analyses and thorough discussion of the results. We have included comparisons with other modeling studies, e.g., zonal advection and meridional advection, vertical advection and vertical mixing (line 280-283). We have also provided more information on model validation, including physical fields (e.g., a good representation of the current system). We agree that other physical processes in addition to vertical mixing may be able to reduce the model bias along the equator. But, this is beyond the scope of this study. Future studies will include sensitivity experiments with enhanced zonal diffusion or viscosity.

Also, as the model is forced with an inter-annually varying forcing, what about the potential impact of El Nino events?

Response: Our preliminary analyses indicated that there were no significant impacts of El Nino events on mid-depth oxygen. But, El Nino events might have large impacts on oxygen fields in surface water. These analyses will be carried out in a future study.

As the manuscript needs substantial revisions, I do not add any specific and/or technical comments at this stage of the review process.

Reference

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