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.

Reply： Thank you for the constructive comments. We agree that there is a need to improve our manuscript with more in-depth analyses and through discussion of the results, and with explanation about the mechanisms that drive the asymmetric shape of the OMZs in the tropical Pacific. We will make major revisions to address these issues. In particular, we will add more analyses on physical supply, including horizontal advection, and vertical transport, focusing on the differences between the north OMZ and south OMZ.

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?

Reply: Thank you for the constructive comments. We will add more details in the model description section, including biogeochemical boundary conditions. "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 will rephrase that sentence to clarify this.

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. But, we will 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 did not using climatological simulations. We will provide more information about the datasets during the revisions.

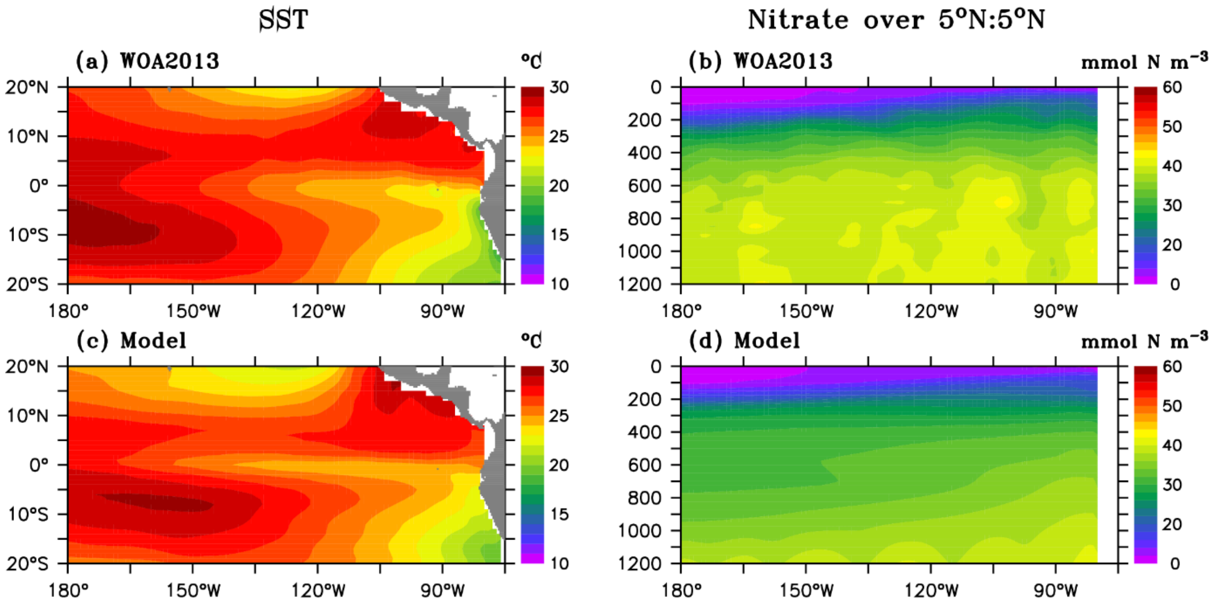
The model data were not detrended before averaging, and we did not find a model drift. The inter-annual model forcing includes interannual 6-day means of precipitation (ftp://ftp.cdc.noaa.gov/Datasets/gpcp) and surface wind stress (from NCEP reanalysis, ftp://ftp.cdc.noaa.gov/Datasets/ncep.reanalysis2.dailyavgs).

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.

Reply: Basin-scale model was calibrated against many biogeochemical fields, including chlorophyll (Wang et al., 2009a), nitrogen cycle (Wang et al., 2009b) and carbon cycle (Wang et al., 2015). In this study, we further calibrated against DON and oxygen fields. Although the reference simulation represents the asymmetric OMZ, it is not a result of the model calibration against biogeochemical fields. We believe that it is largely related to physical processes, e.g., sluggish circulation and weak ventilation associated with the sub-tropical gyres (Kuntz and Schrag, 2018; Wyrtki, 1962). We will add more information on model calibration and in-depth analyses of physical transport during the revision. We will also provide more details on model description, including parameterizations of relevant biogeochemical processes.

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.

Reply: Our previous studies have provided model validation for chlorophyll (Wang et al., 2013), carbon fields (Wang et al., 2015; 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 will make improvement in model evaluation during the major revisions, aiming to better understand the responses of biological consumption and mid-depth DO to reduced remineralization and enhanced mixing, and relative roles of physical supply and biological consumption in shaping the asymmetric OMZs. We will also add new analyses on the regulation of advection and vertical mixing.



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.

Reply: Thank you for your suggestion. We will make major revisions with some reorganization by taking into account all reviewers' comments and suggestions. In addition, we will make necessary changes in the sections of model description and/or model-data comparisons, with details about the cruise data (e.g., DO along P04 and P21, https://cchdo.ucsd.edu) and station data (e.g., DON at HOT, https://hahana.soest.hawaii.edu/hot/hot_jgofs.html).

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, 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.

Reply: Thank you for the constructive comments. We get your point "not necessarily an increase of the asymmetry….", and realize the incorrect text (e.g., ~30-40 mmol m^{-3}). We will make necessary changes/corrections during the major revisions, not only in the description of Fig. 4, but also in the discussion of asymmetric OMZs and underlying mechanisms.

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.

Reply: Thank you for the constructive comments. The choice of the regions (ETNP and ETSP) is mainly for the suboxic water (DO <20 mmol m^{-3}), and all the values are means averaged for the box and over 1981-2000. We will add more details for Fig. 10, and also provide more explanation on the differences in oxygen responses between ETNP and ETSP, by assessing advection and vertical transport during the major revisions.

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 cm^2/s optimal, what about higher rates?

Reply: Thank you for the constructive comments. We will add explanation about the choice of the parameter during major revisions. In brief, new biological parameters were derived during model calibration against DON, and oxygen fields. Regarding the parameter for background diffusion, there is a large range (~0.01-0.5 cm^2/s) in other modeling studies

(Duteil and Oschlies, 2011; Ledwell et al., 1998; Matear and Hirst, 2003; Stramma et al., 2012); most studies reported improvements in oxygen at mid-depth using a value between 0.1 and 0.5 cm^2/s. While stronger vertical mixing could also lead to improved oxygen at mid-depth, a too-high value for background diffusion (mainly representing molecular diffusion) could not be accepted.

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.

Reply： Thank you for the constructive comments. We will add more in-depth analyses (including comparisons with other modeling studies) with through discussion of the results. We will also provide 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 capable to reduce the oxygen 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?

Reply： 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.

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