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.

Reply: Thanks for the constructive comments. We will make major revisions by considering all reviewers' comments and suggestions.

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 NH4 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 NH4. Similarily, remineralization to NH4 needs less O2.

Reply: Thanks for the constructive comments. Since this is a model evaluation paper (not model development paper), we only provide a brief description on the OGCM and biogeochemical model. But, we could add some details particularly for the biogeochemical model if space is allowed. 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.*, 2013), nitrogen uptake and regeneration (Wang *et al.*, 2009b) and carbon cycling (Wang *et al.*, 2006b; Wang *et al.*, 2015).

Regarding "1) how is guaranteed that oxygen consumption does not exceed available oxygen? 2) are there any restrictions to remineralization depending on oxygen levels?", we have been testing the parameterization with sensitivity experiments. Some studies have suggested that remineralization of DOM is retarded by low-DO conditions (Kalvelage *et al.*, 2015; Laufkötter *et al.*, 2017), which yield a wide range of half-saturation constant. We will include the sensitivity experiments in the revised manuscript.

The model does include "oxygen consumption from NH4 oxidation" (see eq. B9), which is a portion of $c_{DON}DON + c_{DS}D_s + c_{DL}D_L$. 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). 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 200m. In view of the extensive degree of revision, I refrain from specific and technical comments.

Reply: 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 will add some of the references in the revised manuscript.

Reference

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