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Comment on gmd-2020-431

Anonymous Referee #3

Referee comment on "Sensitivity of asymmetric oxygen minimum zones to mixing intensity and stoichiometry in the tropical Pacific using a basin-scale model (OGCM-DMEC V1.4)" by Kai Wang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-431-RC3, 2021

Review to: 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

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.

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?

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.

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.

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.

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.

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.

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?

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

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