Response to Anonymous Referee #1

The manuscript investigates, using a model, potential processes that can explain the asymmetry of the tropical Pacific Ocean Oxygen Minimum Zones (OMZs). The topic is important as the modelling of the Tropical Pacific OMZ, and, in particular its asymmetry, is a challenge that have to face modelers. However, in its present form, I have serious reservations about the scientific significance of this manuscript. Essentially, the work presents the results of 4 experiments performed with a coupled physical biogeochemical model. The paper is not innovative in terms of modelling, the authors refer to another work for the description and validation of the model while the analysis of model results is quite basic and could have been done with much more details.

Response: Thank you for the constructive comments. We have made major revisions to improve our manuscript. For example, we have added more details in model description, and more information regarding model experiments. We have also revised our approach in terms of model validation, and the analyses of model results with much more details.

Starting from an initial parameterization of the model (reference simulation) that gives results that are broadly validated with the WOA2013 climatology, the authors decide to perform 4 experiments in which they change the degradation parameter (2 experiments) and vertical (diffusive) mixing (2 experiments) in order to better simulate the volume of low oxygen zone in the region of the Tropical Pacific. Then, the authors compare the 5 simulations and conclude that an increase in the vertical mixing helps with representing the asymmetry in the Tropical OMZ.

First, there are few rationales for justifying the choice and new formulations of the investigated processes (i.e; mixing, degradation). The physics and other biogeochemical variables are not shown and hence for the reader this is not straightforward to understand what motivates the authors to believe that the mixing and degradation are the process that need to be improved. They do not show evidences that the model overestimates degradation or underestimates mixing when looking at modeled variables.

Response: Thank you for the constructive comments. We have made the following changes:

(1) We have added more details about the model in the section 2.2 Ocean biogeochemical model: "The equations for biogeochemical processes and model parameters are described in Appendix A and B. There have been changes in some parameters comparing with those in Wang et al. (2008), which were based on our model calibration and validation for chlorophyll (Wang et al., 2009a, Wang et al., 2013), nitrogen cycle (Wang et al., 2009b) and carbon cycle (Wang et al., 2015)". We have also provided more information in the section 2.3 Computation of oxygen sources and sinks, e.g., "Below the euphotic zone, the concentration of DO is influenced by physical supply and biological consumption …".

(2) We have revised/rewritten the section 3.2 Sensitivity experiments. In particular, to clarify the rationales for the model experiments, the first paragraph has been rewritten as "Given that the mid-depth DO concentration is influenced by physical supple and biological consumption, and remineralization of DON is the dominant process for oxygen consumption, the underestimated DO at mid-depth would be a result of overestimation of consumption associated with DON

remineralization and/or underestimation of supply. Indeed, the reference run over-estimates biological consumption over 100-400 m (Figure 3). Thus, we apply a reduced (by 50%) DON remineralization constant, which leads to a remarkable improvement in simulated DON and consumption. The reference run applies a zero value for background diffusion (equation 4). However, a previous modeling study has demonstrated that background diffusion is an important process for DO supply at mid-depth (Duteil and Oschlies, 2011). Accordingly, we conduct a few more simulations to investigate how reduced remineralization rate and adding background diffusion affect simulated DO distribution, which include changing the partial mixing parameter Pm from 0 to 0.1, 0.3, 0.5 or 1.0 cm^2 s^{-1} (Table 1)".

(3) We have also reorganized the sensitivity experiments, which include some new simulations, in response to some other comments (see more information/responses below).

Then, the authors do not investigate what are the consequences for the simulated physics and biogeochemistry of such changes. Rather, the different experiments are compared with climatology but only for oxygen and over 300-500 m depths. The authors do not mention how an increase in diffusion and transport of oxygen will impact oxygen in the layer above 200 m and below 500 m neither the consequences of this increased diffusion for the other variables (physical and biogeochemical) in terms of agreement with observations.

Response: Thank you the constructive comments. We have showed the comparisons of DO over 200-400, 400-700 and 700-1000 m, and also added more model-data comparisons using cruises' DO data. We have added a new figure to show the impacts of reduced remineralization and increased diffusion on the vertical distributions of DON and oxygen consumption in terms of agreement with observations (see figure below). In addition, we have revised the discussion section, with new figures to show the changes in DO, biological consumption and physical supply over 200-400, 400-700 and 700-1000 m.

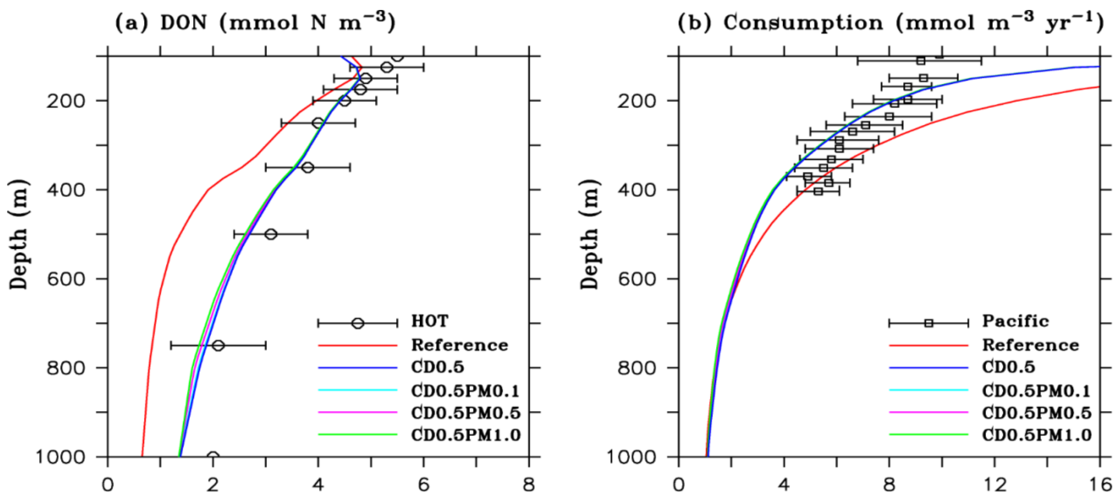

Figure 3. Comparisons of DON concentration (a) and consumption rate (b) between observation and model experiments. Observed DON data are from Hawaii Ocean Time-series program (HOT, 22°45'N, 158°00'W) (https://hahana.soest.hawaii.edu/hot/hot_jgofs.html). Observed consumption data are obtained from Karastensen et al., (2008) for the entire Pacific.

As important, in terms of biogeochemical modelling, the authors decide not to describe the model and to refer to Wang et al (2008) for details. However, looking at Wang et al (2008), I was not able to find oxygen as a state variable which means that the modeling of oxygen is not described neither its validation which is an important prerequisite before starting sensitivity studies. I am surprised to see that important process like nitrification or oxygen production associated with nitrate reduction are not taken into account. I would have hoped to see a detailed description of the modeled oxygen cycling and model formulations with a thorough validation of model performances using oxygen data (in addition to a very board comparison with climatology). This comparison would have allowed the reader to clearly understand model limitations and reasons for changing model formation. Besides, the resolution of the model as well as that of the forcing (i.e. 6-day averaged mean wind stress) is quite rough and this may also explain some of the model deficiencies but this is not discussed at all.

Response: Thank you for the constructive comments. This basin-scale model was developed to study the upper ocean dynamics for the tropical Pacific, and has been used to understand the spatial and temporal variability of physical and biogeochemical processes. Our previous studies have shown that this model can reproduce mesoscale and sub-mesoscale structures such as the tropical instability wave (TIW) (Zhang, 2016; Zhang and Busalacchi, 2008), and the carbon model (Wang et al., 2015) forced by 6-day mean winds did a good job in simulating the carbon fields in the Tropical Pacific. Thus, we believe that the potential bias caused by the resolution of our model and 6-day winds would be small.

The model does incorporate nitrification (see Wang et al., 2009b). There have been changes/improvements (relative to Wang et al. 2008) in some parameters, which were based on our further model calibration and validation, mainly for chlorophyll (Wang et al., 2009a; Wang et al., 2013), nitrogen cycle (Wang et al., 2009b) and carbon cycle (Wang et al., 2015). Oxygen is a state variable in the basin-scale biogeochemical model, but this is the first time showing mode calibration and validation for oxygen cycle. Most parameters used to compute the sources/sinks of oxygen are the same as those for nitrogen and carbon cycles. We agree with that more details about the model and more model validation should be presented in this paper. We have added model equations and parameters, and carried out more model-data comparisons.

Finally, the plausibility of the sensitivity studies is not discussed. I was just wondering what are the rationales for using a background diffusion that is 100 times higher than molecular diffusion and using a modified O:C ratio,

Response: Thank you for the constructive comments. We have made major revisions, with more information regarding the sensitivity studies (see responses above). We realize that there was some “weakness” in our previous model experiments, e.g., no combination of reduced remineralization and enhanced background diffusion. Thus, we have conducted some new experiments, including testing different values for background diffusion.

There is a large range (~0.01-0.5 cm^2/s) in the parameter for background diffusion (K_b) used in modeling studies. It appears that smaller values are used in ocean models that apply the KPP scheme (Large et al., 1994) but higher values used in models with other mixing scheme. For example, Zhu and Zhang (2018) used 0.01 cm^2/s in an ocean model that has the KPP scheme, but

Wang and Matear (2001) used 0.1 cm^2/s in a model with the Chen mixing scheme (Chen et al., 1994). Wang (2002) conducted a comparison of the Chen scheme (K_b=0.1 cm^2/s) and KPP scheme (K_b=0), which showed large similarity in SST, SSS and MLD (see Figure below).

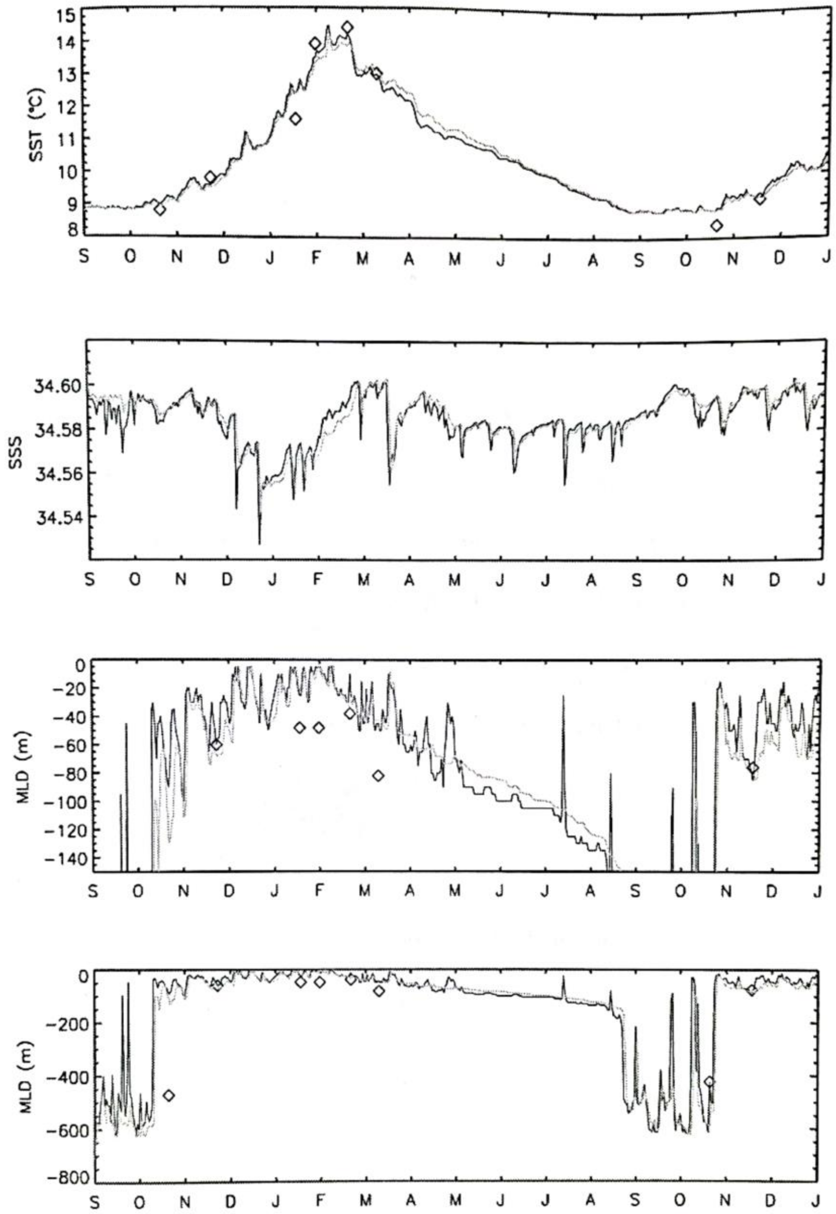

Figure 2.3 The daily sea surface temperature (SST), sea surface salinity (SSS), and mixed layer depth (MLD) from the simulations by the Chen scheme (dark line) and the KPP scheme (light line) and observation for the period of September 1996 to December 1997 in the Subantarctic Zone of the Southern Ocean.

Wang, Xiujun (2002). Modeling upper ocean dynamics in the Southern Ocean: Interaction of physics and biogeochemistry. Ph.D. Thesis, page 31, University of Tasmania.

Details Line 14 For clarity DO needs to be defined, ETNP

Response: We have defined DO and ETNP.

Line 28: "The carbon cycle has garnered much attentions and made significant process", This sentence should be rewritten e.g. The carbon cycle has garnered much attentions and its understanding made significant progresses

Response: We have reworded as "the carbon cycle has garnered much attentions, which made significant progresses".

Line 29: physical/chemical processes (e.g., the fluxes between the atmosphere, land and ocean). This is vague please specify

Response: We have reworded as "physical/chemical processes (e.g., carbon fluxes between the atmosphere, land and ocean)".

Line 39: in most ocean basin, DO concentration is not below 20 mmol /m3 except in OMZs of the Pacific and Indian Ocean

Response: Thank you for the constructive comments. We have deleted that part of the sentence.

Line 56: Please specify: "missing biogeochemical feedbacks in the models".

Response: Thank you for the suggestion. We have rewritten this sentence.

Line 77: "Chen mixing scheme (Chen et al., 1994), which varies from 10 m to 50 m on the equator." I assume that it is not the mixing scheme that varies between 10m to 50 m but rather the vertical resolution. Correct?

Response: We have corrected as "The mixed layer (the upper-most layer) depth is determined…, which varies from 10 m to 50 m".

Line 78: what is the vertical resolution in the OMZs ?

Response: The vertical resolution is ~30-50 m in the core OMZ.

Eq. 8: is it evaluated using the simulated SST or at 20 C ? I do not understand why we have "at 20C)

Response: We have reworded as "where Sc and Sc_{20} are the Schmidt number at SST and 20ºC, respectively".

Line 162-162: "some models overestimated the extent of suboxic water, which might be due to over-estimated productivity in the euphotic zone" This conclusion does not seem in agreement with results of Exp1 and Exp2 that show that a decrease in respiration does not allow the representation of asymmetric OMZ

Response: That sentence has been removed because we have rewritten that paragraph due to the changes in sensitivity experiments and model validations.

Line 225: I find that the use of smaller size is confusing. I guess that the authors mean smaller amount. (and not particles size since the DOM is dissoved).

Response: Yes, we have changed to "a smaller amount of DOM".

Line 245: the authors mention that the asymmetric features in many physical and biological fields in the Tropical Pacific are largely associated with asymmetries in water mass exchange between the equatorial and off-equator Pacific Ocean. However, here they use an enhanced vertical diffusion to create this asymmetry and this is not clear how this parameterization can mimic asymmetry water masse exchanges with the regional outside the Pacific.

Response: Thank you for your constructive comment. We have re-assessed the model experiments, and made some changes in the sensitivity study which includes new simulations with smaller parameters for background diffusion.

Section 3 (very broadly) describes the results of the experiments but is placed outside the results section.

Response: We have changed this section as Results section.

Figure 5: I would say sensitivity experiments rather than sensitive experiments.

Response: Corrected.

Table 3: please correct Pm is a diffusion coefficient and has to be in m2/sec and not /m2/sec.

Response: Corrected. In order to compare with others' results, we use cm^2/s rather than m^2/s.

Bibliography

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