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Interactive comment on "Evaluation of asymmetric Oxygen Minimum Zones in the tropical Pacific: a basin-scale OGCM-DMEC V1.0" *by* Kai Wang et al.

Anonymous Referee #1

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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.

Starting from an initial parameterization of the model (reference simulation) that gives results that are broadly validated with the WOA 2013 climatology, the authors decide

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Discussion paper

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. 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 300-500m 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 500m neither the consequences of this increased diffusion for the other variables (physical and biogeochemical) in terms of agreement with observations.

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.

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,

Details Line 14 For clarity DO needs to be defined, 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

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

Line 29: in most ocean basin, DO concentration is not below 20 mmol /m3 except in OMZs of the Pacific and Indian Ocean

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

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?

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

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

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

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).

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.

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

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

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

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