Anonymous Referee #2

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-RC2, 2021

The manuscript, "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. conduct a suite of model parameter sensitivity experiments with a very old, coarse resolution regional physical ocean model. While using an older model is not necessarily a disadvantage, it is only an advantage is the relative strengths and weaknesses of the model are provided such that the reader can integrate the current analysis to other current understanding. That context is not currently provided. For example, focusing on this region has the advantage that the sponge resets the source O2 (a major weakness of global models) to observations (the authors should note this strength of the current approach). Unfortunately, the comparability of the physical formulation to other models is missing. For example, it is unclear whether the Indonesian Throughflow is represented which is an important part of the advective ventilation in the Western part of the basin and the partitioning of lateral oxygen source waters into the Eastern part of the basin.

Reply: Thanks for the constructive comments. In this regional configuration, the model closes the western boundary and no representation of the Indonesian throughflow is included. Consistent with our previous publications (Wang et al., 2008; Wang et al., 2015; Yu et al., 2021) and numerous other studies (Duteil, 2019; Duteil et al., 2020; Ito and Deutsch, 2013; Llanillo et al., 2018) which focus on the tropical Pacific without the ITF, we rely on the imposed meridional boundary relaxation to constrain our regional solution. Clearly this is inadequate in the strictest sense of the processes mentioned by the reviewer. We posit that the validations presented support our contention that the model is reasonably constrained for the timescales we are considering in this study. Further studies will include the Indonesian-Pacific configuration with an explicit representation of the ITF and the O2 ventilation into the domain as reported in Rodgers et al. (1999). The current focus is on the Pacific processes which we deem are adequately represented in the current configuration. We will provide more details on the model, including configuration, boundary conditions and so on.

The analysis uses an inappropriate definition of "suboxic" (see below). Throughout the manuscript the word "rates" is used when "rate constant" is intended (e.g. on line 204 "Reducing remineralization rate by 50% (Cd0.5 minus reference) leads to large decrease…") making it difficult to interpret the result since it is unclear whether the "rate" is proportionally reduced by 50% with fixed concentration or whether there are compensating responses/increases in concentration that result in a change in the remineralization locations. While the result of the combined need to reduce the remineralization rate constant and increase the vertical diffusivity to better match oxygen distributions is encouraging, the manuscript oddly stops there without coming to any implications of the work for our understanding of the oxygen and nitrogen cycles or the past or future of the OMZ. What was learned that wasn't known before? Most importantly, the final sentence of the conclusions, "Future studies utilizing advanced models are needed to better understand the impacts of physical and biological interactions on the variability and drivers of the tropical OMZs." Suggests the authors themselves are unclear as to the significance of the present work to current ocean biogeochemical modeling. As such, I recommend the authors work to clarify there descriptions and the implications and limitations of the current work in revision.

Reply: Thanks for the constructive comments. Regarding the definition of "suboxic", previous studies have used a wide range of DO as a criterion, e.g., <5 mmol m^{-3} (Bianchi et al., 2012; Karstensen et al., 2008; Yakushev and Neretin, 1997) and <20 mmol m^{-3} (Babbin et al., 2015; Helly and Levin, 2004; Oguz et al., 2000; Wright et al., 2012). Some researchers selected DO < 20 mmol m^{-3} as the boundary of OMZs (Bettencourt et al., 2015; Fuenzalida et al., 2009; Paulmier and Ruiz-Pino, 2009). Accordingly, we adopt the criterion of <20 mmol m^{-3} for both suboxic water and OMZ volume.

"Reducing remineralization rate by 50%" means applying reduced constant (C_{DON}) of remineralization (by 50%, i.e., Cd0.5). We will make necessary changes to clarify this, and also improve the interpretation with more in-depth analyses. We will take into consideration of all reviewers' comments and suggestions during the major revisions, including "implications of the work for our understanding of the oxygen and nitrogen cycles".

Technical comments:

Line 26 –"which made significant progresses" needs rephrasing.

Reply: we will rephrase the sentence.

Line 40 – The authors are misinformed as to the definition of "suboxic", quoting a value of 20 mmol m-3… suboxia is defined as an oxygen level at or below the detection limit, typically 2-10 mmol m-3 where interesting nitrogen redox chemistry such as N2O production, denitrification and annamox occur. The current definition of <20 is rather "strongly hypoxic" as it is well within the detectible range and well above the region of interesting redox chemistry. I would note that the reference the authors cite, Paulmier and Ruiz-Pinu (2009), use a suboxic level of 4.5 umol/gk. Also, if the authors want to describe the truly "suboxic" volume, they should be aware that while Table 3 notes a volume of "suboxic" waters from WOA13, it has been demonstrated that these mapped products strongly underestimate the volume of suboxia at the <5 mmol/m3 definition (Bianchi et al., 2012; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2011GB004209)

Reply: Thanks for the constructive comments. There is a wide range of DO value used to define suboxic water. In the paper of Paulmier and Ruiz-Pino (2009), they cited DO <4.5 umol/kg from an earlier study (Karstensen et al., 2008), but also stated "DO <20 umol/kg corresponds to a usual suboxic condition used to separate the aerobic (O2-respiration) from the denitrifying (NO3-respiration) activity (Oguz et al., 2000)". In addition, Wright et al. (2012) also defined 0-20 umol/kg as suboxic water (See Figure below). We will add some explanation regarding the definition of "suboxic" in the revised manuscript.

We were aware that Fuenzalida et al. (2009) and Bianchi et al. (2012) used the WOA2005 to estimate OMZ volume, and reported similar values at <20 mmol m^{-3} definition. In our study, we derived similar OMZ volume ($5.97*10^6$ km^3 to the north and $1.43*10^6$ km^3 to the south) using WOA2013. We will mention the underestimation at <5 mmol m^{-3} definition during the revision.

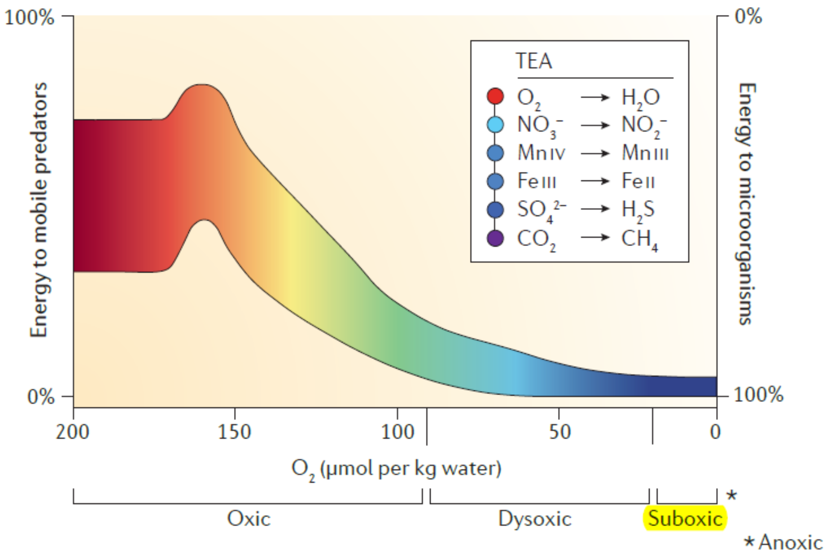

Figure 2 | **O_2 concentration affects ecosystem energy flow.** Alternative states of

Line 45-47 – There is an underlying assertion here that data alone provides understanding, and that more availability of data will resolve the underlying mechanisms. This is a false premise. Only by contextualizing the observations in a theoretical framework can mechanistic understanding be achieved. Also, "our understanding is uncompleted in terms" should be rephrased.

Reply: We will rephrase these sentences.

Line 54 – "often" seems unnecessary here given that if the OMZ stretches across the equator it would seem to always lead to an overestimate of the OMZ area… unless there is a concomitant decline in area elsewhere in some models. If the latter is indeed the case, it would be worth mentioning. If the intent is just to point out the overestimate, then remove "often".

Reply: We will remove "often".

Line 57 – "Apparently, it's necessary to…" this is an odd way of saying this, making it sound like the authors are annoyed at the idea.

Reply: We will remove "Apparently".

Line 73 – This is a really old, coarse resolution model. A lot of advance has occurred over the last 25 years.

Reply: While this is an old model, we would not think it is a coarse resolution model given that the meridional resolution is 0.33° over 5°S-5°N. And the reviewer one called this model a high horizontal resolution model. Our previous studies have shown that this model can reproduce mesoscale and sub-mesoscale structures such as the tropical instability wave (TIW) (Tian et al., 2018; Zhang et al., 2018), and spatial and temporal variations of biogeochemical fields in the tropical Pacific (Wang et al., 2008; Wang et al., 2015).

Line 74-75 – What is the vertical grid? The stated 10-50m +20*10m layers = 210-250 m… this is not deep enough to represent the OMZ…?

Reply: The vertical resolution varies over depth, with ~30-50 m in the core OMZ (at 300-500 m) and the total depth of ~1200 m. We will make some rewording to clarify this.

Line 75 – What is the longitudinal grid? 150W-80E? Are the walls open to admit the Indonesian throughflow? This would seem critical for representation of O2 ventilation flow into the domain (e.g. Rodgers et al, 1999; https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/1998JC900094). Is the Indonesian throughflow prescribed? Both factors should also be explicit.

Reply: The longitude of our model is from 124°E to 76°W. In this basin-scale model, the western boundary is closed, without representation of the Indonesian throughflow. Consistent with our previous publications and numerous other studies which focus on the tropical Pacific without the ITF, we rely on the imposed meridional boundary relaxation to constrain our regional solution. We posit that the validations presented support our contention that the model is reasonably constrained for the timescales we are considering in this study.

Line 96 – the parameters here described as "rates" are actually "rate constants", e.g. r is the rate constant for zooplankton respiration.

Reply: We will replace "rate" with "constant".

Line 116 – What does "DON poor" mean?

Reply: We will correct as "DON pool".

Line 128 – This implies that the model domain extends to 1000 m or more, suggesting line 74-75 is incorrect.

Reply: Yes, the model domain extends to ~1200 m.

Line 140 – It is important to note that "underestimation of supply" is complex and can be from either O2 being too low in the waters that supply or the physical supply mechanisms being either too sluggish or out of balance (e.g. lateral versus vertical and advective versus diffusive"

Reply: Thank you for the constructive comments. We agree that "underestimation of supply" is complex, and will make revision to address this issue, e.g., by adding new analyses such as advection, upwelling and diffusion of DO.

Line 145-149 – How did these perturbations influence the fidelity of T and S?

Reply: Enhanced vertical mixing (i.e., addition of background diffusion) only applies to the most important variables in this study (i.e., DO and DON). Thus, T and S are not influenced. We will clarify this in the revised manuscript.

Line 152 – What is the reference value of Cd? What does it do? There is no parameter called "Cd" is the appendix, only "CDON0" the remineralization rate constant at 10 C, but it's reference value, 0.001, is very different from 0.5. Looking at Table 1, I see that "Cd05" is actually "CDON0*0.5". However, it is not clear what the 100-600 m range of "0.0005-0.00025" means… is this the role of temperature on CDON0? This parameter needs a sentence or two of introduction, definition, and contextualization here to avoid confusion.

Reply: Yes, "Cd0.5" is actually "CDON0*0.5". We will provide more information for the model experiments, and also add a couple of sentences to clarify how CDON0 varies over depth.

Line 204 - the word "rates" is used when "rate constant" is intended (e.g. on line 204 "Reducing remineralization rate by 50% (Cd0.5 minus reference) leads to large decrease…") making it difficult to interpret the result since it is unclear whether the "rate" is proportionally reduced by 50% with fixed concentration or whether there are compensating responses/increases in concentration that result in a change in the remineralization locations.

Reply: We will add more details on model experiments, and rephrase some relevant definition/statements.

Line 211 – "there is somehow a small decrease…" The use of "somehow" is an insufficient explanation… what is causing this decrease? Is it a response to the remineralization constant decrease?

Reply: We will rephrase that sentence (regarding "somehow"). The decrease in supply with enhanced vertical mixing was mainly a result of change in vertical distribution of DO, caused by enhanced downward transportation of DON, which led to lower DON concentration in the OMZ and thus less DO consumption. The relevant discussion was in the next two paragraphs. We will rewrite some of the sentences to make this point clearer.

Line 224 – Only here is it explained that there was no response in temperature to the diffusivity change. This should have been noted earlier in the results as requested above, as well as the salinity response.

Reply: Yes, there was no response in temperature. We will address this point during the revision.

Line 228 – "Limited field studies" – why is the defining feature of these studies that they were "limited"? Is the evidence derived from them inconclusive? More explanation of context would be helpful.

Reply: We will phrase that sentence.

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