Geoscientific Model Development Discussions

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Comment on gmd-2020-431

Anonymous Referee #2

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-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. 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.

Technical comments:

26 –"which made significant progresses" needs rephrasing.

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/kg. 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)

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.

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".

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.

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

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…?

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.

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

116 – What does "DON poor" mean?

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

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"

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

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.

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.

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?

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.

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.