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## **Comment on bg-2022-3**

Anonymous Referee #2

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Referee comment on "Hydrodynamic and biochemical impacts on the development of hypoxia in the Louisiana–Texas shelf Part 1: roles of nutrient limitation and plankton community" by Yanda Ou and Z. George Xue, *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2022-3-RC2>, 2022

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### **Review of “Hydrodynamic and Biochemical Impacts on the Development of Hypoxia in the Louisiana–Texas Shelf Part I: Numerical Modeling and Hypoxia Mechanisms”**

This manuscript by Ou et al. utilized a coupled physical-biogeochemical model to investigate the controlling factor of bottom hypoxia on the northern Gulf of Mexico and Louisiana-Texas Shelf. The authors added the phosphorus cycle and modified the sediment oxygen consumption module in an existing biogeochemical model NEMURO and coupled it with ROMS model. The coupled model was validated with observational data and then used to implement a 15-year hindcast simulation during 2006-2020. Then the authors explored the spatial variation of hypoxia development in the study area and found sediment oxygen consumption (SOC) and water column stratification are main factors to control the bottom oxygen in nearshore and offshore area respectively. Their model results also indicated separate hypoxia development schemes on the west and east Louisiana-Texas Shelf.

Coastal deoxygenation is one of the most prominent environmental issues with important implications for marine ecosystem services. Although this paper made efforts to adopt a more sophisticated biogeochemical model with added phosphorus cycle and improved sediment oxygen consumption module, making contributions to investigate the spatial differences of dominant processes on hypoxia, it lacks original and novel aspects to explore the well-studied topic in this region, as well as comprehensive comparison with previous modeling study on the model performance, simulation results and conclusions, and address the question that how this new model stands out. The manuscript missed an advanced understanding and deep insight on the research topic of coastal hypoxia in a well-organized discussion section, thus this paper is a little thin on content. Although I see the value of this work, I perceive that the publication is premature at this time. My major

and detailed comments are listed below.

Major comments:

- The hypoxia at the northern Gulf of Mexico has been well studied since the 1990s with increasing model studies in recent years. It ranged from a simple oxygen respiration model (Hetland&DiMarco, 2008) to a sophisticated coupled biogeochemical model (Laurent et al. 2012; Fennel et al. 2013). Including this study, they all generated similar conclusions that SOC is the controlling factor for hypoxia. In this sense, the improvement of complexity in the biogeochemical model does not make much sense. Also, the authors mentioned the additional work done on the NEMURO-based model filled gaps in phosphorus cycling and improved SOC representation. It's better to prove the advancement of the new model by validating with important variables, such as DO, Chla, PO4, NO3, with other model simulation studies.
- The oxygen balance analysis is confusing and questionable. Although SOC is the dominant process in the bottom hypoxia generation (You et al. 2015), water column respiration (WCR) should not be orders of magnitude smaller than SOC, especially in the whole water column, as shown in Figure 15 and L455-456. Observational studies still showed varying evidence on SOC contribution (Murrell&Lehrter, 2011; Quiñones-Rivera, et al. 2010). More importantly, the reviewer has a sense that the authors did not understand and explain the oxygen dynamics well (Figure 10 and 15, section 4.2). What is oxygen balance in the text? Based on L450-452, it should be water column respiration plus phytoplankton photosynthesis. This is a very confusing term and the physical transport of oxygen was totally missing. A lot of oxygen studies utilized standard oxygen budget analysis to separate dynamic terms in oxygen change (Li et al. 2014; Scully 2013; Yu et al. 2015). Please refer to those studies on the analysis and consider recalculating/rewriting this part.
- Although this study employed sophisticated machine learning techniques to determine the controlling mechanisms on hypoxia in different regions. It could be actually achieved by oxygen budget analysis, with much clear representation in physical terms (advection and diffusion), rather than relying on stratification indicators. In addition, compared to the manipulating force on DO variability on a seasonal scale, the inter-annual variability is more of interest and worthy to look into.
- The manuscript missed a comprehensive discussion section of advanced understanding of the study topic in-depth and in breadth. The overview of previous observational and model studies in this region, comparison with the current study, what are the agreements and differences, what are the causes, what are the defective aspects of this study, etc. are all important points to include. Expanding the implication to the global context is also valuable to discuss.

Reference:

Murrell, M.C., Lehrter, J.C. Sediment and Lower Water Column Oxygen Consumption in the Seasonally Hypoxic Region of the Louisiana Continental Shelf. *Estuaries and Coasts* **34**, 912–924 (2011). <https://doi.org/10.1007/s12237-010-9351-9>

Rivera, Z. J. Q., Wissel, B., Rabalais, N. N., & Justic, D. (2010). Effects of biological and physical factors on seasonal oxygen dynamics in a stratified, eutrophic coastal ecosystem. *Limnology and Oceanography*, *55*(1), 289-304.

Yu, L., Fennel, K., Laurent, A., Murrell, M. C., and Lehrter, J. C.: Numerical analysis of the primary processes controlling oxygen dynamics on the Louisiana shelf, *Biogeosciences*, *12*, 2063–2076, <https://doi.org/10.5194/bg-12-2063-2015>, 2015.

Li, Y., Li, M. & Kemp, W.M. A Budget Analysis of Bottom-Water Dissolved Oxygen in the Chesapeake Bay. *Estuaries and Coasts* **38**, 2132–2148 (2015). <https://doi.org/10.1007/s12237-014-9928-9>

Detailed comments:

Method

L105-106: are the new features of this biogeochemical model suitable for NGoM?

L108: what is PL? should it be LP (large phytoplankton)?

L120-122: no reactive, labile and refractory category in organic matter pool? In other words, is a single reaction rate enough?

L156: What are ExcZS, ExcZL and ExcZP represented (I could not find those in the Appendix, and guess they should be zooplankton excretion rate to  $\text{NH}_4$ )? Why not include the zooplankton respiration term?

L158-159: How did oxygen inhibition on nitrification and aerobic decomposition rates were calculated? Using Michaelis–Menten formula?

L164-166: how was the portion of sinking PON buried (PONburial) determined? How the initial sediment PON pool was calculated? Is there also an anaerobic layer? Is there any exchange between PONburial and PONsed?

L193: the description of THKbot is confusing. Is it the thickness of overlying water, or sediment layer?

L195: SOC/THKbot is basically the oxygen consumption rate in the sediment. Why not integrate SOC in the hypoxic area and get an overall integrated SOC?

Any observational data validation on the newly added sediment and phosphorus module? In addition to the oxygen concentration validation?

L211: is 5 months enough for spin-up in this area? What is the initial condition (cold start or hot start)?

Biogeochemical model validations

The entire validation is qualitative rather than quantitative. Need statistic metrics to assess the overall model performance, i.e. Taylor and target diagram.

Figure 3: which cross-section was compared in Figure 2b? The difference histogram in (c)(f)(i) is vertically averaged or bottom value?

L287-288: both NO<sub>3</sub> and PO<sub>4</sub> were overestimated

L295-296: this statement is a bit questionable that the high riverine nutrient concentration may not be the cause for the model-observation bias. Because the high concentration of PO<sub>4</sub> and Si(OH)<sub>4</sub> is at the bottom which indicates that it is nutrient regeneration, rather than the allochthonous source.

What are the causes for the hot points (with bottom high nutrient concentration) of PO<sub>4</sub> and Si(OH)<sub>4</sub>?

L303-304: model overestimates DO while also overestimating the recycled nutrient concentration. Usually, it is the opposite case since nutrient remineralization is associated with oxygen consumption. Any explanations?

L331-332: in section 3.4 model validation of oxygen, the result suggested that the model overestimated DO and hypoxia was more frequent in observed WOD profiles. Why here the modeled hypoxia thickness ( $\leq 4\text{m}$ ) is greater than observed profiles?

L336-337: the model showed more offshore extension of hypoxia than observation. Any possible causes?

L346-347: the hypoxia area was separated around 92.5W instead of 91W shown in the model simulation? This may reveal a certain defect in the dynamics of model simulation in oxygen.

L346: the order of figure citation is a bit messy; the figure should be numbered according to the order of citation, not the other way around. For example, the order of **Figure 10** is not optimal for reference.

Figure 7: please adjust the x-axis as the other years for better comparison.

L349: why not include hypoxia area in the water depth  $< 10\text{m}$ ?

L351-352: this means no apparent bias of model simulation in the hypoxia area. How is this model performance compared to other model studies in this region?

## Results

L432: use biogeochemical instead of biochemical throughout the manuscript

L433: denitrification process should not consume oxygen

L453-454: what does it mean by saying contributions are limited? I suggest showing the contribution in percentage. What is DO balance and how it was calculated?

L450-457: the entire description and calculation is misleading and confusing. Generally, all DO budget terms including physical terms, photosynthesis, SOC and WCR should be calculated. The summary of budget terms should match the change of DO. I think the authors did not understand and explain the oxygen dynamics well. Please refer to the model studies with oxygen budget analysis and rewrite this part.

L455-456: does the biochemical process in this sentence represent water column respiration?

L475-476: please indicate the change of PEA quantitatively (e.g. in percentage).

L480-482: west-Mississippi nearshore did not show a change of current direction from westward to southward, rather it pointed to northward.

L498-499: please justify the choice of GBMs method.

L498-511: Move detailed description of GBMs into method section.

Figure 13(a) and Figure 10(a) conflicted in PEA contribution in nearshore West Mississippi?

L540: what does this statement mean? Please clarify it.

L543-544: how does it compare to other model studies? Is this parameterization better or not? Please add a more in-depth discussion here.

L544-548: Figure 10 and Figure 15 looks very similar which is questionable to me. The previous studies suggested that sediment oxygen consumption dominated the hypoxia in the study area, while the water column respiration was still notable.