Comment on bg-2021-142

Anonymous Referee #2

Referee comment on "Biological response to wind and terrestrial nitrate in the western and southern continental shelves of the Gulf of Mexico" by Javier González-Ramírez et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-142-RC2, 2021

The manuscript submitted by González-Ramírez et al. deals with the biological response to wind and river forcing in the western and southern shelves of the Gulf of Mexico. It is based on 21 year numerical simulations using a couped physical-biogeochemical model. I found that the manuscript is globally well organized but would benefit an extensive round of edits. Some portions are quite confused and have to be improved or removed.

The river forcing published by the authors in a previous paper and used in this modeling study constitutes the most innovative part of the manuscript. The lack of published river data was a major limitation for biogeochemical modeling studies on the western and southern shelves. However, there are a number of issues, in particular in the numerical setup, that need to be addressed.

The first major concern is the biogeochemical model used and its description in section 2.2.

The first point is the bottom boundary condition implemented in the biogeochemical model described in Powell et al., (2006) and supposed to mimic remineralization processes in the sediment. The equation (1) implemented into the model is clearly wrong. A simple units check reveals it (left hand side in mol.m^{-3}.s^{-1} and right hand side in mol.m^{-3}). At least some sinking velocities and stochiometric ratios are missing. This error likely have serious impacts on the modeling results presented in the manuscript, especially on the shelves

where the water column is relatively shallow.

The authors should also revised eq. (2). To which time the superscripts on the right hand side refer?

The physical model CROCO is well described and extensively used in a number of studies, but the biogeochemical model requires a more detailed description. Compared to the model described in Powell et al., (2006), the authors are using an additional nutrient and an additional detritus compartment. Could the author detail more why they found necessary to add a detritus group? How the mortality and grazing rates are routed into LDet or SDet? What are the sinking velocities used? The model is relatively simple and the parameters used could be reported in a small table, as table 1 in Powell et al. (2006). Moreover, comparing the equations of phytoplankton in Powell et al. (2006), eq. 2, and in Fennel at al., (2006), eq. 1, I note that phytoplankton does not have a vertical sinking velocity. In consequence, can the authors justify why they add phytoplankton in the remineralization from sediment in eq 1?

The second major concern deals with the validation of the circulation on the western and southern shelves. The authors say that the model manage to reproduce the circulation of the GoM, namely the loop current, loop current eddies, the cyclonic circulation in the Bay of Campeche and the circulation on the shelves (l. 80-84). Since the ocean circulation is an important driver of biology, it is necessary to prove it by showing some figures.

Considering the error in the biological model, it is difficult to trust the results exposed in the manuscript and propose a constructive review. I would suggest that the authors first revise this major issue and update their results. Here are a list of more minor comments and suggestions to help the author in this process:

- l.9: The authors performed a set of two runs, one with rivers forcing and one without. In my mind, this method using the river data already published is the stronger point and the

most innovative part of the manuscript. It can provide solid argument on the river influence on the GoM biogeochemical cycles. I would suggest to emphasize more the method and propose some figures showing the difference between the two runs (the difference in chlorophyll at surface for example).

- l.27: How the advection of low salinity water is associated with winds?

- l.30-31 : "the water is advected over the continental shelf from the TAVE to LATEX region" but these two regions are both part of the shelf. I do not understand this sentence.

- l. 48 : the model has a horizontal resolution of 1/20 degree, which might be around 5 km in this region. However the shelf is particularly narrow (is it well resolved?) and the circulation on the shelf is often associated with small scale currents, especially in the presence of strong buoyancy gradients due to river plumes. The author should discuss which physical processes they expect to resolve.

- l.48 : Are the vertical levels equally distributed or is a stretching function used ?

- l. 53 : Why choosing 2 different set of atmospheric data with different temporal resolution to force the model? The authors are using monthly climatologies for heat and salt fluxes and a product at 6h time step for the wind stress.

- l. 78 : How the others biogeochemical variables are initialized ?

- l. 86 : I cannot see the eddy in the BOC region on figure 4.

- l. 91 : the sentences should be conjugated in the same the tenses.

- l. 91-95 : Where can I see this on the figures ?

- Figure 4: some biaises between model and observations need to be discussed. The model shows much more chlorophyll at the north of Yucatan. Is it due to an intense upwelling circulation lasting throughout the year? Then, the model show very low chlorophyll concentration on the shelf located at the west of Yucatan while satellite images reveal relatively high concentration.

- section 3.2 : I do not really understand why the authors discuss the passage of Loop current eddies. This is quite out of the scope of this manuscript. I appreciate the validation of the vertical chlorophyll distribution but I do not think figure 6 is well suited for this purpose. Why not showing mean seasonal profiles and compared them with observations ?