Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2022-3-RC1, 2022

Comment on bg-2022-3

Anonymous Referee #1

Referee comment on "Hydrodynamic and Biochemical Impacts on the Development of Hypoxia in the Louisiana–Texas Shelf Part I: Numerical Modeling and Hypoxia Mechanisms" by Yanda Ou and Z. George Xue, Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-3-RC1, 2022

General comments:

Overall: The manuscript describes the implementation of NEMURO in a ROMS-COAWST Gulf of Mexico model, including several new features that are targeted at studying hypoxia dynamics in the northern Gulf of Mexico. The main novelty is the inclusion of multiple phytoplankton and zooplankton functional types (from the NEMURO model), phosphorus, oxygen and a benthic layer that can accumulate PON. Using a 15 years simulation, the authors first carry out a validation of nutrient and oxygen, find that the model is able to reproduce the mid-summer hypoxic area and then analyze oxygen dynamics to show that 1) oxygen sinks in bottom waters are dominated by sediment oxygen consumption whereas the role of water column respiration is negligible, 2) hypoxia is controlled by SOC or PEA in the western and eastern part of the shelf, respectively, and 3) there is a quadratic relationship between the hypoxic volume and the hypoxic area, which can be used to predict hypoxic volume from the hypoxic area. My general assessment of the scientific content is that the manuscript lacks originality. There are some technical improvements from other models (see my technical assessment below) but the findings are mostly similar to previous studies using both observations and models, which are cited in the manuscript; the question is then what new knowledge does this study brings on the northern Gulf of Mexico hypoxia? This question should be central in the Introduction and in the Discussion.
Technical assessment: The model developed and used in this study seems appropriate, although I would like to discuss a few points that might need to be revised. These points are discussed in the specific comments below. 1) the main issue is the choice of a fast sinking rate for the particulate organic matter. This choice results in the dominance of the sediment oxygen sinks, which is also a main conclusion of the study. The authors need to validate this part of the model (SOC versus water column respiration). 2) Looking at the results, it is not clear if the model is appropriately initialized/spun up. Hypoxia occurs in deep waters and a long term deoxygenation trend occurs both inshore near the Atchafalaya and offshore. This seems to indicate that PON accumulate in the benthic layer nearshore throughout the simulation and that there is a drift in subsurface oxygen offshore. 3) the model does not include a light attenuation term from river sediment (near the river mouth). This could influence the timing and distribution of primary production over the shelf, and therefore affect the conclusions of the study. In term of model

validation, model results are compared with many nutrients and oxygen data. However the format of the model-observations comparison is questionable and does not result, in my opinion, in a satisfactory validation of the model.
Manuscript assessment: both the Introduction and the Results/Discussion sections need some revisions. The Introduction review the literature of the northern Gulf of Mexico but does not assess what are the gaps in the knowledge. Rather, the authors propose technical improvements, which are welcomed but not sufficient. It is not clear, by the end of the manuscript, if using a more complex ecosystem model is an improvement over previous models. Although previous work is discussed relatively extensively in the Introduction, there is little discussion in the Results/Discussion section. Since similar studies have been carried out before, their results/findings should be compared. It would help to see what is the novelty of this study.

Specific comments:

L25: The rationale/discussion to support your study is not very convincing and also quite vague, you need to provide better arguments that explain why you conducted this research

L33-34: this is true only in a dual reduction strategy

L46-48: All of these authors agree that SOC depends on organic matter in the sediment but because sediment OM is unknown they use a relationship between bottom O_2, bottom temperature and SOC. They assume oxic respiration, which is why they find a direct relationship between SOC and bottom O_2. Justic and Wang (2014) use a sediment tracer that depends on the abundance of deposited OM and is the source for SOC.

L52-53: I don't understand this sentence. SOC would be overestimated at the peak of bloom and underestimated during the post bloom period. This is probably what you meant to say but this is not what I read

L57-58: This is why the models cited previously used a relationship with T/O_2 or instant remineralization. I think what you try to say here is that these earlier parameterizations are not satisfactory and you will try to do better. You should discuss how your SOC implementation will be better than Justic and Wang (2014) because this is the most similar.

L66-68: This is because diatom is the dominant functional group, e.g. Murrell et al. (2014), Lehrter et al (2017). Also, the fact that these models are not a true representation of the reality is not the main point. Here you should point out what these models are doing wrong because of there simple representation of the phytoplankton community and why adding more groups of phytoplankton (and zooplankton) would

improve the representation of oxygen sinks and hypoxia on the shelf. More is not always better.
Murrell et al: Murrell MC, Beddick DL, Devereux R, Greene RM, Hagy JD, Jarvis BM, Kurtz JC, Lehrter JC, Yates DF (2014) Gulf of Mexico hypoxia research program data report: 2002–2007. U.S. Environmental Protection Agency, Washington, D.C., EPA/600/R-13/257
Lehrter et al: 10.1007/978-3-319-54571-4_8

L79-80: there are lots of discussions about the factors controlling bottom O2 in the papers you cited above.

L85: you did not discuss silicate above

L90: what is there to see in the accompanying paper?

L98: do you have sediment transport in your model (since you are using COAWST) and if so, why not having sediment biogeochemistry as in Moriarty et al (2018)

L120: It is obvious why you want to add oxygen but you should discuss the addition of phosphorus, either here or in the introduction

L124: Can you develop? You mean phytoplankton and zooplankton are in N currency, but there is opal, DOP and DON

L126: can you provide a reference, a link to the observations? Would it be possible to get a time series of the observations in a supporting figure (for PO4, POP, DOP, silicate since they are new tracers? Also a map of all the gages would be useful since your model domain is quite large

L129: I don't really understand what are your DOP and POP pools here (see next comment)

L138-139: These terms seem to be just
dDOP/dt = dDON/dt * RPO4N
dPOP/dt = dPON/dt * RPO4N
can you confirm? in this case you only have PO4 in your model

L161-172: please review this paragraph, the clarity could be improved

L163-164: this is the opposite

L164: Note for earlier that the formulations of Hetland and DiMarco (2008) and Lehrter et al (2011) include temperature, which mimics the delay because warmer water occurs after the peak of production

L180-187: this is a bit difficult to follow, could you make it easier?

L181: How come M is expressed in m-3 since it represents the integrated OM decomposition in the sediment. If you express it in m-2 you can remove the THKbot terms which simplifies the equations

L192: Do you use the same expression for the water column respiration?

L199: do you have anaerobic respiration occurring in this case and if not, why?

L211: although this seems fine for the plume region, it seems very short for the entire GoM and may influence you results as the interior GoM is still adjusting during your analysis period. The fact that hypoxia occur>100m later on suggests that this is the case. Also you need to show that your sediment layer reach a seasonal steady state (later on it seems to accumulate throughout the simulation near the Atchafalaya). How was the benthic layer initialized? can you provide a time series of PONsed?

L226-230: do you do any nudging toward HYCOM or any other climatological product?

L240: can you also show the other rivers for completeness?

L245-246: can you elaborate on this assumption?

L250: it is indeed highly oversaturated. can you provide some context?

Figure 2c: The shelfwide surveys were not available prior to 2012? see here: https://coastalscience.noaa.gov/project/integrated-ecosystem-modeling-causes-hypoxia/

L296: this is not a good comparison, you should provide histograms for surface data is spring, summer, winter. A 1:1 comparison would also be more meaningful because it would show where the mismatch occur (at low, high concentrations? in the bottom, at the surface?)

Figure 3c,f,i: this pair comparison is a bit misleading because you mix all data. Subsurface NO3 should be relatively small, resulting in a good agreement, but there could be significant mismatch at the surface. It is at the surface that a good representation of NO3 is important because that is where primary production occur

L301: Same comment here, I don't think this is a proper way to validate the model. Also, what about chlorophyll?

L283: I don't understand your choice of model data comparison. Are you binning the profiles by bathymetry? This assumes that the variability occurs from shallow (north) to deep (south) regions whereas the variability should be from upstream (east) to downstream (west). Also looking at Figure 3b it looks like vertical profiles of nitrate are uniform even though high nitrate at the surface (within the plume) is expected. Another issue is that you are mixing all times together. Your observed nutrient dataset is relatively short so you could make a better comparison, surface and bottom maps for example at key periods of the year
Regarding PO4, high values are mainly found near the bottom, which suggest that the main source of PO4 is from resuspension events rather than from the river. Can you justify these patterns?

L315: the data are available, see earlier comments. These data also include nutrients which could be used in complement of WOD

Figure 4c,f,i: I assume that some differences are much larger than 50% because if the model is normoxic and the observation hypoxic (or inversely) the bias could be several hundred percent

Figure 4h: Aren't SEAMAP cruise occurring in late spring rather than summer?

L335: I don't know why the model data <10m are not shown in Figure 6, these data should be available to the reader

L336: this is not true for the area off the Atchafalaya, observations are available there

L337: 2017 as well. Can you comment on the occurrence of hypoxia around 100m (near the slope). Is that an issue in the model, i.e. does that influence hypoxia on the shelf?

L349: why 10m? I agree that you should exclude the Atchafalaya Bay but you should include the coastal area. Also, you should have a more restrictive longitudinal extent because the observations are always <94.5W

L349-353: In some years the model simulates a relatively large hypoxic area in June, sometimes also in May, do you think this is realistic? Are the SEAMAP data showing similar conditions?
Also, bottom waters don't always get fully reoxygenated in July-August in years with tropical storms/hurricanes, e.g. 2018-2020. Can you comment?

Figure 6: 1) Another way to make this comparison would be to overlay the observations as scatter points over the model maps
2) hypoxia varies rapidly and it might be better to show a mid-cruise map from the model rather than a ~1 week average
3) can you show the other years for completeness?

L364: you use a mixed format for Results and Discussion but then you do not discuss much your results with respect to the literature

L375: I don't quite follow this analysis, what does it mean?
It looks like there is a long term negative trend in the Atchafalaya plume and offshore. The 2 signals could be problematic: the Atchafalaya plume signal indicate that PONsed accumulates there during the simulation and the offshore signal seems to indicate that there is a drift in offshore subsurface O2 or that the offshore part of the model is still adjusting
Note: you don't have resuspension in your model. Can you justify your choice? this feature would be easy to implement and would provide a realistic distribution of SOC over the shelf. This may also prevent the accumulation of PONsed near the Atchafalaya.

L380-390: can you compare these patterns with the literature?

L385: This is surprising that you find substantial hypoxia in a monthly climatology. This means that 1) hypoxia almost always occur at this location during that month (as shown on the right panels) and/or 2) bottom O2 concentrations are low at these locations, well below the hypoxia threshold.

L408: see earlier comment about the long term trend

L450: also vertical diffusion and possibly horizontal advection, as well as SOC

L456-457: you should compare your results with these. For that you should integrate respiration over the subsurface layer (or lower 4m for instance). You could also discuss your results with respect to other budgets, e.g. Yu el al (2015)

L477-478: this is not obvious

Figure 12a,c,e,g: I think the time series in Figure 10 were enough. I don't find these PEA maps very useful

L498-511: this paragraph should go in the Methods section

L513-527: other authors found that water column respiration is not dominant but not negligible either (Lehrter et al, Yu et al), can you comment on that? Is the large dominance of SOC in your model due to the set up of your model, high settling rate for instance?

L517: yes, this is were you find persistent hypoxia

L522: where is this shown? you speculate here

L524: +10% would be a more conservative value and used for climate projections in the region, e.g. Lehrter et al 2017.

L525: you speculate here

L543: ah yes, that explains the very low water column respiration, see earlier comment. In the Atchafalaya nearshore, PON settles instantly to the bottom and accumulate which explains SOC and hypoxia there. I think this is problematic as your model setup drives

your conclusions. This brings up two points: 1) you should validate your choice of high settling rate. For instance if surface nutrients, surface chlorophyll, water column respiration and SOC compare well with the observations/literature then your choice is fine. If not then you may want to recalibrate your model. 2) if PON sinks rapidly to the bottom and water column respiration is not significant, then why do you have 3 functional types of zooplankton?
Note: with this type of model setup the predatory zooplankton tend to have a top-down control over primary producers, is this the case in your system and is this why the sinking rate is so high, to escape this control?

L543-555: I don't get the point of this paragraph

Figure 15: I don't get the point of this figure

L569-570 (see also earlier comment): Given your fast sinking environment it seems that a single functional group for phytoplankton (diatom) and zooplankton was enough in you study of the LATEX shelf. A more convincing argument for your model choice would be that it is needed for the open ocean part of your domain (if indeed it is)

L571: P limitation: you did not show that either

L572-573: this was the main novelty of this work. However, model tuning may be necessary to properly reproduce water column respiration (see also earlier comment)

L573: you did not show that, see earlier comments

L627-628: The model does not include a light attenuation factor for terrigenous material near the river (dependant on salinity for instance)? Light limitation is strong near the Mississippi and Atchafalaya River mouths but this light limitation effect is not included in your model. This lack of light limitation would result in high primary production near the river mouths and less production downstream, thereby influencing the timing and distribution of phytoplankton, respiration and bottom oxygen over the shelf
Also (and L638), why is PAR different for small and large phytoplankton? shouldn't it be the same, each functional type having a different sensitivity to light? Looking at your parameter table I see that you are using the same value for both so effectively there is no difference in PAR

L650: L650: did you mention how these parameters were chosen? were they calibrated to the Gulf of Mexico?

Minor comments/typos:

L1: "impact" is not the right wording

L30: shrinking is not the right word, reduction is better. Please rephrase the sentence accordingly

L34: "shrinkage" is not the right word, may be "reduction"?

L34-35: replace with: "Transient phosphorus limitation on the shelf (Laurent et al 2012; Sylvan et al 2007) was deemed..."
Sylvan et al: 10.4319/lo.2007.52.6.2679

L35: "with the delayed onset and reduction of the hypoxic area"

L39: Conley et al 2009 is not related to the LATEX shelf

L56: "coupled"

L93: you could mention your main results here.

L123: I don't think you need this reference as this formulation is wide spread. However you could mention that you use the same formulation as for the other nutrients

L162: please rephrase, the sentence is not complete

L332: I agree but you could mention the underestimation of the hypoxic layer

L345-346: you did not introduce Figure 7 yet

L377: this makes sense, the STDs are larger in the plume region where hypoxia occurs

L381: that seems normal since the hypoxic area is calculated from bottom O2

Figure 8e: the DO scale is a bit misleading
Why do you show bottom oxygen up to 100m in Figure 6 but then limit the output to 50m in Figures 8-9, 11-12?

L400: yes because the extent is a climatology (see comment above)

L414/446 (and elsewhere): "trough": minimum may be a better word (elsewhere as well)

Figure 9: can you show the results for the coastal area when you show maps?

L448: "also water stratification (Figure 10)."

L450: be more accurate, here you talk about water column processes

Figure 11: Since you don't compare modeled SOC with observations it would be easier to keep the original units

L468: Note that the maps show a nearshore/offshore gradient in PEA, following the bathymetry. This is due to the multiplier z in the PEA equation, which increases PEA with increasing bathymetry

L471: may be 1 reference is sufficient here?

L537: replace "low" by small

L568: "the NEMURO model"