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Interactive comment on "The Role of Sediment-induced Light Attenuation on Primary Production during Hurricane Gustav (2008)" *by* Zhengchen Zang et al.

Anonymous Referee #1

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General Comments The paper is in itself interesting and promising. It is easy to read, however, there are many grammar errors, most of which are missing articles or mismatches in tenses. A professional spellcheck is definitely recommended. Some figures need remastering.

The authors make an interesting case for the specific case of a hurricane making landfall. Their work is certainly relevant to the general topic. However, I am going to be very critical of certain aspects. Unfortunately, the authors missed a few chances to make an outstanding publication. Nevertheless, the paper will still be very good when the criticism is addressed.

The experiment design is not flawless. It is not explained why a 20year run was necessary to achieve a stable initial condition, which itself is not shown or validated against data at all. Then, the model starts a few days before the hurricane makes landfall, but it would have made more sense to start a week earlier to show what a “normal” state of the ecosystem in that time period would have looked like. Also, to show whether the model actually recovers to said “normal” state, the model should have been run a week longer after it actually ends. While this would have taken more time, a 20y run to spin up the ecosystem was strictly speaking not necessary. 2-5 years are common in literature, and setting this up is literally an issue of an hour, if the forcing is already available. More experiments could have been performed, varying a_sed in smaller increments. CDOM specific attenuation could have been included via an inverse relationship with salinity to make the sensitivity study more representative.

While all of these points may have improved the paper, I, however, do not think it is necessary to repeat the entire study in the same way as I suggest, because the methodical point comes across that higher sediment specific attenuation shifts a regime from being nutrient limited to light limited. See my more specific comments below. It is of course free to the authors to consider any of my suggestions for improvement here made and to perform additional runs.

Section 1 This chapter is well written and gives a good overview of the subject.

In line 72, parameterisation approaches to sediment specific attenuation are discussed. One might mention the often strong correlation between sediment content and bathymetry, as employed or discussed e.g. in Zhou et al. 2017 (https://doi.org/10.1016/j.pocean.2017.10.008) and Thewes et al. 2020 (https://doi.org/10.3389/fmars.2019.00816). Their approach has the disadvantage of not being time variant, making a stronger case for online modelling of sediment.

Section 2.1 This section heavily relies on previous publications, which is fine. However, this way, a few bits of information fall under the radar, like how many different sediment types there are. Also, it is possible and not too difficult to program a specific coefficient for each sediment type. An explanation as to why this was not done is needed.

In line 110, it says the model was “largely built on” NEMURO. Other than the addition of a sediment specific attenuation term, has anything else been changed?

Because passages in later sections are on the subject of hypoxia, NEMURO might not have been an ideal choice. Perhaps it should be explained why NEMURO was chosen over models that have been coupled to ROMS and published before, which calculate oxygen as a state variable.

There is a very important error in the equation in line 132. Because plankton biomass and sediment content are functions of depth, the equation must include integrals of PSn + PLn and SSC over depth . Section 2.2 The authors should explain why they selected the specific values of a_sed that they chose. Are the studys to which they refer conducted in the same region? Are they at all comparable to the model situation?

Section 3 The authors have conducted a 20y run to obtain an initial condition, yet the initial condition is never shown or validated against data. Because the 20y run is not the object of the study, it needs no validation, but the initial condition certainly does. This could be an extra panel in figure 3.

RMSE and R were computed. For this to be done, one of the data sources would have to be regridded to match the other. Was satellite data or model data regridded?

Figure 4 shows logarithmic values for SeaWiFS and model data. Were RMSE and R calculated using the actual data or the logarithmic data?

In line 174 it says that the “model’s performance was significantly improved in high productivity waters where chlorophyll concentration is >1mg/m^3”. Does this refer to satellite or model data?

Section 4.1 NPP is not defined in the text. Although there are literature definitions, this should be explained.

Why is there a 3-4hour delay in chlorophyll with respect to NPP? Are biomass and chlorophyll uncoupled? Do different species have different C:CHL ratios?

In line 197, it says that surface cooling and decreased light contributed to reductions of chlorophyll and NPP. Can you identify the individual contributions? From fig. 5b, it seems that surface cooling limits by about a factor of 0.5 and light limits by up to 0.3 at the maximum. This should be calculable just by putting in representative values in the respective equations for light and temperature limitation. Also, it says reduced temperature and light availability "contributed" to the reductions. What else might have contributed? The first peak in figure 5a is significantly broader than most of the following peaks. After that, they seem to have an almost bimodal quality. This is not addressed in the text. Is this due to the different species? It is true for both runs. The NPP in test 1 recovers almost immediately to the same peak value, albeit narrower. Chlorophyll does not really recover to pre-storm values at all, but the benchmark run does. Again, is this due to speciation?

I suggest to separate contributions of the individual plankton species to both NPP and CHL. It looks to me as though one of the two phytoplankton species is more susceptible to light or temperature limitation than the other. If the run had been longer by a week in both directions, one might have seen a full recovery to a "usual" state after the hurricane (i.e. the broad peak in NPP on the 30th of August). It looks like the benchmark run recovers faster to that broad, supposedly "normal" peak, while test 1 shows a quicker recovery in the leading peak. It may, as stated in the manuscript, be largely due to the boost in NO3. However, temperature and light sensitivity might be different for the two phytoplankton species. This should be disclosed.

NPP is a depth integrated quantity, but only surface chlorophyll is shown. There is no info on lower layer productivity. What is the vertical structure of phytoplankton, or rather, does it change when switching on sediment specific attenuation?.

Section 4.2 Figure 6 is very illustrative. It might be helpful to have two more panels showing NO3. In that case, perhaps rotate orientation to columns <-> time and rows <-> parameter.

In the second paragraph of this section, the authors discuss hypoxia. However, NEMURO does not provide oxygen output. Then, the situation in the nGoM is compared to the Delaware estuary. It would be prudent to show that these areas are at all comparable, which they are likely not, because an estuary is usually bounded horizontally and is characterised by strong lateral salinity gradients. The Delaware estuary experiences hypoxia due to density stratification. There is no figure representing temperature or salinity stratification in the nGoM model, but only chlorophyll and SSC. The latter strongly influences the former and SSC stratification is perhaps purely due to sediment settling. What is implied in the text is that the chlorophyll stratification is due to a density stratification, and that phytoplankton does not reach the lower layers, because it is physically bound. There needs to be a figure showing temperature and salinity, or density and stability frequency, to be able to imply a similar situation as in the Delaware estuary. In line 237 of the text it says that "post-hurricane stratification recovery" prevented oxygen ventilation to the bottom. Is this with reference to the model in the study? Please show stratification along the transect. Perhaps show some in situ data of oxygen from that time period, to show that there actually was a hypoxia event after Gustav. Otherwise, consider removing the paragraph.

Section 4.3 This section does make a good attempt at explaining the offshore bloom, but it really would strongly benefit from the previously recommended inclusion of panels in fig. 6 that show NO3, with yet even more panels, showing daily averaged velocities along the transect D, which is almost perpendicular to the 50m isobath. My suggestion is to reorganise fig. 6 thusly: make the columns represent time and stick with three of them (left column: 31st of August, middle: 2nd of September, right: 10th of September). Then plot as rows the following variables: chlorophyll (test1), chlorophyll (benchmark), SSC (those first three in that order), NO3, density or temperature and salinity, circulation along the transect (i.e. colours as magnitudes and arrows as direction). This would

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in my opinion massively improve the theses of this work.

Section 4.4 Although it is clear from a modeller's perspective and from a methodical point of view why the authors decide to do a sensitivity study with regards to sediment specific attenuation, the explanation that this is to accommodate for the way different types of sediment attenuate light is vague. It is true that different types attenuate differently, but then it appears more reasonable to simply compute the model with one coefficient for each sediment type. The programming effort is a day's work at best. The researching effort to get plausible values might be a little more work, but all in all it is not clear why this has not been done. It might be worth an explanation or at least an elaboration as to what prevented the authors from doing that. Again, it is a perfectly reasonable approach to perform a sensitivity study over a varying singular attenuation coefficient, yet the explanation lacks context. Why were the specific values chosen? They were taken from referenced studies, but are the sediments in these studies comparable in their make up (see comments to section 2)? Why not linearly vary around a reasonable value by 20% (this is almost the case anyway)? Why not do 5 tests instead of 3?

Figure 7 hints at he chaotic nature of the ecosystem by showing how a small change in an initial state can alter the following development. Even though after the 6th of September, SSC at the surface was almost zero, the benchmark and test 2 deviate more from each other after that day than they do before. This is perhaps an interesting point to make.

At the end of the second paragraph of this section, the authors say that in the last two days, the ecosystem had shifted back to a nutrient limited one. While this may be true and seems reasonable, it would again be helpful to have numbers on the limiting factors.

Ideally, for all runs with sediment attenuation, the light limitation should be identical over time. However, given the chaotic nature of turbulence, an elaboration as to whether or not the individual run's SSC deviate from each other in any way is missing. It is not expected to deviate heavily, but a note on whether or not there is deviation is appropriate.

In line 290 it says that the study does not provide a widely accepted SSC range to determine whether a_sed plays a vital role in photosynthesis and primary production [. . .]. This is somewhat confusing, as a_sed clearly plays a vital role. If it is 0, the model output looks drastically different in some regards. I am not sure what is meant by a "widely accepted SSC range". Does that mean that a certain value of a_sed may be valid for a certain range of SSC? If so, please rephrase the sentence to make this clearer.

The authors further speculate in line 296ff that there might be variation on possibly annual to decadal scales. To emphasise the validity of the claim, as well as the importance of the subject, the following studies on the North Sea might be of interest and inspiration: Dupont & Aksnes 2013 (https://doi.org/10.1016/j.ecss.2013.08.010), Capuzzo et al. 2015 (https://doi.org/10.1111/gcb.12854) and Wilson & Heath 2019 (https://doi.org/10.5194/os-15-1615-2019). It might also be noteworthy that hurricane events are predicted to occur more frequently under the changing climate. Implications of this could be lowered primary production.

Section 4.5 The authors are right in addressing the lack of CDOM representation, and they correctly point out that many models share the same weakness. However, as they also point out, salinity is often used as a proxy for CDOM via an inverse relationship, which is well documented and easy to implement. It does make the paper more complex, but it might have been worth the effort, specifically when doing a sensitivity analysis with respect to when an ecosystem shifts from a nutrient limited to a light limited regime. The CDOM contribution would undoubtedly have biased the results of the sensitivity test. It is therefore appropriate to include a sentence or two in the discussion, explaining that CDOM specific attenuation would likely shift a hypothetical threshold a_sed downwards, below which the regime may be considered nutrient limited.

Section 5 The authors say in the first sentence of this section that they "introduced a sediment-induced light attenuation algorithm to the coupled physical-biogeochemical model on the platform of ROMS", which is, strictly speaking, not true. They did introduce it to NEMURO, however, ROMS still uses a different light attenuation scheme. See e.g. Mobley et al. 2015 (https://doi.org/10.1002/2014JC010588) or Cahill et al. 2008 (https://doi.org/10.1029/2008GL033595) to find what the not insignificant difference is. There is no coupling between sediments and light absorption in the physical model.

In line 367, the authors again claim that the post hurricane situation might have caused a hypoxia. Without any data, this is purely speculative and does not belong in the conclusions chapter.

Minor comments See attached documents for grammar errors and other minor comments.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2020-58/bg-2020-58-RC1-supplement.pdf

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