

Interactive comment on “Seasonal Patterns in Phytoplankton Biomass across the Northern and Deep Gulf of Mexico: A Numerical Model Study”

by Fabian A. Gomez et al.

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General: This manuscript describes a new biogeochemical model for the Gulf of Mexico. The model results are analyzed with focus on seasonal variations in phytoplankton biomass, primary production, and relative abundance of diatoms versus small phytoplankton in the northern shelf region that is influenced by the Mississippi River and in the oligotrophic open Gulf. Previous regional modeling studies for the Gulf used simpler biogeochemical models that only include one phytoplankton functional group, so this study is a welcome extension. However, I have three major concerns that need to be addressed before I can recommend publication. These are related to model vali-

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dation, the terminology used in describing model results, and a slight tendency by the authors to oversell their results while diminishing previous studies. These concerns are described in more detail below.

1) With regard to validation:

1.1) The authors provide no validation of the physical model. If there are previous publications in which this is reported, it would be fine to refer to those. Otherwise some physical model validation should be provided.

1.2) On page 6 (first paragraph) the authors state that the model underestimates mean satellite chlorophyll by factors between 2.5 to 3. These are rather large deviations in the mean. They then give reasons for why the satellite can't be trusted. There are two problems with this: first, the simpler models of Fennel et al. (2011) and Laurent et al. (2012) reproduced satellite chlorophyll without such large biases; and, second, if chlorophyll cannot be trusted it shouldn't be used in validation. However, satellite-derived chlorophyll is essentially the only data set that is used in this manuscript to validate the model.

1.3) There should be some validation of the biogeochemical model with in situ observations of phytoplankton biomass and nutrients. Such observations are available in the NODC and GOMRI databases. Profile comparisons for the open Gulf should be included.

1.4) Perhaps the largest omission, given the objective of the study, is that there is no validation of the different phytoplankton groups. I recognize that it is hard to get good data sets for this purpose, but there are some algorithms that can be used to separate satellite-derived chlorophyll into different size groups (see Hirata et al. 2011, Mouw et al. 2017 and references therein).

2) With regard to terminology: In section 3.4 the authors define the "biological term" as the balance between phytoplankton production and biological losses. This is the same

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as Net Phytoplankton Growth, a widely used term in biological oceanography. It is not only unnecessary to redefine this as a new term, but also potentially confusing. Then the authors state that the balance of the biological and physical terms determines the change in net phytoplankton growth. This is wrong. Net phytoplankton growth is equal to what they defined as the biological term. The balance between the biological and physical terms is the local rate of change of phytoplankton.

3) In the following instances the authors should be more accurate in describing their results and the context in the existing literature:

3.1) In the last sentence of the abstract, they claim that their study shows the importance of representing large and small plankton in order to describe PP patterns. This is not supported by the results presented. On the one hand, there is no validation of the contributions of large and small phytoplankton to biomass and PP (see 1 above). On the other hand, there is no comparison to simulated phytoplankton abundance and PP from a model with only one phytoplankton group. Simpler models exist that, in fact, reproduce chlorophyll from satellite more accurately than this model in the Mississippi plume region (see comment 1.2).

3.2) In the second to fourth sentences of the discussion the authors make statements about their results that are not supported. “Inclusion of two phytoplankton components allowed for realistic representation ...” is not accurate as simple models arguably reproduced this better (see comment 1.2). “The good agreement between model outputs and observations of chlorophyll ...” is a questionable statement (see again comment 1.2).

3.3) With respect to phosphorus (P) the authors seem to be diminishing previous findings in an effort to justify why their model does not include P. On page 3 (line 8, sentence starting with “Although...”) they seem to suggest that previous studies (specifically Laurent et al. 2012) suggest P limitation to be unimportant. This is not the conclusion of Laurent et al. (2012) nor of the follow-up studies by Laurent and Fen-

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nel (2014) and Fennel and Laurent (2017), which are consistent with the observational studies by Sylvan et al. (2006, 2007). All these studies do suggest the P limitation is critically important in the region influenced by the Mississippi River plume. Saying that P limitation is “moderate” while N and Si limitation are “critical” seems disingenuous. To be clear, I do not object to the fact that P is neglected in this model. All models are simplifications. It would be fine to state that their model neglects P, although it has been shown to be important in a portion of the model region. In the Discussion (end of first paragraph) it would be appropriate to be more forthcoming about previous studies on P limitation.

3.4) The statement in the Discussion (last sentence starting on page 11) about consistency with the dilution-recoupling hypothesis of Behrenfeld seems a bit cavalier. No detailed analysis in support of this statement was presented in this manuscript. The authors may want to consider the study by Kuhn et al. (2015), which used the same data set as Behrenfeld, and later papers by Behrenfeld where he backtracked himself somewhat from his early paper (Behrenfeld et al. 2013).

Other comments (not in order of importance):

4) P1, Line 13: Suggest inserting “improving” after “tools for”

5) P1, Line 14: Suggest removing “However”

6) P1, Line 19, sentence starting with “The model results show . . .” and following sentences in the abstract. Because diatoms in the model are strongly silica-limited doesn’t necessarily mean they are in reality. Making inferences about reality from the model requires that the model accurately reproduces reality, which in this case is hard to prove. The authors certainly haven’t (see my comments about validation). I would suggest that here and throughout the remainder of the abstract and manuscript the authors are more precise in their language. It is fine to say “diatoms in the model are silica limited” or some variation thereof. And “Simulated nanophytoplankton are . . .” rather than “Nanophytoplankton are . . .”

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7) P1, Line 27: Suggest replacing “vertical diffusion” with “turbulent vertical diffusion” or “vertical mixing.” Diffusion typically refers to molecular diffusion which acts on too small scales to make any difference to the processes considered here.

8) P1, Line 27, sentence starting with “This study highlights the . . .” This is an overstatement not supported by the results actually presented in this manuscript. See major comment 3.

9) P2, Line 9: “. . .because of deleterious impact on coastal ecosystems.” The authors should provide one or more references in support of this statement, or modify it. I would like to challenge them to find a study that shows deleterious impacts on the ecosystem in the northern Gulf of Mexico (I am not aware of one). There are studies about specific aspects of the ecosystem, which would be fine to cite if sentence is slightly modified.

10) P3, Line 5, sentence beginning with “New modelling efforts . . .” I object to the logic of this statement. Adding complexity to biogeochemical models is not in itself a worthwhile undertaking. It has to be motivated by the scientific questions (e.g. one might be interested in species succession). Sentence should be reformulated accordingly.

11) P3, Line 8: “. . .diatoms require . . .” Citing a modeling study (Kishi et al.) in support of a general statement about diatom traits seems inappropriate. There are more appropriate references. I suggest the authors look up publications by Elena Litchman and collaborators. She has worked extensively on documenting phytoplankton functional traits.

12) P4, Line 30: Which basin does “basin-scale” refer to here?

13) P5, Line 18: “. . .randomly selected year” Which year?

14) P5, Line 23: Stating the model “reproduces” the observations is an overstatement. It would be more appropriate to say they agree qualitatively.

15) P5, Line 31: The authors should make it much more clear upfront that these are anomalies (i.e. that the bias was removed).

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16) Results, general: No oxygen results are shown. Given this, there is not much point saying the model includes oxygen.

BGD

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