

***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.**

Anonymous Referee #2

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General comments:

This paper investigates the spatial and seasonal patterns of phytoplankton biomass in the Gulf of Mexico (GoM) using a three-dimensional biogeochemical model that could explicitly simulate small- and large-size plankton groups. The authors demonstrate that the model could reproduce the satellite observed dominant seasonal patterns of phytoplankton biomass in GoM and explore the underlying mechanisms controlling the seasonal variability. This work is of interest to the community and would complement previous modeling work to improve understanding of phytoplankton dynamics in GoM. However I have a few concerns, as listed below, but subject to these being addressed

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I would recommend the paper for publication in Biogeosciences.

1. My major concern is associated with the validation of the coupled physical-biogeochemical model:

First, there is no physical validation presented in the paper, despite that the authors have emphasized the importance of physical processes on the net phytoplankton growth. Has the physical validation work been done and/or published elsewhere? If yes, it is important to summarize that here in some way. If not, I think it's worthwhile to do some extra work on physical validation to make the presented results here more convincing considering how important the physics is controlling the biogeochemical cycling in this region (e.g., the mixing and transport by riverine waters to northern GoM, Loop Current and eddy interactions to deep GoM, etc.). For example, the simulated spatial extent of the high chlorophyll river plume in northern GoM is narrower than that observed in satellite (visually viewed from Fig. 2), could it be associated with the distant transport of riverine nutrients?

Second, the validation of biogeochemical (BGC) model doesn't seem sufficient to me. The BGC validation in the paper primarily relies on comparing model simulated and satellite observed surface chlorophyll. While the model overall reproduces the dominant seasonal and spatial patterns in satellite chlorophyll, it significantly underestimates the coastal chlorophyll both in magnitude (2.5-3 times lower in the model) and spatial extent. The authors attribute the mismatch to satellite overestimating in situ observations of chlorophyll in northern GoM. If true, it would be useful to also include comparisons between simulated and in situ observations of chlorophyll in the paper for justification. In addition, while satellite chlorophyll observations have the advantage for model validation due to its spatial and temporal coverage, they are limited to the first optical depth that could hardly represent the plankton dynamics in subsurface water (e.g., the deep chlorophyll maxima). Hence a good complement to the validation might be including comparison to chlorophyll profiles, which to my knowledge is available in GoM during the model simulation period (e.g., the bio-optical profiling float results presented

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in Green et al., 2014). Also, there are relatively ‘abundant’ observations, apart from chlorophyll, in the northern GoM, such as those provided by Mechanisms Controlling Hypoxia (MCH) program (<http://hypoxia.tamu.edu/field-program>), in situ observations of primary production (Lehrter et al. 2009), and water column community respiration rates (Murrell et al. 2013). These datasets might improve the BGC validation in coastal region where satellite chlorophyll is considered to have higher uncertainty.

2. One novelty of this work is that the model includes two phytoplankton types and two zooplankton types that complement the previous modeling work in GoM that mostly only includes one phytoplankton and one zooplankton type. While the additional complexity added to the BGC model is more faithful in representing the lower-trophic level dynamics in real system, it also adds more complexities and challenges in calibrating and validating the model. With respect to calibration, have the parameter values shown in Table 1 (especially those with *) been informally or formally tuned or optimized? Are the conclusions presented here sensitive to the selected parameter values? I think providing more information/comments on these would be helpful to others. The additional complexity of the BGC model also adds difficulties in model validation, e.g., the model-data chlorophyll comparison alone cannot tell how reasonable the model simulates each type of phytoplankton group as it could not distinguish the contribution from small- and large-size phytoplankton groups. How has the added complexity benefit us to understand the plankton dynamics in this region? Does the presented model do a better job than the previous modeling work that only include one phytoplankton type (e.g., compared with Xue et al. 2013)? I think readers would appreciate with a bit more discussions/comments on these.

Specific comments:

1. Page 4, Line 6: Would it be more appropriate to list an observational rather than a modeling work (Xue et al., 2013) as a reference?
2. Page 4, Line 14: delete one ‘to’ either in front of the ‘:’ or after the number.

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3. Page 4, Line 22: Why listed MODIS SST here? Has it been used anywhere in the paper?
4. Page 4, Line 28: Horizontal diffusivity is non-zero here, but it seemed to be neglected when analyzing the role of advection and diffusion in section 3.4.
5. Page 4, Line 30: Does the basin-scale model also include biogeochemistry and provide BGC initial conditions? If not, how do you specify them? Could you also provide more information on how you specify open boundary conditions? Has tide been included?
6. Page 5, Line 18: Where were the boundary conditions and surface fluxes extracted from? the basin-scale model?
7. Page 6, Line 23: 'mean production values', is it spatial or/and temporal mean? Maybe also provide the standard deviation if available, since the primary production is highly variable?
8. Page 7, Line 29: change 'ranges' to 'range'?
9. Page 8, Line 26: In the text, it's switching between 'summer' (or winter) and 'months' back and forth. Could you specify the summer and winter months at the first time they appear?
10. Page 10, Line 22-24: This statement is a bit exaggerated to me since the validation is on chlorophyll, a combination of two phytoplankton groups, that how well each type of phytoplankton is simulated by the model is not directly validated.
11. Fig.2: the lower limit of the color bar is missing? From 0? What does the gray contour line represent? 200m isobath?
12. Fig.8: should be '...in panels a-b depict ...'

References

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Green, R.E., A. S. Bower, and A. Lugo-Fernandez (2014), First autonomous bio-optical profiling float in the Gulf of Mexico reveals dynamic biogeochemistry in deep waters. *PLoS One*, 9, 1–9. doi:10.1371/journal.pone.0101658

Lehrter, J. C., M. C. Murrell, and J. C. Kurtz (2009), Interactions between Mississippi River inputs, light, and phytoplankton biomass and phytoplankton production on the Louisiana continental shelf, *Cont. Shelf Res.*, 29, 1861–1872, doi:10.1016/j.csr.2009.07.001.

Murrell, M. C., R. S. Stanley, J. C. Lehrter, and J. D. Hagy (2013), Plankton community respiration, net ecosystem metabolism, and oxygen dynamics on the Louisiana continental shelf: Implications for hypoxia, *Cont. Shelf Res.*, 52, 27–38, doi:10.1016/j.csr.2012.10.010.

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