

# ***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 #3**

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The paper by Gomez et al presented a coupled physical-biogeochemical modeling study for the Gulf of Mexico. By combining the NEMURO NPZ model by Kishi et al., 2007 and the biogeochemical model by Fennel et al., 2011, the author tried to introduce a more complicated nutrient-low trophic level model to the Gulf of Mexico, which is plausible. Nevertheless, I totally agree with the major concerns from the other two reviewers, the study presented so far failed to demonstrate their model's credibility for either the physical or biogeochemical part. Given these model validation been provided, the paper failed to prove the benefits of introducing this multi-plankton group lower trophic level model. My major concerns, alike those of the two other reviewers', are as follows:

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1) Validation of the physical model. The paper stated that the boundary conditions were from a HYCOM model, yet the model (ROMS)'s own performance regarding circulation and T/S fields was not evaluated, without which, I would have a big question mark about the results presented in the manuscript; 2) Validation of the biogeochemical model. The author evaluated their model's performance via a comparison against satellite data and admitted that their model underestimated the Chl-a. And unfortunately, these satellite data were the only source used for model evaluation. How about the model's performance on nutrient and plankton groups? Without such information, it is hard to conclude that the model could at least represent the nutrient and biological cycle in the Gulf; 3) Given that point 1) and point 2) were addressed, I could not find the benefit of introducing the new plankton group (2 phytoplankton and 3 zooplankton vs. 1 phytoplankton and 1 zooplankton by Fennel et al. 2011), which, indeed, could be the most important contribution of this study.

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