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# *Interactive comment on* "Quantifying spatiotemporal variability in zooplankton dynamics in the Gulf of Mexico with a physical-biogeochemical model" *by* Taylor A. Shropshire et al.

**Anonymous Referee #3**

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Shropshire et al. used the NEMURO biogeochemical model (Kishi et al., 2007) to describe spatiotemporal patterns in zooplankton biomass across the Gulf of Mexico (GoM). They made a series of changes to the original NEMURO formulation and parameterization to represent better low trophic level dynamics in the warm waters of the GoM. They used the MITgcm offline tracer advection package, which allows running the biogeochemical model offline, using existing model outputs from ocean circulation models. This significantly increases the model time step compared to online-coupled models, thus reducing the required time for model simulations.



Comparisons between observed and simulated patterns of zooplankton biomass show good agreement. Also, there is a good correspondence between simulated and observed surface chlorophyll patterns in the open GoM. However, the model does not reproduce realistic coastal chlorophyll patterns at surface, and important differences between model and observed rates for grazing, primary production, and specific growth are reported.

I have the following main concerns that need to be addressed before I can recommend publication.

1) A validation for nutrient's patterns should be included to gain additional confidence in the model results.

2) The model is not able to reproduce surface chlorophyll patterns in the coastal regions (bottom depth <1000 m). Is this a consequence of the model parameterization or a misrepresentation of river runoff fluxes? Assuming that all rivers have the same nutrient concentration than the Mississippi river is wrong. The USGS have data that should be considered to better constrain the land-ocean nutrient fluxes.

A mean time series for all the coastal regions in the GoM does not seem to me appropriate, because there are important differences among shelf regions. I would suggest an independent comparison for the shelves off Louisiana-Texas, Mississippi-Alabama, west Florida, and Yucatan.

3) The authors reported a good correlation between model and observed vertical profiles of chlorophyll. But a good correlation not necessarily implies that the model is simulating well the concentration values. A figure showing chlorophyll vertical profiles should be included in the paper main body or the Supplement, ideally displaying data for each season.

4) The in-situ data used for model validation was mostly based on measurement collected in two cruises during May of 2017 and 2018, with all the cruises stations located

in the open GoM. These data do not allow evaluating whether the model is reproducing cross-shore patterns or seasonal variability. There is abundant data in the northern GoM that the authors could use to improve the model validation, like the Gulf of Mexico coastal ocean observing system (GCOOS).

5) The original Kishi et al model included a small, large and predatory zooplankton to represent ciliates, copepods (mesozooplankton), and euphausiids (macrozooplankton). Shropshire et al. redefined large and predatory zooplankton as two size-classes of mesozooplankton. This conceptual change is not indicated in Section 2.1.2. Do the zooplankton parameters in the model, like maximum growth rate and maximum grazing rate need to be revisited after this redefinition? I dislike that the model validation results in Section 3 can be dependent of the size-class arbitrary choose to define large and predatory zooplankton.

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