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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 #2

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GENERAL COMMENTS

In their manuscript, Shropshire et al develop an offline coupled physical-biogeochemical model to quantify zooplankton dynamics in the Gulf of Mexico (GoM). This study has 2 part: (1) the development, tuning and extensive validation of the biogeochemical model (adapted from NEMURO) and (2) the analysis of zooplankton dynamics, trophic pathways and their controlling factors on the shelf and in offshore waters. The manuscript is well written and the conclusions supported by the results. The authors carried out a significant validation effort, in particular on the zooplankton

side of the model, which is usually unconstrained. The validation also includes phytoplankton growth rates and primary production. In that respect, the study clearly stands out. The validated model is then used to provide insights on mesozooplankton biomass and diet on the shelf and in the open Gulf. Despite the large effort, the model validation has some limitations, detailed below, which should be better acknowledged. The model results on mesozooplankton diet, which represent the main scientific result of the study, is only a small of the manuscript and could be expanded, in particular with respect to their ecological meaning (e.g. trophic efficiency, fisheries), to get a better balance between model development and scientific findings in the manuscript.

SPECIFIC COMMENTS

The authors adapted NEMURO to the specificities of the GoM using an empirical method with parameter tuning and a parameter sensitivity analysis. The empirical tuning is carried out with a 1D (offshore) and the 3D version of the model. The procedure is detailed extensively in the supplement. The tuning is fairly qualitative and does not use statistics to minimize the model-data mismatch or at least this was not mentioned. For instance it is indicated that DCM magnitude is used as a benchmark during the tuning but observed DCM biomass was not available. This suggests that there was no systematics comparisons with observations during the process. Clarifications on the procedure could be added. Also, more comparison with the literature and from model/data comparison could be provided to support the choice of parameters. Although Text S3 is too long, some of the tuning explanations could be incorporated to the manuscript as it seems relevant to the reader. For instance a schematic of NEMURO including the default and modified pathways and parameters would be useful for the reader to get a general sense of the modifications. Other technical information could also be added to the manuscript. For instance, a brief summary (in a sentence or two) of the model forcing, initial, and open boundary conditions in the manuscript would be useful (L188-189). It would also be informative to have a table in the manuscript showing the parameters used in the sensitivity analysis and their range (L415-416).

Comment on the lack of useable chlorophyll concentration data in the SEAMAP cruises (L351-352, L463-472). Vertical profiles of chlorophyll concentration would have been very useful for the validation. Chlorophyll samples were not collected during the SEAMAP cruises? The authors could look into the World Ocean Database (WOD) and the World Ocean Atlas (WOA), the latter being used for the 1D model. A comparison of monthly averaged vertical profiles of chlorophyll in WOD and in the model would be informative since the magnitude of the DCM cannot be validated with the SEAMAP data. Alternatively an average profile in the offshore area could be compared with a spatially averaged profile from WOA. This would provide a better comparison of the vertical distribution of chlorophyll.

As mentioned above, the validation is thorough but there are some mismatches that are not properly acknowledged. For instance there is a large discrepancy between observed and simulated surface chlorophyll on the shelf (Fig. 2F) but it is referred to as a good agreement in the manuscript (L419, L755-756). The model clearly overestimates surface chlorophyll on the shelf, which may have implications for the secondary production. This should be discussed. Also, regarding the model-data mismatch in grazing rate (L546-549), the statement that it is due to the model functional types not reflecting the size classes would be true only if the total grazing was similar in the model and observations, which is not the case for mesozooplankton. A better explanation should be provided. Specific growth rates at the DCM are discussed during the validation (L556-557) but the data are not presented. A figure of the specific growth rate profiles could be added to the supporting material? Regarding the "sensitivity" analysis, model results are compared with observations (e.g. Figure 6). Some model-model comparisons could also be presented. For instance, mesozooplankton biomass cannot be evaluated because the observations are depth averaged (L595-597) but this information is available in the model and therefore the effect of parameter/functional changes can be evaluated with the model.

The benthic condition in the model is simple and more information supporting this

choice could be added (L167-170). For instance it is not clear what is the basis for the 10% conversion of particulate matter. The fact that this choice "had no significant impact on the model" should be explained. The authors also note that benthic processes such as denitrification are not included in their model (L763-765), which may contribute to model-data discrepancies on the shelf. Including denitrification would probably result in lower phytoplankton biomass, hence a better agreement with observations. It might also alter the trophic pathways or at least the magnitude of the fluxes. Therefore, I wonder why this important process for shelf regions was not included, as it is relatively easy to implement (e.g. Fennel et al, 2006). The potential impact on the model results should be discussed.

Model validation and results are discussed with respect to the open Gulf (>1000m) and the shelf (<1000m). This choice seems reasonable. There are clearly strong differences between these 2 regions, which are highlighted in the validation and analysis of the results. However these two regions are not uniform and therefore averaging may be misleading. The open Gulf region is relatively uniform, although as noted in the manuscript biological dynamics are different in/out of the Loop Current and eddies. The GoM shelf area includes the western Florida shelf, the northern GoM shelf, the western GoM shelf and the northern Yucatan shelf, which have different characteristics. These differences are relevant for the study. Presenting model validation for the different subregions would be very informative. For instance the surface chlorophyll mismatch may occur on the Yucatan shelf only or due to a poor representation of the northern Gulf. Such subdivision was used recently by Gomez et al (https://doi.org/10.5194/bg-2019-430). Discussing the results regionally would also provide more in-depth information on zooplankton dynamics in the Gulf and give more confidence to some of the results. For instance, even though the Yucatan shelf results are questionable, they would not influence the other shelf areas.

TECHNICAL CORRECTIONS

L30: does mt stand for metric tonne? if yes please use t instead or alternatively 10ˆ6 g throughout the manuscript

L187: "in Supplement S3"

L486-493: why switching to carbon units? mezozooplankton biomass is in mmol N m-3 in Figure 3.

L512: "sampled yearly": you mean a climatology? Could you rephrase?

L517: replace "community" by "biomass"

L558: replace "estimated" by "simulated"

L568-561: I don't understand these two statements. Why are you referring to all biogeochemical models?

L715: "NEMURO"

L711-713: PBMs are not used to fit an observational dataset and not just meant to explain variability. You may want to rephrase your statement.

L744-745: can you provide some support from the literature for this choice?

L785-794: Is this related to the CB mismatch? If not start a new paragraph

L890: "Despite their importance in the offshore region"

L891: Above you mention the "importance of protists in oligotrophic regions" but then you cite Fennel et al (2011), which is a northern GoM shelf model.

L892-893: Remove the double citation

Figure 2: Why having twice the same information? panels A and B should be removed, they could also be replaced by a 1:1 chl plot and a bias map

Figure 3: Same comment as for Figure 2. Figure 3a: A line plot would be better to compare the model with observations Figure3b-e: What is the depth range used for averaging? it is depth averaged right, not integrated?

Figure 6: It is difficult to assess the individual sensitivity experiments because they are all represented as black dots. Can you color-code them or plot experiment numbers rather than dots?

Figure 8: It is a bit difficult to get around the color scale. The information is also is also somewhat redundant with Figure 7.

Supplement:

- for model parameters/processes you could use NO3 and NH4 instead of NO2 and NH2

- the KSI line in Table S2 indicates Ammonium half saturation constant (mmol N m-3)

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