

Dear Referee,

Thank you for taking the time to assess our revised manuscript. You raised several specific comments that we are addressing here after. We incorporated some changes to the manuscript to reflect your suggestions.

**Reviewer :** *I would suggest the authors to also include the open boundary conditions of biological component.*

**Authors:** We included the open boundary conditions used for the biogeochemical tracers in the section II.1: “The open-boundary conditions of biogeochemical tracers are prescribed from the World Ocean Atlas observation database (Garcia et al., 2010) for  $\text{NO}_3$ ,  $\text{O}_2$ , Si, and  $\text{PO}_4$ , and from the global configuration ORCA2 (Aumont & Bopp, 2006) for DIC, DOC, Alkalinity, and Fe. The other state variables are forced arbitrary very small constant values.” (P6 Line 107-110 of the revision-tracked manuscript).

**Reviewer :** *P9 Line158 of the revision-tracked manuscript: The stratification would result in the shallow mixed layer.*

**Authors:** We preferred avoiding any preliminary interpretation at this stage of the manuscript. We simply rephrased as follow : “The stratification of the water column is evaluated by the square of the buoyancy frequency” (P9 Line 157-158 of the revision-tracked manuscript).

**Reviewer :** *P9 Line172 of the revision-tracked manuscript: Please give the definition of the euphotic layer: the depth where the light intensity is 1% of the surface?*

**Authors:** We gave the metric used to define the euphotic layer : “The euphotic depth corresponds to 1% of the incoming photosynthetic active radiation at surface” (P9 Line 172-173 of the revision-tracked manuscript).

**Reviewer :** *P16 Line276-277 of the revision-tracked manuscript: This sentence is confusing. I would suggest to change the ‘associated with’ into ‘accompanied by’.*

**Authors:** We thank the reviewer to point that the sentence was confusing and followed its suggestion (P16 Line 275 of the revision-tracked manuscript)

**Reviewer :** *P17 Line293-294 of the revision-tracked manuscript: The contrasted seasonal response to the LCEs cannot be recognized by the surface chlorophyll. This is one of the main conclusions of this manuscript.*

**Authors:** We agree that this can be interpreted as a contradiction with the main conclusion of the manuscript. To avoid any misunderstanding, this was deleted of the revised version (P17 Line 292-293 of the revision-tracked manuscript).

**Reviewer :** *P17 Line300-304 of the revision-tracked manuscript: As in my first general comment, I am concerned about the biological model performance in the subsurface and the potential influence of this model weakness on the main conclusion: the increased winter chltot within the core of the LCEs. As the authors mentioned in their response, the vertical profiles of chlorophyll vary a lot in the winter: some individual profiles are well mixed without the DCM while others are ‘stratified’ with the distinct DCM. Based on Damien et al 2018 to which the authors referred their model validation, the averaged winter profile shows a distinct DCM at about 60m with the  $[\text{Chl}]$  DCM ~50% higher than the  $[\text{Chl}]$  surf. All of these clues suggests the significance of ‘stratified’ chlorophyll profiles in the winter of Gulf of Mexico. The failure of the biological model to reproduce these important ‘stratified’ profiles may have large influence on the results. At least, the results of this paper do not apply to these ‘stratified’ winter*

*profiles. I am not asking the authors to re-run the model or to accept my opinion, but I hope that the authors can fully discuss it in their manuscript and be cautious about their conclusions.*

**Authors:** We understand the concern of the reviewer and we tried in our first response to demonstrate the robustness of our modeling results at a basin and mesoscale scales although acknowledging that modeled vertical profiles of chlorophyll can individually present some biases. In a revised version, we discuss more into details these biases and how they can affect the main conclusions of the study (P32 lines 546-553 of the revision-tracked manuscript).

**Reviewer :** P24 Line 405-407 of the revision-tracked manuscript: *Where is the euphotic layer? Could the authors plot it along with the nitracline? The authors mentioned it earlier in their revised manuscript that the euphotic layer can reach between 120 and 150 meters in the Gulf of Mexico. If it applies here as well, the nitracline is still within the euphotic layer.*

**Authors:** The base of the euphotic layer and the nitracline were added to the figure 9. The nitracline, defined as a nitrate concentration threshold in this study, is used as a practical metric of which relative position with the mixed layer depth controls the amount of nutrient injected into the surface layer in winter. The surface nitrate concentrations confirm that the relative position of the MLD and the nitracline controls the amount of nutrient supply to the surface in winter and consequently the  $[CHL]_{tot}$  increase. As defined here, the nitracline is indeed still within the euphotic layer. In case we would want to investigate the nutrient flux through the base of the euphotic layer, a more restrictive threshold would be preferable to define the nitracline.

**Reviewer :** P26 Line 448-452 of the revision-tracked manuscript: *Please refer to Figure 9*

**Authors:** Done (P26 Line 448 of the revision-tracked manuscript)

**Reviewer :** P27 Line 466-467 of the revision-tracked manuscript: *It is hard to see the differences in the decoupling of production and grazing between the eddy core and background GOM (e.g.  $GRZ_{tot}/PP_{tot}$  both about 0.95 on February) from the figure. I would suggest the authors to provide the mean values and the standard deviation, and to reduce the y-axis scales of the figure.*

**Authors:** The mean values and standard deviation have been added to the text (P27 Line 363-364 of the revision-tracked manuscript). The y-axis of the figure can hardly be reduced if we do not want to cut off the variability envelope.

**Reviewer :** P27 Line 472 of the revision-tracked manuscript: *The major source of recycled nutrients should be the remineralization.*

**Authors:** Right. Remineralization is the major source of recycled nutrient and grazing feed remineralization by the production of dissolved and particulate organic matter. To avoid this ambiguity without going too much into details, we rephrased as followed : “Since grazing is known to be a major contributor of the recycling loop in the euphotic zone, ...” (P27 Line 469-470 of the revision-tracked manuscript).

**Reviewer :** P28 Line 474-477 of the revision-tracked manuscript: *How did the authors draw this conclusion from Figure b1 which show the biological source and sinks of  $NO_3$ ? Could the authors explain it clearer.*

**Authors:** Regarding inorganic nutrient such as nitrate ( $NO_3$ ), the production of organic matter (or primary production) expresses as a biogeochemical sink. In figure B1, we can see that the intense primary production rates in winter is associated with a strong consumption of nitrate. This  $NO_3$  biogeochemical sink is larger and occurs on a thicker surface layer within the LCE core. In the revised manuscript, we explained it clearer: “In addition, the biogeochemical consumption of nitrate that foster

the production of organic matter occurs in a deeper layer within the LCEs core compared to the background GoM (Fig. B1. e. f.)." (P28 Line 472-473 of the revision-tracked manuscript)

**Reviewer :** *P30 Line 511-524 of the revision-tracked manuscript: This paragraph discussed the relative importance of two mechanisms (eddy pumping and eddy-wind interaction) in the winter and should not be in this subsection (Section IV. 4 How to explain summer productivity). I can understand that it is an extension of the discussion on two mechanisms, but I hope that the authors can re-organize it better. Maybe because I am not a physical oceanographer, what is the definition of eddy-Ekman pumping? Is it Eddy-wind interactions? Please make it clearer.*

**Authors:** We understand that the title of the section is the main reason of the reviewer confusion while reading this last paragraph. He is right since it explicitly mentions “summer” while this last paragraph focus on the relative importance of two mechanisms in winter. In the revised version, we changed this section name to “Eddy-wind interactions” to keep it coherent with the content of the section. Eddy-Ekman pumping is eddy-wind interactions. We make it clearer in the text (P29 Line 491-492 of the revision-tracked manuscript)

**Reviewer :** *P30 Line 519 of the revision-tracked manuscript: the word ‘seasonal’ is duplicated. Please remove one.*

**Authors:** Done. We thanks the reviewer for reporting this typo.

**Reviewer :** *P34 Line 610 of the revision-tracked manuscript: I guess this should be 80%:20%, right? Caption of Figure B1: ‘minus’, not ‘menu’*

**Authors:** It is 80%:20%. The text has been updated according to this notation. The error in the caption was corrected.