

Dear Dr. Zuo Xue,

Thank you for giving us the opportunity to submit a revised draft of our manuscript. We appreciate the effort that you have dedicated to providing this valuable feedback.

**Reviewer :** *Overall, I only have one suggestion-since the authors argue that the winter total PP is correlated not only eddy activity but also vertical mixing and the interaction between wind and the LCEs, why not carry out some sensitivity tests by manipulating the strength of wind-induced mixing in winter to support such hypothesis?*

**Authors :** We understand that your main concern regards the sensitivity of the primary production response to mixing within the LCE core. In a sense, this is what we did in the manuscript at the seasonal scale showing that the increase mixing in winter is associated with the increase in primary production. This is coherent with the seasonal and mesoscale focus of the study. For finer details, we could, following your suggestion, carry out a set of numerical runs with varying atmospheric forcing and/or mixing parametrization and look at the primary production response. However, this approach would be pretty heavy to implement since it would require several additional simulations over periods of time long enough to build robust LCE composites. It would also take out this study from its realistic context.

To derive this suggestion using the simulation described in the manuscript, we can argue that mixing presents significant variations on time scales ranging from one day to one week. Thus we can look at the [CHL] response to these variations within LCEs. This requires abandoning the methodology based on the seasonal composite analysis followed in the manuscript to adopt the Lagrangian point of view of each individual LCE. Figure R1 shows the time-series of surface density and chlorophyll concentration in the center of one LCE. A surface density increase can be interpreted as a proxy for mixing. It is clear that [CHL] shows variations at high frequency and there is indeed a correlation between mixing and a chlorophyll increase at high frequency in winter (yellow band). It is however not as robust as at seasonal scale since other drivers might become important at higher frequencies. Moreover, 5-day average outputs constitute probably an important limitation to study high-frequency processes.

Given these limitations, we agree with the reviewer that proper sensitivity test involving a set of new simulations would be helpful. However, we believe that expanding our dataset is hardly feasible, given the costs involved and would not significantly support our argument given the seasonal focus of the study.

Following is a point-by-point response to the reviewer' minor suggestions. The spelling and grammatical suggestions provided were incorporated. We have highlighted the changes within the manuscript.

**Reviewer :** *Line 105, details about the boundary and atmospheric forcing is needed, as well as rivers, although similar to Damien et al. 2018*

**Authors :** More details and references were added (lines 108-110).

**R:** *Line 205, what does “zonal” mean here?*

**A :** Zonal here means “along latitudes”. We rephrased using “westward” to avoid confusion.

**R:** *208-212, here you mean the model results, so try not to use “images”*

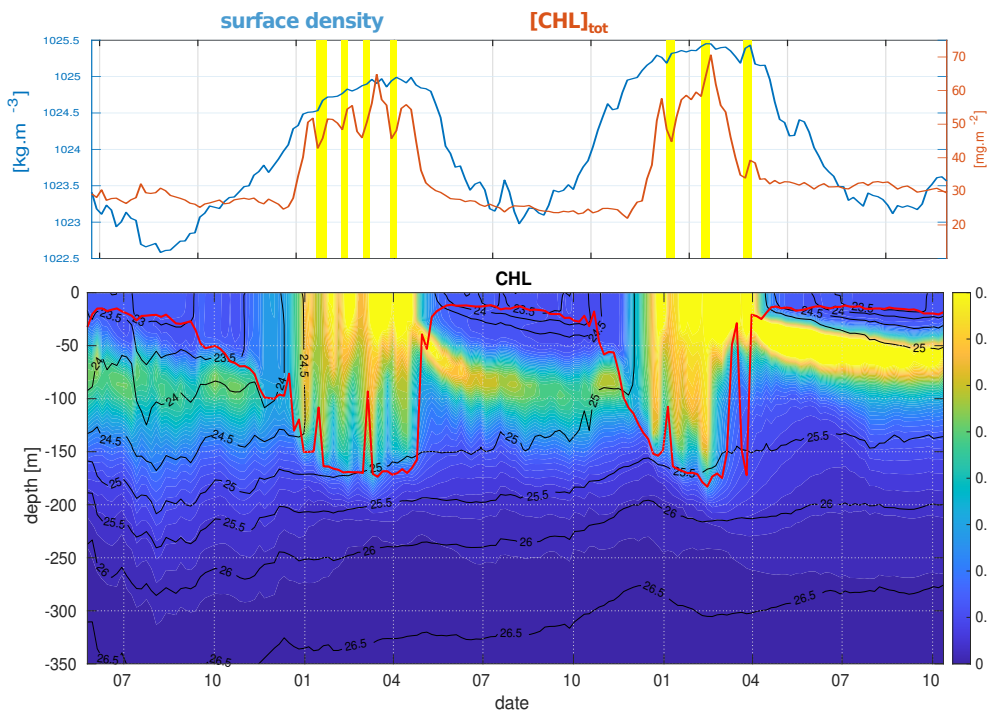
**A :** We used “model outputs” instead (lines 215-222).

**R:** Line 279-287 & Figure 6. Winter should be DJF and summer should be JJA, right?

**A:** We choose to define the composite averaged over the months of January and February as “Winter” and the one averaged over “July and August” as “Summer” (c.f. lines 144-146).

**R:** Line 323-328, very good figure 8. needs more words for the salinity as well

**A:** Salinity is discussed in the discussion section IV.1 dealing with eddy-trapping mechanism



**Figure R1 :** time-series of (upper panel) surface density and vertically integrated chlorophyll, and (lower panel) chlorophyll profile (in  $\text{mgCHL.m}^{-3}$ ) at the center of an individual LCE. Black contours refer to density anomaly and the x-axis is labeled in months.

**R:** Line 363, what is an eroded salinity maximum?

**A:** “Eroded” was used here as a metaphoric synonym for “diffused”. We rephrased it as “lower salinity maximum” (line 373)

We would like to thank the referee again for taking the time to review our manuscript.

## References :

Damien, P., Pasqueron de Fommervault, O., Sheinbaum, J., Jouanno, J., Camacho-Ibar, V. F., & Duteil, O. (2018). Partitioning of the open waters of the Gulf of Mexico based on the seasonal and interannual variability of chlorophyll concentration. *Journal of Geophysical Research: Oceans*, 123(4), 2592-2614.