General comments

This study is to investigate the biological response within the Loop Current Eddies (LCEs), which are anti-cyclonic eddies in the Gulf of Mexico (GOM). Based on the satellite data in combined with a 3D coupled physical-biological model, the authors identify the positive anomalies of vertically integrated chlorophyll (chltot) within the core of LCEs in relative to the background GOM waters in the winter, which is, however, not reflected by the surface chlorophyll. In addition, the authors attribute this positive anomaly to the winter mixing within the core of LCEs.

In general, this study is very interesting, and its results can have important implications for the global carbon cycle. However, there remain some issues that by being addressed, the manuscript can be improved.

First, given that the winter increase of chlorophyll within the core of LCEs cannot be detected from the surface, I am concerned with the model performance of biological properties below the surface. The authors mention in the introduction that they will pay particular attention to the validation of eddy structures and surface chlorophyll. However, I would suggest more validation of subsurface biological properties inside and/or outside the LCEs. As I know, there are six autonomous floats which were deployed from 2010 to 2015 in the Gulf of Mexico. The authors can use these float measurements to do some model-data comparisons as they did in their previous paper (Damien et al., 2018) and to support their main conclusion, i.e. the positive anomaly of winter chltot within the core of LCEs. From my standpoint, this is necessary. For instance, model results show the nearly monotonically decreasing patterns of chlorophyll along the vertical direction in winter (Figure 6), which contrasts with the summer patterns with a distinct deep chlorophyll maximum (DCM). The winter chltot is higher because of the deeper inflection point and homogenized layer within the core of LCEs. However, based on the in-situ observations collected from autonomous floats in the Gulf of Mexico, the DCM is distinct throughout the whole year with the depth around 70-100m (Fommervault et al., 2017; Green et al., 2014). Model validation results (Figure 3 in Damien et al. (2018)) also show that this coupled model fails to reproduce the observed DCM in the winter. This could be a result of using suboptimal values of key biological parameters. Due to this model's weakness, the authors should be more careful about their results. Is it possible that the vertical profiles of chlorophyll respond to the LCEs in a similar way as they do in the summer, e.g., the deeper DCM and lower chltot? The authors should justify whether this model deficiency will change their main conclusions.

Second, some topics are not discussed comprehensively, making it look like a half-cooked product. For instance, the authors use salinity as a tracer to explain the eddy trapping mechanism. I really like it. However, there is no further discussion on its roles in the biological properties. Is the eddy trapping mechanism important for the positive anomalies in the core of winter LCEs? Is the positive anomaly produced locally within the LCEs or trapped from their original places during the eddies' formation? Based on the model results (Figure 8b, also 19 Line 323-328), the preferential increase of chltot within the winter LCEs is not observed before shedding and little differencse in chltot exist between the eddy center and background waters, which seems to support that the positive anomaly is produced locally. However, this behavior is largely determined by the poorly constrained open boundary conditions. Therefore, I would suggest the authors to complete this discussion based on their model results and float profiling observations.

Another example is in Section IV.4. The authors suggest that in the summer, the Ekman pumping within the LCEs can provide additional NO3 to sustain a comparable level of new primary productivity with the background waters. However, they don't explain the lower values of regenerated primary productivity, which determines the negative anomalies of chltot within the eddy. Which mesoscale mechanism is responsible? Why the new and regenerated primary productivity respond to the LCEs differently?

Specific comments

P7 Line 127-128: Could the authors explain more explicitly why a shallow detection depth can maximize the accuracy?

P9 Line 162: The authors seem to mix up the chlorophyll anomaly (in unit of mg m-3) and its normalized one (unitless). Based on their definition of normalized chlorophyll anomaly [CHL]', it should be unitless. However, they use chlorophyll anomaly almost throughout the whole manuscript without any definition (e.g. P11-12 Line195-200, Figure 7a, b). Based on the unit, I guess it might be calculated as $[CHL] - \overline{[CHL]}$. The authors should be clearer about it.

P12 Line 213-215: I can't see this paragraph because it is covered by the Figure 4

P23 Line 386-387: What's the definition of euphotic zone in this study. No figures show where the euphotic zone is.

P24 Figure 10. This figure is used to illustrate that in the winter of LCEs, the mixed layer is closer to the nitracline. However, it shows the results in summer (please see the figure caption).

Section IV.3: This subsection is not discussion. It should be in Results section.
Section IV.3: The grazing rate looks very important. What is the role of grazing rate in the positive anomalies of chltot within the core of winter LCEs? This top-down perspective will be interesting.

Section IV.4: It is unfair to compare the amplitude of annual averaged Ekman pumping with the deepening rate of mixed layer in the winter. What's the seasonal variability of the Ekman pumping?

P26 Line 449: Does this sentence mean that the vertical transport is a net effect of eddy pumping (downwelling in the LCEs) and eddy-wind interaction (upwelling in the LCEs)?

P28 Line 470: Does it means 0.06±0.13 m/day, or from +0.06 to -0.13 mg/day?

P28 Line 471-472: Can the authors refer to a figure which shows upwelling of isopycnals within the LCEs

P29 Line 97: As one of main conclusions, the authors never show anything about phytoplankton. As they mentioned before, the changes of chlorophyll can be a result of either the real change of phytoplankton or the photoacclimation. The authors should provide some results about the phytoplankton.

Reference:

Fommervault, O. P. De, Perez-brunius, P., Damien, P., Camacho-ibar, V. F. and Sheinbaum, J.: Temporal variability of chlorophyll distribution in the Gulf of Mexico: bio-optical data from profiling floats, Biogeosciences, 14, 5647–5662, doi:10.5194/bg-14-5647-2017, 2017.

Green, R. E., Bower, A. S. and Lugo-Fernandez, A.: First Autonomous Bio-Optical Profiling Float in the Gulf of Mexico Reveals Dynamic Biogeochemistry in Deep Waters, PLoS ONE, 9(7), 1–9, doi:10.1371/journal.pone.0101658, 2014.