RESPONSES TO REVIEWER 2

We would like to thank the reviewer for the constructive comments and suggestions and respond to them below in blue type.

The study was carried out via comprehensive methods, including numerical models, remote sensing observations, and field observations. It made important contributions to the studies on (sub)mesoscale hydrodynamic motions and bio-geo-chemical process, and it also provided a successful example for how numerical model can be used to instruct the design of field campaigns.

Except for some minor issues in the text and figure descriptions, more detailed descriptions about the decomposition of OW parameter should be provided in the method and supplied in the results. Also, the effect from Coriolis force and thermal stratification should be further discussed if that is still within the scope of the study.

Please see responses below.

I recommend the manuscript should be accepted subject to minor revisions. Please find my specific comments below:

Abstract: the abstract contains too much introduction. More focus should be put on the major results you found via applying the numerical model and in field observations.

The Abstract was modified.

Line 88-89: this sentence is not necessary.

Authors believe that this sentence is necessary there to alert reader that there is an SI.

More discussion about the effect from Coriolis force and seasonal stratification on the gyres' size, lifetime, and boundary are required.

Two new Sections 5.4 and 5.5 and Figures 13 and 14 were added to the revised manuscript. Please note, however, that the focus of this paper is on the procedure to detect gyre patterns, not on the physics of gyre formation.

The meteorological data used to drive the numerical model were from the atmospheric model, but the wind information showed in Fig. 5 and used to identify the event was from Buchillon field station. Have you compared the model input with the realistic wind data? How is the spatial variation in the wind field? The statement or comparison figure are required to clarify that.

The data associated with the Buchillon field station were only used to demonstrate that the COSMO data are realistic and can be relied upon for forecasting purposes and data analysis. As shown in Figures 5a-d and as discussed in Section 4.1.3, the temporal and spatial variations of both wind stress and heat flux extracted from the COSMO data were used in the EOF analysis, not data from the Buchillon field station.

Modifications were made (L291-294). Furthermore, the averaged wind speed and direction during the three campaign periods based on the COSMO data are presented in new Figure 7 in order to show the spatial variation of the wind field.

Fig. 2: Describe the sources of inset images in the figure.

This information was added to the figure caption.

Line 230: Is there any specific reason for choosing September? Due to the availability of Satellite data? Or thermal stratification is vanishing in this month?

The studied months represent two different stratification conditions: strongly stratified (September and October) and weakly stratified (November and December) conditions. The thermocline layer is relatively strong in September, at a depth of approximately 10-15 m, and the epilimnion consists of a shallow mixed layer. These conditions are ideal for the formation of cyclonic gyres in lakes.

More details about the effect of thermal stratification were added to new Section 5.4.

Line 248-249: Why? The combine effect of Coriolis force?

The Coriolis effect results in flow divergence and as a result, upwelling will occur at the center of the cyclonic gyre.

A detailed discussion is given in new Section 5.5. We also added temperature profiles to new Figure 13 in order to provide evidence for pelagic upwelling.

Fig. 3: What do the percentages in the figure represent?

The values given in Figure 3 indicate the percentage of the observed spatiotemporal changes to the monthly OW_N values (here September). For example, the first spatial mode in the near-surface layers accounted for approximately 56% of the total observed spatiotemporal changes in the monthly changes in OW_N values, whereas the second mode only contributed 23%.

Modifications were made at L245-248.

Fig. 5: Are the values in (a) and (c) integrated over a specific time range? Can you give more explanation about how you decomposed OW parameter? In (e) and (f), the information is blur here. Why OW parameter is negative when Pown is positive? Does that mean Eown and Pown always have opposite signs?

The values in (a) and (c) represent the first mode of EOF results corresponding to the wind stress and the net heat flux for September 2018 obtained from COSMO data.

According to the Eq. (5), the OW values were decomposed into the basis function (E_{OW}^{k}) and the principal component time series as:

$$\mathrm{OW}(X,t) = \sum_{k=1}^{N} E_{\mathrm{OW}}^{k}(X) P_{\mathrm{OW}}^{k}(t) = E_{\mathrm{OW}}^{(1)}(X) P_{\mathrm{OW}}^{(1)}(t) + E_{\mathrm{OW}}^{(2)}(X) P_{\mathrm{OW}}^{(2)}(t) + \cdots$$

We only kept the negative values of OW because, in the presence of gyres, the terms on the right side of Eq (5) have to be negative in order to represent elliptic regions. For better visualization, we presented only the negative values associated with $E_{\mathrm{OW}}^{(1)}(X)$ and $E_{\mathrm{OW}}^{(2)}(X)$, which indicated a clear gyral pattern, as defined in Section 3.1. The positive values of $E_{\mathrm{OW}}^{(1)}(X)$ and $E_{\mathrm{OW}}^{(2)}(X)$ are given in new Figure S2, where it can be seen that the gyral patterns are insignificant compared to the negative values. Therefore, the reviewer's interpretation is correct, i.e., E_{own} and P_{own} always have opposite signs.

Figure S2 and L249-253 have been added and Figure 5 was modified.

Line 404: Is that because the spatial resolution of the field measurement is not fine enough?

Since SE1 and SE2 are located in the nearshore regions, boundary currents may be mistaken for eddies. We increased the field resolution to ensure that the measured velocity field represents a cyclonic circulation in order to avoid such misinterpretations.

Line 423: Did you record the lifetime of them?

We were not able to record the lifetime based on field measurements due to frequent short (few hours) wind events, which prevented us from carrying out field measurements (we can only measure reliably and safely under calm conditions).

Fig. 9: description of panel (h)?

Added.

Line 497-499: You have said that in the result part.

These lines were removed from the result section.