Response to Referee #2

This study used a combination of satellite-derived ocean color data and in-situ measurements of chl-a and nutrient to examine the spatial and temporal patterns of phytoplankton blooms in the eastern China seas. I can see a large amount of work went into the manuscript. I think this study can potentially have an important contribution to the understanding of bloom variability and its drivers (e.g. light availability, surface mixing, river inflow, and nutrient supply) in the region. However, I found this manuscript, as presented in the current version, is not able to demonstrate that the analyses are solid and the conclusions are sound enough. There are a number of issues in the manuscript that affect the quality of the paper. I agree with the Anonymous Referee #1 on most of the concerns he/she raised (which I am not going to repeat here), and I have additional concerns and suggestions (see below). Therefore, I would suggest a major revision before the ms can be considered for publication.

1. The authors calibrated the SeaWiFS and Aqua/MODIS data using in situ observation for relatively low turbidity areas with Rrs555<0.005 (based on Fig 3e,f). Can the similar calibration be done for areas with Rrs555>0.005 based on Fig 3c,d? If so, how will the results be affected?

Response: Thanks for this suggestion. In the revised manuscript, we have presented the linear regressions for the whole matched-up dataset, and use a uniform linear regression from the whole matched-up dataset to correct the satellite Chla data, since our study areas cover the clear outer shelf water, moderate turbid middle shelf water and high turbid coastal water.

2. The authors used 10 µg /l as a threshold to define the phytoplankton bloom (The Referee #1 questioned about it). I understand (from P121, line 10-20) the authors' argument that even in ". . .waters are extremely turbid, the maximum chla is generally less than 10 µg/l". Therefore, by having 10 µg/l as the threshold, they can effectively eliminate the 'false blooms' caused by the satellite overestimation. This argument seems reasonable to me, but the authors need to clearly indicate it when they define the bloom frequency and bloom intensity. For example, when they mention the bloom intensity, they might specifically say something like "bloom intensity for chl-a concentration over 10 µg/l", or define it right from the beginning (e.g. in Eq. 4). Considering the comments from Referee # 1, it would be helpful to conduct a sensitivity analysis by adjusting the threshold slightly lower and higher than 10ug/l to see if it affects the results and conclusions. Please also consider my first comment about the calibration in high turbidity areas because the threshold value could be smaller once the satellite values are calibrated in those areas.

Response: We appreciate the reviewer for understanding our expressions. We agree with the reviewer's suggestion, and we have added the explanation that our bloom frequency and intensity was for Chla over 10μg/l in the section 3.4 in the revised manuscript.

We thank the reviewer for good suggestion to conduct a sensitivity analysis. In the revised manuscript, we have carried out a sensitivity analysis to check the effect of threshold values variation as 8μg/l, 10μg/l, 12μg/l on the bloom results, based on the new correction results. Comparing with the bloom intensity index (BI) derived by 10μg/L, the mean differences of the BI derived by 8μg/L and 12μg/L are 9.8% and 6.4% for the region A in the Changjiang Estuary, and 22.7% and 14.7% for the region B in the Yellow Sea and Bohai Sea. Although the BI slightly varies with the threshold values from 8 to 12μg/L, the inter-annual variation and long-term trends are consistent well. The results prove that the influence of small variation of our chosen 10μg/L as threshold for large boom identification is quite less.

3. I have concerns when the authors divide the whole domain into region A and B, and then try to relate it with forcing. Especially for region A, it covers a large area with spatially heterogeneous bloom frequency and intensity (e.g. large difference between nearshore and offshore regions as shown in Fig 10). Trying to link regional averaged bloom intensity to climate index or nutrient loading could be misleading. On the other hand, the spatial variability is probably an interesting feature worth exploring in this study. One idea is to conduct an EOF analysis to see both spatial and interannual patterns. This may help the authors to link the spatially-explicit bloom dynamics with different forcing.

Response: Thanks for these comment and suggestion. We agree with the reviewer's comment that linking the regional averaged bloom intensity in region A to climate index or nutrient loading may not suitable due to large spatial heterogeneity. To avoid the misleading, we have selected a smaller region with less spatial heterogeneity in the Changjiang River mouth, then link the bloom intensity index to river discharge in the revised manuscript.
Reviewer suggested a good idea to carry out EOF analysis to see the spatial and temporal variability. However, since the bloom intensity is defined as the regional integral, EOF can not derive the spatial variability. Even though we can take EOF analysis for the bloom frequency, the short time series (10 maps for SeaWiFS, 9 maps for Aqua/MODIS) may limit the effect of EOF results. In addition, information of the spatial and seasonal variability can be derived from the climatology seasonal distributions in the manuscript. We have added the spatial variability in the results section in the revised manuscript.

4. Pg 125, line 27. The argument that "Light is not a limiting factor because it is

located in the mid latitudes" is not justifiable. Vertical mixing is another factor related to light limitation (think about classic Sverdrup's critical depth model).

Response: Thanks for this suggestion. From Fig.7, we know that phytoplankton blooms mainly occur in spring and summer in the eastern China seas. The upper mixed layer depths in the eastern China seas are usually less than 20m in Spring and less than 15m in Summer (unpublished data from Zhiliang Liu as the cruises from CHOICE-973 project, 6-31 August 2009 for summer cruise and 31 May to 8 June 2011 for spring cruise)), while the euphotic layer depth are generally larger than 30m in the middle and outer shelves in the Spring and Summer (Figs.S1), and large than the upper mixed layer depths. In addition, the classic Sverdrup's critical depth is generally larger than the euphotic layer depth (corresponding to the compensation depth in the Sverdrup's critical depth model). Thus, light may not be a limiting factor for the phytoplankton blooms in the eastern China seas. We have added this discussion in the revised manuscript.

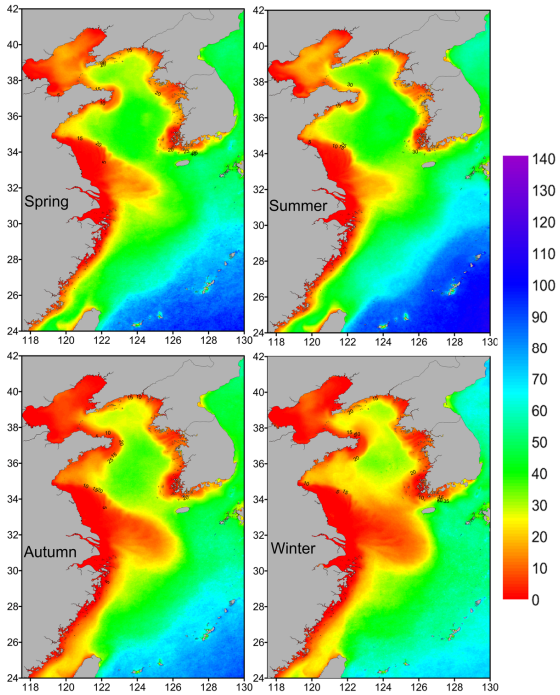

Fig. S1. The climatology seasonal euphotic layer depth in the East China Sea (ECS). The euphotic layer depth was retrieved by the satellite-derived water transparency from SeaWiFS (1998-2007). (For the details of the algorithm, please see the Jiao et al., 2005).

5. The link between blooms and ENSO needs to be revisited. Some statistic analyses are indeed needed to establish the link. If the authors think it is pre-mature to discuss it, I would suggest delete the discussion about this link from the text.

Response: Thanks for this suggestion. It is difficult to assess the impacts of ENSO and PDO on the blooms using only 14 years dataset. Yet, we think it is still possible to analyze the relationship between ENSO, PDO and bloom intensity index. In the revised manuscript, we have added more statistical results

and focused on the relationship analysis instead of the impacts.

6. As listed by the Referee #1, the authors could use some proof-reading before submitting the ms. For instance, Fig 7, upper panel should be for PE and bottom panel for Fbloom.

Response: Thanks for this suggestion. We have carried out a careful proof-reading for the revised manuscript, and corrected the error in Fig.7. Also, we have asked a native English speaker to make the proof-reading.