Biogeosciences Discuss., 10, C118–C120, 2013
www.biogeosciences-discuss.net/10/C118/2013/

Interactive comment on "Satellite views of seasonal and inter-annual variability of phytoplankton blooms in the eastern China seas over the past 14 yr (1998–2011)" *by* X. Q. He et al.

Anonymous Referee #2

Received and published: 21 February 2013

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?

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 chl a 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.

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.

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).

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.

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.

Interactive comment on Biogeosciences Discuss., 10, 111, 2013.