

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 #1

Received and published: 7 February 2013

He et al use MODIS and SeaWiFS satellites to derive chlorophyll-a concentration within the eastern China seas to identify seasonal and inter-annual variability. A tremendous amount of effort seems to have gone into this manuscript and the authors are off to a reasonably good start. However, I have many concerns regarding the presentation of the data and manuscript. This manuscript requires major revision before it can be considered for acceptance. First the manuscript presents little if any quantitative information or statistical results. The data is presented and described in a qualitative fashion, which is inappropriate for a scientific article. Second, I find the discussion and conclusions to be relatively boring and, as presented, provide little new information. Finally, would benefit from minor overall editing to tighten up some sentences.

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Abstract: The abstract provides no quantitative information. The authors state they validate and calibrate chlorophyll algorithms, it doesn't seem like they successfully did any type of rigorous calibration effort. Instead they used a previously calibrated algorithm and performed a validation using in-situ data. The results of the validation are never discussed, other than the algorithms overestimate in highly turbid waters. This has repeatedly been the case with MODIS and SeaWiFS OC3 and OC4 algorithms. It isn't clear what the authors mean by the concentration caused by high water turbidity was less than 10 ug/L. How is this justified in being a threshold for bloom identification? What are the statistical results from the ENSO and PDO cycles? Is it possible to really assess the ENSO and PDO decadal cycles if the authors have just over 1 decade of data? Seems like a stretch to me.

Introduction: The authors mention previous work that used CZCS and OCTS but state the algorithms over estimated chlorophyll, how is that any different than the MODIS and SeaWiFS performance? The authors also mention Yamaguchi and Shi and Wang looked at seasonal climatology and interannual variations of chlorophyll. Then they state their work is different because seasonal and interannual variations have never been investigated. This is contradictory. **Data:** What do the authors mean “preserve statistical rigor”? It doesn't seem like the authors undertook any calibration effort, so this should be removed from the text. What method was used to match in-situ data with satellite data? Bailey and Werdell provide a standard method which is the accepted approach. They recommend using only L2 data at the native resolution of the sensor. Instead the authors used daily binned data, which is problematic because multiple pixels may have been spatially composited into a daily data bin. The authors never discuss the box extraction, do they use 1x1, 3x3, or greater? Do the authors filter for viewing angle at all? They should follow the method of Bailey and Werdell. What is meant by “heavy cloud masking”? Do the authors know all of the masking was due to clouds using the L2 flag array? Delete the words “powerfully organized”. What is meant by having “almost the same sampling stations?” Where the in-situ chlorophyll samples corrected for phaeopigments, what method was used, please provide a

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reference. Remove the sentence about “a valuable and treasured dataset.” How do the authors handle multiple samples with depths less than 5m? Are they averaged? Weighted?

2.2.3: How is the “surface layer” defined? How are “serial stations” defined? How is a “serial oceanographic survey” defined? 3: When the annual means were calculated, how was the spatial extent addressed? Was this the annual mean for the entire eastern China sea? It seems odd the authors discuss figure 2 then jump back to figure 1. Figures should be discussed sequentially. What is meant by SeaWiFS overestimates more? 3.2: What is meant by “same daily time window”? What hours were used, typically +/- 3hrs of satellite overpass are retained for match-ups. What solz and senz angles were filtered for each satellite? What variance was used to excluded satellite extracted points during the match-up process? How is “heavy cloud cover defined”? How do the authors justify “good performance”? Instead of qualitatively stating this, it should quantitatively be presented. It isn't clearly presented how the authors choose $Rrs_{555} > 0.005$ as an indicator of high turbidity and where the overestimation of chlorophyll begins. Even when chlorophyll associated with $Rrs_{555} < 0.005$ are considered the linear regression follows the same trend of “overestimation.” So this doesn't seem like a justified approach. Again, the authors don't clearly justify their selection of 10ug/L. A selection of 17.9 or 5.7 could have also been selected (authors discuss these numbers in 5.4) and would have similar results, so why 10? What do the authors mean, “Thus, the statistics of blooms are expected to be reasonable and meaningful”? This is an ambiguous statement and no statistics are presented, so it is impossible to tell what is being presented.

5 Discussion: Overall, I think the discussion section tends to ramble on, and in many parts reads more like a results section. The discussion seems to lack the expected comparison of other systems. How does this compare to other areas around the world, such as the Gulf of Mexico. Pg 127, section 25 is awkward and unclear. What are the authors talking about when they say the SeaWiFS plumes were inverted for CDOM?

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The section on nutrients reads much like a result section and only provides qualitative discussion. Some statistical analysis should be conducted, perhaps a multiple linear regression to prove there is a relationship instead of just guessing. The connection to ENSO and PDO seems very weak, and requires quantitative analysis and statistics. 6. Conclusion: The authors state, “despite significant inter-annual variation” but the significance is never presented. In the previous sections the author highlight the overestimation of 10 ug/L now they discuss an overestimation for chl a less than 3 ug/L. Back to my original point, using $Rrs > 0.005$ seems arbitrary and the overestimation is the same if all data is included, only data above $Rrs > 0.005$, or $Rrs < 0.005$. Figures 2 and 3 present no statistics, what is the regression equation, R^2 , RMSE, and significance? Figure 5: This figure isn't clearly explained in my opinion, I'm not sure what computation was undertaken. Figure 9 and 13: What is the slope? Is it significantly different from zero?

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

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