Biogeosciences Discuss., 11, C3292–C3298, 2014
www.biogeosciences-discuss.net/11/C3292/2014/

Biogeosciences Discussions
Open Access

Interactive comment on "Progressive eutrophication behind the world-largest super floating macroalgal blooms in the Yellow Sea" *by* Q. Xing

Q. Xing

qgxing@yic.ac.cn

Received and published: 5 July 2014

In this paper, the authors attempt to show that the increase in the eutrophication of the Yellow Sea is the lead cause for the resurgence of the floating macroalgae blooms over recent years. The authors attempt to show this by using a nutrient pollution index (AWCPI-NP) and satellite-derived chl-a concentration as proxies for levels of eutrophication. The main issue I have with this paper this that the authors do not provide sufficient proof for this to be the case. For instance, it would be very interesting to show the correlation between satellite derived chl-a concentration and the spatial coverage of the floating macroalgae bloom. If there is a high, positive correlation then

this may give the reader more belief in that fact eutrophication is a driver in the resurgence and extent of the floating macroalgae blooms. In addition, the authors attempt to use satellite-derived Chl-a concentration as a proxy for the AWCPI-NP which itself is a proxy for the eutrophication index. The issue lies in the fact that there exists a low correlation between satellite-derived Chl-a and the AWCPI-NP over the Yellow Sea. Furthermore this article requires a thorough review by a technical editor to correct any grammatical errors. If the editor wishes to publish this manuscript then the following corrections should be made:

———

Dear reviewer,

Thank you very much for your comments and helpful suggestions on the revising. For the interesting topic in the "correlation between satellite derived chl-a concentration and the spatial coverage of the floating macroalgae bloom", this is the point of our work. As we showed, the occurrence of super floating macroalgal blooms was a sudden event in the Yellow Sea and the biomass (or spatial coverage) was subjected to many other conditions, so we couldn't use a traditional linear regression to analyze the link between the biomass (or spatial coverage) of floating macroalgae and chlorophyll concentration (or nutrient index). Alternatively, comparison was made between the two time spans corresponding to the pre-bloom phase (2002-2006) and bloom phase (2008-2012), respectively. For all of your concerns, our reply is given below.

1) That the authors attempt to find a suitable satellite derived water quality parameter with a higher correlation to the AWCPI-NP, or that the authors tone down the conclusions drawn from these results. Response: Yes. Thanks for your suggestion, we toned down the conclusions. In the abstract, the last sentence was changed to be: "The progressive eutrophication observed over the 2001-2012 period is a non-ignorable process for exploring the reasons of the non-linear outburst in the growth of macroalgae, i.e. green tides, in the Yellow Sea since 2007."

BGD
11, C3292–C3298, 2014

2) The first line of the abstract should be reworded!

Response: Yes. It was modified as “World-largest super floating macroalgal blooms of Ulva prolifera have occurred every summer in the Yellow Sea since 2007.”

3) Page 7033, Line 20-25: “or other similar indices (Hu et al., 2009)” the authors should also reference Shi and Wang (2009) and Garcia et al. (2013). Shi. W., and Wang, M.: Green macroalgae blooms in the Yellow Sea during the spring and summer of 2008 : : :: : : Garcia, R. A., Fearns, P., et al.: Quantification of floating macroalgae blooms using the scaled algae index : : :: : :

Response: Yes. We noticed these progresses on this issue. Thanks for your reminding. We amended these references “. . .; Shi and Wang, 2009; Garcia et al 2013.” in the revision.

Garcia, R., Fearns, P., Keesing, J.K., and Liu., D.: Quantification of floating macroalgae blooms using the scaled algae index, J. Geophys. Res. Oceans, 118, 26–42, doi:10.1029/2012JC008292, 2013.

Shi, W., and Wang, M.: Green macroalgae blooms in the Yellow Sea during the spring and summer of 2008, J. Geophys. Res., 114, C12010, 2009.

4) Page 7033, Line 25: “A threshold value of NDVI was set to identify the floating macroalgae patches.” Did the authors set pixels that were classified as ‘algae’ to constitute 100% spatial coverage?? If the authors did not, then the spatial coverage of the floating macroalgae blooms would be vastly over-estimated. Garcia et al. (2013) noted that if the NDVI value of an algae pixel was just above the specified threshold, then it would be unlikely to constitute 100% spatial coverage of the pixels’ area. As such the spatial coverage’s of ‘algae’ pixels were scaled in accordance with their NDVI value. Indeed the method of Garcia et al. (2013) produced a spatial coverage of 455.8 km2 on the 31 May 2008 (see table 3) as opposed to 1200 km2 shown in Table 2 for the same date. The authors should note that the method of Garcia et al. (2013) used

BGD

11, C3292–C3298, 2014

MODIS' 250 meter spatial resolution bands as opposed to the 500-meter bands used here, and as such would theoretically produce higher accuracies.

Response: We agree with you on this issue. A higher resolution would produce higher accuracy. However, due to pixel-mixing effects (see Fig.1 for in-situ photos of floating macroalgae), MODIS' 250 meter spatial resolution bands (Garcia et al., 2013), even Landsat's 30 meter bands (Hu et al., 2010; Xing et al., 2011a) can not ensure a 100% spatial coverage. Resolution of data highly impacted the threshold for classifying coverage. The estimation results of Liu et al. (2009), Hu et al. (2010), Xing et al. (2011a), and Garcia et al. (2013), are highly dependent on resolution of the data they used. To keep the consistency, we used the same strategy of 500 m bands to estimate coverage area of green tide of every year. To avoid some confusions on this issue, one comment was amended: "It should be noted that researchers using different spatial resolution images and difference indices (Liu et al., 2009, Hu et al., 2010, Xing et al., 2011a, Keesing et al., 2011, and Garcia et al., 2013), could give different results on covering area of floating macroalgae. In this work, to keep the consistency with our previous results (Liu et al., 2009; Xing et al., 2011), ...".

5) The authors use the word 'imageries' throughout the manuscript; this should be changed to 'imagery'.

Response: Yes. We modified these and other grammar errors.

6) Page 7035, Line 20-25: "and conservatively collected iomass of green algae: : :." Do the authors mean 'biomass'

Response: Yes. Thank you. We revised the manuscript for typing errors.

7) Page 7037, Line 20-25: "where the super macroalgal blooms outbroke in every summer from 2007, : : :" should be changed to "where super macroalgal blooms outbreak every summer, : : :"

Response: Yes. Thank you for your suggestion. We rewrote this sentence.

BGD

11, C3292–C3298, 2014

8) Page 7038, Line 5-10: “The floating macroalgae could cause false values in the satellite-derived Chl-a (Xing et al., 2014)”. This paper that the authors reference is in preparation, therefore the authors should briefly explain why Chl-a might be incorrect over floating macroalgae-dominated waters.

Response: Yes. We updated this part as “The floating macroalgae could cause false values in the satellite-derived Chl a (Xing et al., 2014), i.e., pixels optically contaminated by floating macroalgae were not completely masked during the processing of standard Chl-a product and thus Chl a values for these pixels were mistaken as phytoplankton-related Chl-a.”

9) Page 7038-7039, Line 25-5: “The increase in Chl a in coastal waters was most likely to be driven by the local eutrophication due to input of excess nutrient because of human induced activities (Morand and Briand, 1996; Seitzinger et al., 2005).” This is a generic statement with references NOT related to the coastal waters of the Yellow Sea. In the next paragraph the authors provide reasons why they believe this to be the case over the Yellow Sea. Therefore this sentence should be changed to “The increase in Chl a in coastal waters has been noted to be likely driven by: : :.”

Response: Yes. Thank you very much for your suggestion. We followed your suggestions: “The increase in Chl a in coastal waters has been noted to be likely driven by the local eutrophication due to input of excess nutrient because of human induced activities.”

10) Page 7034, Line 10-15: “To avoid the uncertainties in satellite-derived Chl-a products in the turbid waters, we extracted the Chl a at the middle of the Yellow Sea”. This is unclear; one cannot ‘avoid’ uncertainties in any satellite-derived parameter. Here uncertainty is a consequence of sensor noise and spectral noise introduced through the atmospheric and sun-glint correction. Both these corrections – that are at times imperfect - are applied to MODIS and SeaWiFS imagery to obtain remote sensing reflectance from which Chl-a concentrations are derived. I suspect the authors are talking about

BGD
11, C3292–C3298, 2014

low accuracies of derived Chl-a concentrations in turbid waters, and therefore obtained the Chl-a estimate in more clearer waters. The authors should provide a reference for this effect.

Given the limited temporal and spatial data to calculate the AWCPI-NP over the Yellow Sea, the authors attempt to use satellite-derived Chl-a concentration as a proxy for this index. However, given the low accuracies of Chl-a over turbid waters, the authors then tried to use the Chl-a concentration over the central Yellow Sea region as a proxy for the Chl-a over the turbid waters. This, I find is the biggest weakness of this paper, particularly since the Chl-a concentration at these two regions may not be correlated with one another. In other words, unless a correlation exists, the Chl-a over the clearer water may not increase as the Chl-a over the turbid water increases and vice-versa. This could by a reason why there exists a low correlation (r2 = 0.48) between AWCPINP index and Chl-a shown in Figure 4.

Response: Yes, we were talking about the low accuracies of satellite-derived Chl-a concentrations in turbid waters. We used Chl-a over the clearer waters, mainly considering the problems of application of OC4 Chl-a algorithm in turbid waters which were mainly caused by the high load of sediments in the Yellow Sea. To make it clearer, the effects and relevant reference were amended: "The high load of suspended sediments mainly due to river discharge and sediments re-suspension in shallow waters, could lead to overestimation in satellite-derived Chl-a in the Yellow Sea (Yamaguchi et al., 2012). To avoid. . .."; and we replaced "To avoid uncertainties" with "To reduce the uncertainties in analysis which might be brought by the inaccurately estimated Chl-a in turbid waters, . . ."

In our view, the attempt of using satellite-derived Chl-a concentration over clearer waters is not a weakness of this paper. For the semi-enclosed Yellow Sea basin, AWCPINP index gave trophic status for the entire basin (as described in section 3.2, and Xing et al., 2011b), and accordingly it is an appropriate strategy of using the Chl-a data over the vast clearer waters to represent the trophic status of the basin rather

BGD
11, C3292–C3298, 2014

than the wrong Chl-a in turbid waters (see also our reply to your concern on the issue of accuracies in Chl-a). Macroalgal blooms mainly expanded in the clearer waters (Fig.1, see also descriptions in Page 7035, Line 8-14.), and so it is better to use Chl-a data in this type of waters. Considering that the observation was not an in-door experiment with controlled conditions and the natural processes were usually non-linear and impacted by other factors, we think correlations shown in our paper were acceptable (e.g., r= 0.48, 0.69, 0.84); this situation was also seen in other cases about the correlations between Chl a and other index, e.g., r = 0.6 (Martinez et al., 2009). We also have to mention that the two different indices of AWCPI-NP index and Chl-a were equally important in this paper: the nutrient-phytoplankton intrinsic coupling (see introduction) and agreement between the two indices suggested that the derived results from them were reliable.

Martinez E, Antoine D, D'Ortenzio F, Gentili B. Climate-driven basin-scale decadal oscillations of oceanic phytoplankton. Science. 2009 Nov 27;326(5957):1253-6. doi: 10.1126/science.1177012.

Interactive comment on Biogeosciences Discuss., 11, 7029, 2014.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper