

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

Anonymous Referee #2

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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 is 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

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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:

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.

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

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., Fearn, P., et al.: Quantification of floating macroalgae blooms using the scaled algae index

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

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

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

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

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, ..."

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.

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 ..."

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

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from which Chl-a concentrations are derived. I suspect the authors are talking about 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 be a reason why there exists a low correlation ($r^2 = 0.48$) between AWCPi-NP index and Chl-a shown in Figure 4.

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