

Interactive comment on “Effects of flooding on organic carbon consumption in the East China Sea” by C.-C. Chen et al.

Anonymous Referee #2

Received and published: 12 May 2015

General comments:

An interesting set of data contributing to a globally relevant issue of increased flooding events and their impact on coastal plankton communities and carbon balance. Considering the topic of flooding, however, not enough is made of potentially major factors such as the impact of increased particulate matter loads and CDOM concentrations on light availability in the coastal water column or of the impact of allochthonous organic carbon inputs on community respiration and metabolic balance. The conclusions of the paper could be strengthened if these factors are considered more.

Specific comments:

1. Primary production method, p.6 lines 20-23: Were samples always taken at same

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time of day? Short incubations so important to consider diurnal variability. 2. p.7 line 17: Reference from 2001 can't refer to the last decade of data if data set reported is from 2009 and 2010. 3. Wording, p.8 line 12: Maybe clarify at times when referring to values from previous studies as can get confusing to reader at times. 4. p.8 lines 18-19: "suggests that the growth of phytoplankton might be limited by the availability of light". Data on suspended particulate matter, CDOM or turbidity may help reinforce this statement and whole argument could be expanded upon more. 5. p.9 line 18: Adding a contour plot of phosphate to Fig. 1 would help reader interpret the nutrient dynamics in the two years of study. 6. Wording, p.9 line 27: Initially unclear that this is referring to a previous study. 7. p.10 line 9: These correlations suggest that biomass is not limited by phosphate at this time. 8. p.12 lines 7-9: Wording confusing. Was cyanobacteria present in the CDW in 2010 or not? 9. p.13 lines 18-24: Could the CR:PP regression and relatively steep slope suggest that allochthonous organic carbon inputs are fuelling a higher CR rate for the same PP rate than in other regions? 10. p.14 lines 16-17: "phytoplankton assemblage varied between both periods". Earlier (p.10 lines 25-26) it is stated that phytoplankton was not identified or enumerated in 2009 and an assumption of potential community composition is made based on other findings. The next sentence goes on to "assume" based on these differences in community composition. Suggest some rewording as these conclusions are a little tenuous. 11. p.14 line 24: a reference could help here. 12. p.15 lines 3-7: Expand upon this argument as this is a more likely scenario than the previously proposed control of CR by in situ PP considering the volume of riverine discharge and the potential for DOM within this discharge to be more bioavailable as flooding will have minimised the amount of time it has spent being reworked by microbes in the soil.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C2127/2015/bgd-12-C2127-2015-supplement.pdf>

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12, C2127–C2129, 2015

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