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

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Chen et al. Effects of flooding on organic carbon consumption in the East China Sea. 2015 Biosciences

General Comments

It is important to distinguish between biomass-limitation (as indicated by [Chl]) and growth rate limitation (as indicated by PP/Chl). Please clarify.

Given the known importance of fluvial (allochthonous) inputs of organic matter (dissolved and particulate) as substrates for CR, the focus on the fluvial input of dissolved inorganic nutrients is unfortunate. To address the "Effects of flooding on organic carbon consumption" measurements of suspended sediments, dissolved organic matter and

particulate organic matter should have been made.

Adjectives like “huge” and “tremendous” are used too frequently and should be dropped in favor of more quantitative terms. Surely they can estimate the actual riverine inputs?

Overall conclusions: The most likely data-based scenario is that phytoplankton production during the period of observation was light limited due to fluvial inputs of suspended matter and CDOM and that the increase in CR was primarily caused by an increase in allochthonous inputs of organic matter (dissolved and particulate).

Specific Comments

1. Primary production, p. 6, lines 18-20: What time of day were the samples collected and incubated? Diurnal periodicity is an important factor here, especially when using short-term incubations (2 hours) to estimate daily rates. 2. p. 7, line 17: How can a 2001 publication provide data “over the last decade” (2000-2010)? 3. p. 8, lines 1-2: When were the stations occupied (samples collected) relative to the peak in river flow? 4. p. 9, lines 17-19: In regard to phosphate limitation, please include the distribution of dissolved reactive phosphate in Figure 1 to support this possibility. What about light limitation as suggested above (p. 8, lines 18-19)? 5. p. 10, lines 7-8: This is confusing. It is stated above (p. 9, lines 26-27) that concentrations "reached bloom criteria" (> 20 mg/m3). 6. p. 10, lines 8-10: This correlation suggests that biomass was not limited by phosphate concentration. 7. p. 10, lines 15-16 (“. . .the phytoplankton biomass in the surface water (Table 1), or average over ZE (data not shown), did not did not differ significantly between 2009 and 2010.”): This is based on concentration (mg/m3). If one defines the CDW zone by the same isohaline (e.g., 31 psu) for each year, what was the total Chl content of the CDW and was there a difference? 8. p. 10, 18-19: How does PP/Chl compare between 2010 and Chen et al. (2009) estimates of the ECS in summer? 9. p. 10, line 22: Should this be “abundance” or biomass? Abundance would be measured in terms the number of cells while biomass is measured in terms of mass. 10. p. 13, lines 1-4: As for chlorophyll, how does bacterial biomass integrated

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over the area of the plume (defined by the 31 psu isohaline) compare? 11. p. 14, line 17: It might be "reasonable" to speculate, but not to assume. 12. p. 14, lines 23-24: A reference for how PP/CR may be interpreted as an index of whether or not the system is autotrophic or heterotrophic should be given here. Also, as for phytoplankton and bacterial biomass, this should be calculated for the CDW as a whole (integrated over the area of the plume as defined by the 31 psu isohaline). 13. p. 15, lines 3-10: This is most likely and is contrary to the conclusion above that CR is controlled by phytoplankton production. The conclusion that the CDW may be a heterotrophic system is also consistent with large amounts of allochthonous (fluvial) organic matter inputs. If CR were "controlled" by primary production, the system would probably be autotrophic. 14. p. 16, lines 12-13: High relative to what?

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1503/2015/bgd-12-C1503-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 5609, 2015.