

***Interactive comment on* “The effect of typhoon on particulate organic carbon flux in the southern East China Sea” by C.-C. Hung et al.**

Anonymous Referee #3

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General comments: This paper provides in situ observations of vertical profiles of various parameters, especially POC fluxes, after typhoon passage. Such kind of observations is very rare and thus valuable, although enhance of POC export after typhoon passage could be anticipated after so many cases of hurricane/typhoon induced phytoplankton bloom were observed. An additional strength of this paper is the use of multiple approaches (in situ, satellite and model). However, there are spaces to improve. Details of methods are not provided, for example, for C14 PP measurement, is it 24 h incubation? And some of the data are not clearly presented and explained.

Specific comments: 1. The authors should do a careful proof-reading. There are some obvious mistakes. For example, P3525, L16 should be “typhoon”, rather than “typhoons”; Table 1, TD should be MLD. 2. P3525, L1-3: “the cited reports were based

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on study of satellite images, and the reported phytoplankton biomass (e.g. usually determined from chlorophyll absorption spectra)". Actually almost all of the reported biomass change was estimated by using the empirical band ratio algorithm (OC4) rather than deriving from absorption spectrum. 3. Annotation of dates on figures is not consistent and thus is confusing. For example, on Fig.1, it is 02/08/2008, while on Fig.3, it is 8/2/08. 4. Fig.3, T/S figure is not comprehensive. Why not show T & S profiles? 5. Fig.4, Is the MODIS Chl from Aqua or Terra? It looks that there might be something wrong with data processing. Never seen those kind of high chl ($\gg 1$ mg/m³) bands across the Taiwan Strait. I would like to suggest the authors to double-check the data processing and use the newly updated MODIS data (Version r2009.1). One way to convince people about the data quality is to compare the MODIS data with SeaWiFS data (also use Version r2009.1) when both having data (because there is no SeaWiFS data during the period under study). As to the reason why in situ chl is higher than satellite chl (p3533, paragraph two), it is not likely to be item 2-3. 6. Logic is not clear. The authors actually plan to show two cases. So it is better to mention this clearly, rather than mentioning only one at first (in Section 1 & 2) and then mentioning one more in Section 3. And there is no need to repeat the description of cooling in the discussion section. 7. There is a pre-existing upwelling in the study area, right? It looks like that typhoons enhance the upwelling and/or mixing. In a word, I see weak physics in this paper. Clearer and more careful interpretation is necessary. 8. It is not good to compare 07 with 08, to represent the differences between pre-typhoon and post-typhoon. At least the authors should keep caution of potential interannual variations. I suggest using satellite data to evaluate this impact.

Interactive comment on Biogeosciences Discuss., 7, 3521, 2010.

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