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Comment

## ***Interactive comment on “Behavior and fluxes of particulate organic carbon in the East China Sea” by C.-C. Hung et al.***

**Anonymous Referee #1**

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General comments: This paper reports the direct measurement of POC fluxes and primary production in the East China Sea and applied a vertical mixing model to correct the effect of bottom sediment resuspension, which representing a huge challenge in the constraint of POC fluxes in a continental margin due primarily to the dynamic nature of these systems. In my opinion, the data are valuable and could constitute a welcome addition to the global database even though the study is regional in scale. While I believe this paper should be ultimately published, I have a series of concerns/suggestions listed below. The biggest issue is the application of the vertical mixing model which is a selling point for this paper. However I can not comment on the validity of the model because there is not enough information presented here to make a good judgment. From table 2, the big difference between the model-derived Cs and the measured Cs suggested the unreasonable extrapolation for this model. The assumptions that go into

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the model should be examined rigorously to scale its validity.

Specific comments: 1, section 3.1 (page 4276) “An interesting phenomenon is that at stations 5, 10, 26 and 28, maximum Chl a concentrations were always observed above the depth of the euphotic zone (Table 1) and decreased with increasing depth.” This should be slightly discussed within this paragraph. 2, section 3.1 (page 4277) the authors claimed that at station 18, POC at the lower depth were higher than at shallow depths. However this trend is not clear in Fig.3 for the absence of the data at the lower depth. Please indicate it. 3, section 3.2 (page 4278) please specify the seasons for the POC flux from Iseki et al., (2003). There is a big difference of the reported values for one station. Is it due to the seasonality or other reasons? 4, section 3.3 (page 4278) the authors claimed that one of reason for the low PP at S5 is light effect due to high TSM. However TSM of S5 is similar to other stations in the inner shelf. Why not light effect in the other stations in the inner shelf? 5, section 3.4 (page 4279) the sentence “The measured POC values in the surface sediments in the ECS in this study ranged from 0.08 to 0.61% (an average value =  $0.30 \pm 0.16$  %).” This range is inconsistent with the values in table 2 (0.07-0.52). Please check and revise it. 6, section 4.1 (page 4281) the authors claimed that “It is reasonable to predict that in situ phytoplankton species composition and abundance are mainly responsible for production of POC, thus result in good correlations among these parameters in the ECS.” How to draw this conclusion? It can be inferred from the relationship of POC and Chl a is that the production of POC might be affected by the phytoplankton activity. If the author could divide Fig 4a into the subset like Fig 5 and have different POC/Chl a values for different areas, it might be an indication of the effect of phytoplankton species composition on the POC. 7, section 4.1 (page 4281) How much the POC/Chl a changed with such a small change in the contribution of the main phytoplankton group to autotrophic carbon (75-80%)? Please state it. 8, section 4.1 (page 4282) the last sentence in the last paragraph has not logical relationship with the former sentences that discussed about the possible impact of high POC flux in the inner shelf. It's ambiguous to mention table 1 in this sentence. 9, section 4.3 (page 4283) the sentence “As a result, the POC exports

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in the outer shelf of the ECS are roughly 26 and 33mgCm<sup>-2</sup> d<sup>-1</sup>, respectively.” Did POC exports here mean the lateral transport out the outer shelf? If yes, please clarify it to avoid misleading. Also it might be interesting to roughly calculate the carbon budget in this area 10, section 4.4 (page 4284) the title for this section is “Possible carbon export in the outer shelf of the ECS”. However the authors also discussed the e ratio in the inner shelf. 11, table 3 the Cs comes from the average of the measured values. It would be better to delete “intercept ” to avoid misleading Technical corrections: 1) Page 4275, Line 22: “The PB-E” instead of “ThePB-E” 2) Page 4275, Line 24: add “.” after “light” 3) Page 4276, Line 4: “concentrations in” instead of “concentrationsin” 4) Page 4278, Line 20: delete “in later section” 5) Page 4281, Line 13: “abundance” instead of “abundant” 6) Page 4283, Line 20: please add the reference “Fang et al., 2007” in the bibliography 7) Page 4298, Fig. 1: “sampling locations (black dots) and hydrography” instead of “sampling locations (black dots) of hydrography” 8) Page 4300, Fig. 3: it would be helpful if the resolution of the figure would be improved; also more contrast colors would be welcome. 9) Page 4302, Fig. 5: Please delete the line below the figure for the clearness

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