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

Anonymous Referee #3

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General comments

1-The authors present interesting data on POC distribution, primary production and POC fluxes on one of the largest marginal seas in the world as the East China Sea. These kind of studies where PP and POC fluxes are coupled together provide a useful tool for a better understanding of C cycling in marginal seas. The effort of discriminate the resuspension contribution to the flux by mean of a simple two end member mixing model gives an idea of the relevance of this process to the measured fluxes however it needs a further revision especially in the proper choose of the end members. The data presented refer only to the summer season and this is a limitation to the generalization of the results on POC fluxes and dynamics in this marginal sea.

2-The application of a simple two end member mixing model is highly dependent on the

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values assigned to the end members. The authors use as end member for resuspension an average of OC concentration obtained from their work (but there is no mention to the sampling and analyses of bottom sediments in the methods) and from previous studies carried out in the same stations many years before without specifying the season. Especially in shallow areas the dynamics of sediment settling and resuspension can be highly dependent on the seasonal and interannual changes (meteo-marine conditions, phytoplankton blooms, zooplankton successions, etc.). This points should be better discussed.

3-As there is a high variability in the concentration of organic carbon in bottom sediments why the authors do not use the concentration for the same seasonal period (i.e. summer) for which they have all the other data? The use of data averaged on different years and seasonal periods could be misleading in choosing the proper end member.

4-In the discussion it is not clearly addressed the role which could have bacterial degradation of OM during the settling of OM in the different sub-areas of the East China Sea. Moreover why different deployment time of the drifting traps were chosen spanning from 3 to 8 hours? Could a significant bacterial degradation of settled organic matter occur in this time span as no preservatives were used inside the sediment traps?

5-I think that in order to discriminate the contribution of resuspension versus marine produced matter the use of stable C isotopes and of major metals constituting the minerals of the fine fraction of seabottom sediments should be used to better check the validity of the estimates based on TSM and OC.

6-Though a part of the East China Sea is under the influence of the river Changjiang overall in the paper it seems that the direct deposition of riverine POC is not properly considered as only resuspension and marine autochthonous organic carbon are discussed. However the authors state (P. 4282, L.3-5) on the basis of previous results that up 50% of the OC can be of riverine origin. This aspect affect also the consideration of the authors for the high export ration in the CDW (Changjiang Diluted Water) area. The

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authors explain that this could be due to a PP limitation by light intensity due to high turbidity or by nutrient limitation or by strong vertical mixing. It is very improbable that in turbid waters under riverine influence there is a nutrient limitation. Moreover the strong vertical mixing should be occurred during or just before the cruise and this should be demonstrated by thermohaline profiles and /or meteorological conditions (e.g. wind direction and intensity) during the sediment traps deployment and in previous days.

7. The authors measured the fluxes during the daylight and assumed that no variation in the fluxes occurred during the night. This affirmation is not properly supported also by references as some include not only the night but also day time (P.4285 L.15) moreover they refer only to a particular season (not specified). This assumption could lead to an underestimation of the flux due to the vertical migration of zooplankton (and related faecal pellet flux) especially in deeper areas where the traps were deployed quite distant from the seabottom.

8. In the conclusions that Authors do not consider that in shallow areas under riverine influence with turbid waters there could be a limited primary production but a high flux of riverine and resuspended particulate matter this could imply that the higher POC flux with respect to primary production are not necessarily overestimated. I think that there is a need to better address this issue in the discussion.

Specific comments In the title (P. 4271) it would be more correct to refer to summer. In the Abstract (P.4272) the sentence L.15-19 is not clear: “in assessing reasonable quantitative estimate” of what? In the Table 1 the depth of the euphotic zone is presented however in the “Sampling and analytical methods” section (P. 4274-4275) it is not reported how it was determined. No information on sediment sampling and analysis is presented in the “Sampling and analytical methods” section, though this data are reported in Table 2. The authors describe in the method the use of transmissometer (P.4274 L. 9) but they do not present nor discuss this data which could be useful in relationship with total suspended matter and POC. There are no details on the washing of the suspended matter in order to determine TSM (P. 4275, L. 4-8), nor on the

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method used with precision and accuracy of this measurement. As washing could cause osmotic shock on the phytoplankton cells it could lead to an underestimation of POC concentrations. It would be useful if the authors could specify the details of HCl fuming (P. 4275, L.6) in order to remove carbonates as a not efficient removal could lead to an overestimation of POC concentrations and fluxes. The authors state that the "at each sampling depth the PB-E curve was determined using a seawater –cooled incubator"(P. 4275, L. 18-25). Which where the temperature of incubation for primary productivity and which where the differences with respect to the real in situ temperatures? Could the temperature difference (incubation versus in situ) affect the estimate of the primary productivity? In the methods section should be specified how were the PP data integrated on the water column.

In the discussion the comparison with the primary production derived by satellite data easy to understand: 1) are the satellite data average for the whole year or for shorter period? 2) they are 3 fold higher than the summer fluxes reported by the authors and this important issue is not discussed.

Technical comments Units for the Chla concentrations are expressed sometimes as mg m⁻³ and sometime as ug L⁻¹. It would be better to use uniform units throughout the text and as liter and not m³ are filtered as for POC and TSM the ug L⁻¹ should be preferred. P. 4276, L.4. Correct: "concentrationsin" and "July2007" P. 4275, L.22. Correct: "ThePB-E" The reported data (P.4277, L.17) of Khodse et al. (2009) seem to be quite different from the Authors' results when converted from mole g⁻¹ to g g⁻¹. When comparing the data with previous findings P. 4278, L. 5-9. the authors should consider the seasonal period (more interesting than the nationality of the researchers, which could be avoided). P. 4278, L. 10-15. The same comments as above apply also for the comparison with previous PP data. P.4281, L. 13 "phytoplankton cell abundant" should be substituted with "phytoplankton cell abundance". P.4281, L.13-15 The sentence is unclear: what is the meaning of the specification between the parenthesis? P. 4283 L.19. Correct: "organic phosphorous mineralization" P.4284 L. 14. The authors

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introduce “Another possible transport pathway” but it is not clear which it the former pathway. P. 4284 L.21. Correct: “currentsflow”

Figures Fig. 2 (P.4299). In the caption change “POC concentrations” with “POC concentration”. Fig. 4 (P.4301) Insert the statistical significance of the relationships presented. Fig. 5. (P. 4302). It is useless to use the symbol S in the X axes it would be better to use 1/TSM as in Fig. 4.

Tables Table 2 (P. 4294). For most stations the data presented in the ms are lower than previous data. How do the authors explain these differences? Table 3 (P. 4295) No relationship is presented in figure 5 for SMW, why? However for SMW it is used the values of the slope obtained for CDW. This aspect should be explained in the text. Table 4 (P. 4290). The caption should contain the explanation of the symbol used : Ct, R and T. Table 5 (P. 4297) In the caption correct: “ofcorrected”. In the title of one of the column “bottom” should be substituted by “Bottom depth”.

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