

Interactive
Comment

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

Anonymous Referee #2

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Carbon (and also other elements) are highly dynamic in marginal seas. In the case of the East China Sea, this is especially the case due to strong marine (Kuroshio) and terrestrial (Yangtze R.) interaction. The shallow depth and broad width makes the region a hot study site for carbon cycles and budgets. It is also due to the high dynamics, results in marginal sea should be treated with more care, whereas in the case of the open ocean, distribution and variations are more or less already-known. The authors presented here a POC flux work in East China Sea, which focused on the sediment trap data. The high light is they try to quantitatively estimate the resuspension effect, though the calculation method is derived from literature. The results suggest that 49–93% of the POC flux in the ECS might be from the contribution of resuspension 15 of bottom sediments rather than from the actual biogenic carbon sinking flux. One can feel that great effort has been applied to these original data set. Moreover, this is not

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the original version and the authors should have considered the editor's suggestions and comments. After read this ms, however, there are still several key problems that should be overcome before it can be considered for publication in biogeosciences. Following are the comments:

Key problems. 1. The authors presented both PP and vertical flux result in the ms, while they emphasize that the fluvial input is somewhat not obvious in the season and in these region. If this is the case, the vertical POC flux at the bottom of the euphotic layer is a novel result, noting that there is already a published work in the same region (e.g., Iseki et al., 2003). Although Iseki et al's work is mentioned in this ms, the vertical POC flux is not compared and discussed in the whole ms. It is likely that the Iseki et al's result is quite different from the authors' result. It does not make sense that 10 years later, the vertical flux changes such a lot. The authors should carefully explain the reason. Otherwise, this indicates that there is something wrong either in the former work, or in this presented study.

2. there seems to be problems in the original data set. As indicated by fig. 2 and fig. 3, I highly doubt about the original data quality. According to fig. 2, POC at station 19 is ~450 ug/L, whereas at station 26 the POC is only ~50ug/L. But the TSM in these two stations are almost the same, as indicated by fig. 3. If this is the case, POC% at station 19 would be something like 22%. So far as I know, this is not possible and there should be something wrong. The data quality is essential, as flux result is highly depending on POC and TSM concentration.

3. Sediment OC%. The authors seems also measured sediment OC in this ms. According to the resuspension calculation equation, sediment OC% is a key parameters in this study and the calculated resuspension contribution to POC flux is highly depending on the sediment OC content. Firstly, I failed to find the method description in MATERIALS AND METHODS so I have no idea how they obtain and measure the sediment OC%. The key problem here is sediment grain size and sediment OC% variation from station to station. The authors investigate almost the whole East China Sea, so

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they actually covered a complex surface sediment grain size, ranging from over 64 μm (sand) to less than 4 μm (clay). OC content (OC%) is highly depending on the sediment grain size and hence the whole study area would have a notable variation in sediment OC%. If I was doing this calculation, I would do the resuspension-contribution calculation with the exact sediment OC% data at that station. I would say it is not persuasive, or wrong, to do the calculation with a uniform OC% parameters for all the stations without considering the differences of sediment OC% from station to station.

4. negative values. Why the result for KW is negative (p4282, line 22). The authors should explain this in much more details quantitatively. Is it because this model is not applicable to this region or to this data?

5. Pore size. POC, TSM and Chla in this ms is collected by GFF, but suspended particles in sediment traps seems to be collected by quartz filters (p4275, line 2). Is the pore size the same? The pore size introduction is missing. If this was not the same, there would then be a pore size problem in the POC flux calculation.

6. the chla data. Chla seems not to be a key parameters in POC flux calculation. But this also affects the reader's confidence about the authors data quality and the way they carry out research, and hence the Journal quality. First problem is the collection. It seems that only 500 ml water is used for chla determination. It may be OK in the high chla region, but how about in the estuary and oligotrophic offshore? Secondly, did they do the filtration under mild vacuum? Was it performed in dim light? There data quality control seems to be missing. Third, I have no idea whether MgCO_3 should be used or not here. In this presented study, it seems not being used. Fourth, in the Kuroshio region, majority of chla is contributed by pico-phytoplankton, the size of which is usually less than 0.7 (the authors' GFF). In this case, usually more water should be filtered to minimize the problem. What's worse, as GFF pore size 0.7 μm is statistical result (i.e., the average pore size of the whole filtration procedure, probably from large particle size in the beginning to small particle size in the end), so if only 500 ml is filtered, the real status for the filtration on board is then more likely that they only obtained particles with

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size probably larger than 0.7 μm .

7. section writing. Although has been suggested by the editor, this version I get still seems to have the problem of “mix the result and discussion”. For example, p 4277 line 0-10, and p 4278 line 14-15, these two parts seems to be discussion, not result. I would suggest the authors check this problem again thoroughly.

Minor problems and suggestions. 1. Physical background names. In figure 1, the authors give several names (abb.): YSW, CUW, KW, CDW, TCWW... Besides the commonly accepted TCWW, KW and CDW, what is the reference for YSW and CUW? For example, why there is Yellow Sea water (YSW) in the East China Sea? As for the CUW, the authors surface temperature distribution patterns seems not supporting that this region is an upwelling region in this study. 2. Another small problem is the term. The authors widely use “POC in sediment”. I would suggest they use “OC in sediment” or “OC content in sediment” or “OC% in sediment” instead. 3. fig 3: it would be better if the euphotic layer depth is indicated in this figure. 4.the title: I would suggest the authors use only the word “flux”, but not “behavior”. Smaller title sometimes helps. 5.the equations. The term $1/\text{TSM}$ and $1/\text{S}$ sometimes seems to be the same. It does not make sense to use different terms within one ms. I would say it is be better if we choose one and use it uniformly in the whole ms. 6. table 2: grain size data (e.g., D50) needed. 7. if you have overcome the key problem2 above and prove that there is no problem in data quality, then to make the whole work more beautiful, I would suggest you also do the TSM calculation like you did to POC, which makes the ms more significant in science.

literature cited: Iseki, K., Okamura, K. and Kiyomoto, Y., 2003. Seasonality and composition of downward particulate fluxes at the continental shelf and Okinawa Trough in the East China Sea. Deep-Sea Research. Part II, 50: 457-473.

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