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Biogeosciences Discussions

Interactive comment on "Insignificant enhancement of export flux in the highly productive Subtropical Front, east of New Zealand: a high resolution study of particle export fluxes based on ^{234}Th:^{238}U disequilibria" *by* K. Zhou et al.

Anonymous Referee #2

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In this manuscript Zhou et al. present results on POC export based on 234Th measurements at high spatial resolution in the Subtropical Front area between New Zealand and the Chatham Rise during late-autumn early winter. Results show low to moderate export fluxes during this period over most of the study area, despite large variability in fluorescence and POC. The study presents several flaws (listed below) that should be addressed by the authors.

1) The horizontal distribution of POC stocks and chlorophyll or fluorescence are not shown so it is difficult for the reader to see how they relate to the export estimates.

2) One that is typical for this kind of studies is that POC measurements in suspended matter and the resulting C/Th ratios are actually assumed to be representative for sinking matter. As a result, the variability in estimated POC export could as much reflect export as variability in POC concentrations in the samples. Further, as mentioned before, the horizontal distribution of POC stocks is not shown leaving the reader at odds to understand which of those two factors is actually determining.

3) The authors have studied the system presumably at the end of the growth season, possibly when silicic acid concentrations were low. No information about the latter is given in the manuscript, even though the authors mention the importance of community composition, in particular diatoms, in determining export. In addition, although phosphate concentrations are mentioned, they do not seem to refer to the study period. Could PP be low during this study because of limitation by macro-nutrients?

4) In the same line as the previous comment and despite the authors' statement that this was not the case, based on visual inspection of salinity and temperature profiles, the mixed layers, during this study seemed quite variable but mostly quite deep. One might argue that under these circumstances, PP must again have been quite low and export proportionally high. In short, I am not sure that the results are discussed in the proper context.

Additional comments:

p. 9538, line 5:"free floating cylindrical moorings". Are the traps moored or free floating?

p. 9538, lines 22-27: It is not clear what the purpose of the description of fluxes in other areas (South China Sea, ALOHA etc ...) is. Can the authors be more specific what is particular to these areas that also applies to the region were this study was carried out? Otherwise this part of the text could be removed.

Section 2.3: For the sake of consistency why not use PON instead of PN?

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p. 9542, lines 2-3: from visual inspection of salinity and temperature in Fig. 4, I do not understand how the authors can claim that the upper water column was well stratified. On the contrary, I see quite deep mixed layer at station C17 ($\sim$ 70 m) and C11 ($\sim$ 130 m). This is also reflected in the fluorescence, although this parameter is more "noisy" (also not surprising).

p. 9543, lines 14-16: I am not a specialist in the use of 234Th to estimate export and am quite surprised that only 10% of 234Th activity is found associated to particles. How can 234Th be a proxy for particle export when such a small fraction actually adheres to particle?

p. 9546, lines 4-5: Even though temperature and salinity differences at the C3 site are not as large as for the C4 site, the differences still indicate that also at C3 a shift in water mass occurred. Unless the authors present evidence that this is not the case, their assumption and the use of a NSS model is not valid, even though the NSS and SS models give similar results.

p. 9551, lines 4-5: The probabilities indicated in the text for fluorescence do not correspond to values in Table 3.

Fig. 1: the numbers on the color scale are too small to be read. The same applies to the station numbers and the transect line is difficult to see.

Fig. 2: It would help the reader to understand processes such as mixing if the authors put the acronym of the different water masses (both surface and deep) in the figure.

Fig. 5 & 6: As in Fig. 1, the numbers on the color scale and in the plots are much too small to be read.

Fig.9: It would be helpful if lines showing the limits for the different areas considered (low, mid and high salinity water) were reported in the figure.

Interactive comment on Biogeosciences Discuss., 8, 9535, 2011.