

Interactive comment on “Particle-associated dissolved elemental fluxes: revising the stoichiometry of mixed layer export” by A. N. Antia

Anonymous Referee #1

Received and published: 4 May 2005

Review of "Particle-associated dissolved elemental fluxes: revising the stoichiometry of mixed layer export" by A.N. Antia

General comments

In this work C/N/P/Si and Corg/Cinorg ratios have been derived from the analysis of material collected by the sediment traps. Unlike most previous studies of this kind, the material released to the collection cups, determined from the analysis of the supernatant, has been taken into account for calculating the total elemental fluxes. The resulting C/N/P ratios are in much better agreement with those obtained by isopycnal mixing analysis than the ratios based on the analysis of the particulate matter alone. The C/N/P and Corg/Cinorg ratios are important indicators for the CO₂ uptake efficiency of the biological pump, and so the topic is relevant and suitable for Biogeoscience Discussions. The approach is interesting, and the arguments and interpretations made

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by the author generally sound. However, I have one major problem (see points 1 to 3 below) that needs to be addressed before I can recommend the paper for acceptance. In addition there are some other minor comments/suggestions:

Specific comments

1) The excess concentrations of dissolved inorganic ($\text{NO}_3, \text{NO}_2, \text{NH}_4$) and organic nitrogen, phosphorus, silicon and calcium, found in the supernatant, may result either from the degradation/dissolution or passive leaching from particulate matter collected in the sampling cups. The author assumes, “that the excess dissolved concentration in supernatant water are not an artefact produced by degradation of material within the cups but result solely from passive leaching of material that arrived in the trap in the interstitial spaces of particles.” In order to support this hypothesis it would be helpful to include data, if available, on oxygen concentration in the sampling cups after recovery of the traps.

2) The author did not measure the dissolved inorganic carbon (DIC) in the supernatant water. She ignored DIC production because ‘ÉProduction of DIC though microbial activity would indicate ineffectiveness of the trap poison, which at the level used (0.12% HGCL2) stop bacterial activity within hours (Lee et al., 1982)” and “cups water was well buffered ($\text{pH} > 7.6$) leading us to assume no significant DIC increase” (see page 282 line 15 -18). Nevertheless, in spite of poisoning the cups, the DIN and DIP (products of bacterial degradation) showed significant increases, and so there is no reason not to expect an increase in DIC as well, especially if one suggests that the dissolved compounds have mainly been formed in the water column where aggregate are ‘hot spots’ of bacterial activity. I think the DIC produced during the decomposition of organic matter needs to be included into the calculation of C/N/P/Si and Corg/Cinorg ratios. This in my view is the main problem that needs to be addressed.

3) The argument about pH is not very satisfactory. Besides the dissolution of CaCO_3 increases pH that could have countered the decrease due to organic matter decay.

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4) Scholten et al. (cited by the author) determined the ^{230}Th -trapping efficiency of the OMEX traps which seem to be the same traps used in this study. The Th-trapping efficiency was $< 100\%$, sometimes as low as 40% , but at OMEX -3 the trapping efficiency was also $> 100\%$ at water-depth > 1440 m. It would be interesting to see in more detail to what extent the determination of elemental fluxes is affected by hydrodynamic biases and by the exclusion of the dissolved fraction during the calculation of elemental fluxes.

5) In addition to SD as given in Tab 1 - 3, I think, it will also be important to include error ranges caused by the analytical methods.

6) Fig. 2: Y-axis is not labelled

7) a conclusion chapter could be added to the manuscript.

Interactive comment on Biogeosciences Discussions, 2, 275, 2005.

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