Biogeosciences Discuss., 5, S1664–S1667, 2008
www.biogeosciences-discuss.net/5/S1664/2008/

Biogeosciences Discussions

Interactive comment on "Sinking rates of particles in biogenic silica- and carbonate-dominated production systems of the Atlantic Ocean: implications for the organic carbon fluxes to the deep ocean" *by* G. Fischer and G. Karakas

Anonymous Referee #3

Received and published: 9 September 2008

The authors address a fundamental and difficult question in biogeochemistry regarding aggregate characteristics. They describe aggregates composition and settling velocity, both horizontally and vertically, that are important parameters driving the total flux of aggregates to the deep ocean. To do so, they use interesting sediment traps data from the Atlantic Ocean at different seasons and contrasting trophic conditions. They complete their approach by a modeling study in order to estimate the total organic carbon flux off the NW African upwelling region.

The paper is well written and clear, even if a distinction between sections results and

discussion would have been appreciated. The data set presented by the authors is of great interest for the study of processes affecting aggregate remineralization and carbon flux to the deep ocean. First, the authors demonstrate that sinking speed of aggregate increases from the mesopelagic layer to the bathypelagic layer which is an important result for the field. Second, the authors try to demonstrate that higher sinking rate is observed in carbonate dominated environment compare to opal-rich production system. However, I do not see any evidence of this second aspect in their data set. Such conclusions need to be highlighted by statistics that were not performed in this paper. Fig 3c and Fig 4d were mainly used to reach these conclusions (section 3.1 pp 2550) but error bars would have been necessary.

Sinking speed of aggregate can be driven by mass properties (aggregate chemical composition, density...) and/or shape properties (size, porosity, fractal dimension...). The present study is of great interest in order to better understand carbon flux and sequestration into the deep ocean even if the authors only focused on mass properties and give little concern to the second characteristic (shape). I believe that this study will provide useful information to the literature in the field but needs major revision before being considered for publication in Biogeosciences. The revised paper could not be accepted without stronger results supported by relevant statistical tests sustaining authors' conclusions.

Specific comments

Page 2543 line 10: "Small zoo and phyto... sinks predominantly". This sentence needs to be change because zooplankton do not sink but migrate in the water column. It's the dead organisms, molt, fecal pellets... that sink.

Page 2543 line 20: Is "They" referred to particle characteristics? I would suggest being more specific.

Page 2546 line 10-11: Why only fraction < to 1mm has been used by the authors for the particle flux data? Aggregates larger than 1mm can be frequently encounter

BGD

5, S1664–S1667, 2008

Interactive Comment

in sediment traps and their contribution to the total elemental flux can be substantial. Are there any consequences to remove this important aggregate size fraction? How this could change the estimation of the settling speed of aggregates? These large aggregates can have really high settling speed (100s md^{-1}) and their contribution to the total flux may be important. Is there evidence that the 2 methods used in the paper to estimate aggregate settling speed (time shift of major peaks and the correlation methods of Berelson (2002)) provide similar results when the reduce fraction of flux (aggregates < 1mm) or the total flux (all aggregates) are considered?

Page 2549 line 1: Why the authors choose 2 different remineralization rates for small and large aggregates (0.18 and 0.06 d^{-1} respectively) and why those values? This needs a reference.

Page 2549 line 20: 1) Large aggregate settling speed in the model is set at 75 md^{-1} referred to results of Helmke et al (2005). However, if I understood well, the authors calculated that the settling velocity of aggregates varied from 73 to 741 md^{-1} with a mean value of 210 md^{-1} (page 2550 line 1-2) and settling speed at CB in the present study vary between (125 and 288 md^{-1}: Table 1). Why the authors decided to select a settling speed of large aggregate inferior to the one calculated in the present study? This point need to be discussed in the manuscript. 2) Gruber (2006) used a settling speed of 1 md^{-1} for the small fraction of aggregates. Did the authors conserve this value? This has to be mentioned in the text. Then the settling compartment of the model is composed by 2 categories of aggregates: One that settle very slowly (1 md^{-1}) and one that settle fast (75 md^{-1} or 150 md^{-1} during summer time). To summarize almost all flux is attributed to large aggregates. How this could affect the carbon flux calculated by the authors?

Page 2550 lines 12-15: "Lowest... NW Africa": Are those differences statistically significant?

Page 2550 line 19: "We observe highest rates at and...". Something is missing in this

BGD

5, S1664–S1667, 2008

Interactive Comment

sentence.

Page 2555 line 10: What the authors mean by "mode of particle transport"?

Table 1: When possible can you provide the standard deviation of calculated sinking rates?

Fig 1 (caption): Need to provide definition of the following abbreviations (CB, CV, BO, WR, and PF)

Fig 3c and 4c: Need to add error bars to the mean normalized values of settling speed

Fig 6: Labels (2003/2002) for the X axis may have been inverted.

Interactive comment on Biogeosciences Discuss., 5, 2541, 2008.