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Interactive comment on "

Ballast minerals and the sinking carbon flux in the ocean: carbon-specific respiration rates and sinking velocities of macroscopic organic aggregates (marine snow)" *by* M. H. Iversen and H. Ploug

M. H. Iversen and H. Ploug

morten.iversen@uni-bremen.de

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Ms. Biogeosciences Discuss., 7, 3335–3364, 2010

Corrections in response to the review of Iversen et al.: Title: "Ballast minerals and the

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sinking carbon flux in the ocean: carbon-specific respiration rates and sinking velocities of macroscopic organic aggregates (marine snow)"

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Reviewer #2: General comments I liked this paper very much. It was well written and interesting. I made a number of comments and editorial suggestions on the pdf file attached below.

Specific comments The specific comments contained several suggestions to improvements of the written language in the MS (see pdf-file: bgd-7-C1681-2010, provided by the second reviewer). These have all been included in the MS. Page: 3336 1) Reviewer: Armstrong et al hypothesized that the organic matter was 1) protected by the minerals, or that 2) the organic matter glued the minerals together. This second idea is important because as organic matter is decomposed, then the particles would fall apart and become smaller (and perhaps sink more slowly than otherwise). Have the authors thought about whether any of their data applies to this question?

Response: The idea that the organic matter within aggregates (e.g. TEP) acts as the glue binding the aggregate together, has been observed in several studies (e.g. De La Rocha et al., 2008; Global Biogeochemical Cycles, 22, doi:10.1020/2007GB003156). However, we did not follow the dynamics of abundance and size distribution of the aggregates formed within the roller tanks in detail, and cannot identify if the small aggregates that occurred within the roller tanks originated from large aggregates that had fallen apart due to decomposition or from small suspended particles which collided and formed aggregates. Therefore, our data does not apply to the second idea of Armstrong et al. 2002.

2) Reviewer: lower? On p. 3345, and in Fig. 4e, it states or shows a lower L?? Or is there a confusion as to what L is? In fig. 4e, it has units of per meter, whereas remineralization length scale should be in meters. This needs to be clarified.

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Response: The reviewer is correct, it should state lower L for calcite-ballasted aggregates compared to opal-ballasted aggregates. The degradation length scale is in units of per meter, since it states the fractional degradation an aggregate has per meter it settles, which is defined in line 263 to 265: "The remineralization length scale, L (m-1) is calculated by dividing the carbon-specific respiration rate with the settling velocity of the aggregates, and it expresses the fractional remineralization in aggregates per m settled".

Page: 3337 1) Reviewer: Again see glue hypothesis. See also De la Rocha et al. GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 22, GB4005, doi:10.1029/2007GB003156, 2008.

Response: The findings by De La Rocha et al. has been included in the MS (line 69-71): ". However, a study by De La Rocha et al. (2008) showed no evidence supporting calcium carbonate as a better carrier of organic matter than opal."

Page: 3338 1) Reviewer: The ballast ratio hypothesis as stated by Armstrong et al suggests that mineral-associated organic matter (which appears to make up less than 5% of the total mass) might be protected. In the respiration experiments described here, respiration of the more labile material was undoubtedly measured. So I don't think that these experiments necessarily negate the possibility of mineral protection.

Response: This has been clarified in line 83 to 85: "However, the study by Ploug et al. (2008b) only investigated the degradation of labile organic matter and cannot exclude that mineral-associated organic matter is protected."

2) Reviewer:; Engel et al 2009a

Response: We have inserted the reference Engel et al., however, it is not shown in 2009a, but 2009b.

Page: 3339 1) Reviewer: Salinity is dimensionless.

Response: The dimensions from salinity has been removed and the sentence now

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reads: "Cultures of the diatom Skeletonema costatum (North Sea) and the coccol-ithophorid Emiliania huxleyi (strain PML B92/11, North Sea) were grown for 13 days at 15 °C in 0.2 μm filtered seawater (salinity 32) enriched with nutrients according to f/2 medium (Guillard, 1975)."

Page: 3340 1) Reviewer: when oxygen gradients were at steady state???

Response: Yes, all respiraiton measurements were done when O2-gradients were at steady state. This has been clarified in line 165: "...aggregate-water interface at steady-state".

Page: 3342 1) Reviewer: Is this OK? See: KHELIFA, A., AND P. S. HILL. 2006. Models for effective density and settling velocity of flocs. J. Hydraulic Res. 44: 390–401.

Response: Khelifa & Hill (2006) uses the same equation (Eq. 1) to find the excess density of several aggregates from different studies. They then try to use different models to find the excess density from aggregate sizes and other parameters and compare that to the results of Eq (1). If the model returns the same value as Eq. (1), the model is considered appropriate for use in larger numerical models. Thus, our choice of equation to find excess density seems good according to Khelifa and Hill (2006). Page: 3345 1) Reviewer: This seems awfully high for just liths. Is it possible that were some whole cells included? Why would the %OC be the same as in whole diatom cells?

Response: As discussed in the first paragraph of the discussion DOC might have been absorbed to the coccolith surfaces and both resulted in aggregation. This would also explain the high POC to dry weight ratio found for the aggregates composed of coc-coliths. We did not observe any whole coccolithophores within these aggregates, but it is of course possible the whole coccolithophores have been scavenged after the ag-gregates had formed from liths. This has been clarified in line 305 to 307: "Adsorption of DOC on the coccolith surfaces might also explain why the aggregates formed from coccoliths had POC to dry weight ratios similar to aggregates containing whole diatom

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cells."

2) Reviewer: Shouldn't this be in meters? Or at least clarify this with regard to the literature and to your Abstract.

Response: Please see response above (page: 3336, second reviewer comment).

2) Reviewer: opposite to Abstract????

Response: This has been corrected in the abstract, see response above (page: 3336, second reviewer comment).

Page: 3346 1) Reviewer: In line 12 on the previous page, you said they were not significantly different??

Response: The reviewer is correct, there is no significant difference in the carbon-specific respiration rates, and the differences in L are driven by the sinking speeds. The sentence has been erased.

3) Reviewer: Silver & Gowing (Prog. Oceanogr., 26: 75-113, 1991) say that there are TWO sources of aggregates. One is the type described here, but another is the "de novo accretion of particles by living organisms such as the production of mucus and the compaction of discrete particles into wastes (i.e., fecal pellets, pseudofeces)". I think it is important to remember that many aggregates are formed by organisms and not by random collisions.

Response: It is true that aggregates can form both physically and biologically. In this study we focused on the physically formed aggregates, hence, we did not want to include the biologically formed aggregates. This has been clarified in the MS (line 285-287): "When aggregates form by physical coagulation of particles, the aggregate formation is driven by shear and differential settling in the ocean, leading to collision of particles (Jackson, 1990;Kiørboe et al., 1990)."

Page: 3349 1) Reviewer: This has been questioned by Xue and Armstrong (2009.

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An improved "benchmark" method for estimating particle settling velocities from time-series sediment trap fluxes. Deep-Sea Res. II, doi:10.1016/j.dsr2.2008.11.033). The absence of such a relationship could be explained, for example, if glue holding particles together decomposes with depth, then the particles might disaggregate and have a slower average sinking rate.

Response: The study of Xue and Armstrong (2009) has been included in the discussion (line 364 to 367): "However, a recent study found no strong evidence for increasing sinking velocity with depth when using fluxes of mass and chemical tracers to determine the most likely average sinking velocity of particles at different depths (Xue and Armstrong, 2009)." It is correct that disaggregation of aggregates will lead to slower average sinking rate, when measuring a pool of aggregates. Still, the size-specific sinking velocities of the individual aggregates will be higher for aggregates ballasted by minerals and/or with little amounts of relatively buoyant organic matter. This has been clarified in the text (line 372- 375): "Thus, aggregates with high content of minerals and low amounts of relatively buoyant organic matter are likely to have higher size-specific sinking velocities. Such aggregates are likely to be found in the deep ocean where they had longer time for scavenging of ballast minerals and for microbial degradation compared to surface aggregates."

Interactive comment on Biogeosciences Discuss., 7, 3335, 2010.

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