

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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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)”

Biogeosciences Discussion

Reviewer 1: General comments The authors present a laboratory study of respiration rates and sinking velocities in phytoplankton-derived aggregates of varying size and species composition. I believe that, overall, the experiments were well-executed and give interesting results.

a) Reviewer: Upon reading the Introduction, I expected to see some measurements of respiration and sinking rates on non-ballasted aggregates for comparison, but perhaps these aggregates are more difficult to produce in a laboratory setting?

Response: The most common macro-aggregates observed in situ are formed from diatoms. In situ non-ballasted aggregates could be e.g. appendicularian houses, which mainly consists of organic material. However, the fractal structure are not comparable, and, hence, the small scale physics are not directly comparable between phytoplankton-derived macro-aggregates and appendicularian houses. Nano-flagellates, which are used for non-ballasted fecal pellets by Ploug et al. 2008b, are motile which prevent the physical aggregate formation. Further, macro-aggregates formed from flagellates are not observed in situ. Thus, strictly non-ballasted aggregates were not possible to include in this study, since the aggregate formation don't occurs for motile non-ballasted phytoplankton and direct comparison between biological and physically formed aggregates are not possible.

b) Reviewer: I also think that the authors could make a stronger case in the text for why their results are novel and exactly how they contribute to our knowledge of ballasting, remineralization and sinking rates.

Response: The strength in this MS is that several parameter (size, sinking speed, respiration, and composition of the aggregate) have been measured on individual ag-

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gregate. This has been pointed out the introduction (Line 100-106): "Aggregate size, sinking velocity, and respiration rates were measured in a vertical flow system, in which aggregate sinking velocity is balanced by an upward-directed flow velocity while respiration in the aggregate is measured using an oxygen microsensor at the aggregate-water interface. Hence, respiration was measured under similar hydrodynamic conditions as those occurring at the aggregate water interface during sedimentation. The composition of the same aggregate was analyzed after respiration measurements."

Specific comments 1. Reviewer: I suggest shortening the title slightly, to "Ballast minerals and the sinking carbon flux in the ocean: Carbon-specific respiration rates and sinking velocity of marine snow aggregates".

Response: The suggestion has been included in the MS.

2. Reviewer: Line 68: insert "that" after "suggested" and "is" after "carbonate".

Response: This has been corrected.

3. Reviewer: Line 76: the authors should provide a reference for these observations.

Response: Ploug et al. 2008b has been added to the sentence.

4. Reviewer: Line 100: delete "finally", "our" and "measured" and insert "collected" so that the sentence reads "We compiled previously collected data on : : :".

Response: This has been corrected.

5. Reviewer: Methods, line 107 and throughout the text, figures and figure captions: Please correct the spelling of "huxleyi". It appears as "huxley" in several places.

Response: This has been corrected throughout the MS.

6. Reviewer: Line 107, delete "during", replace with "for". This section is a bit confusing. It says that the cultures were grown in f/2, but then says (line 108) that the cultures were kept in 0.2 μm filtered sea-water. Which is correct? I expect that the f/2 was made

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with filtered sea water, but does this mean that they were transferred to just sea water later? Please clarify. Also, please remove the parts per thousand symbol (salinity is dimensionless) and add the irradiance under which the cultures were grown.

Response: The f/2 medium is made by enrichment of 0.2 μm filtered seawater and the phytoplankton stayed in this medium during the whole growth period. This has been clarified in the text (line 115-118): "Cultures of the diatom *Skeletonema costatum* and the coccolithophorid *Emiliana huxleyi* 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)." The light intensities of the light period has been added to the text (line: 119-120): "The cultures were kept under a light:dark cycle (12:12 h) with light intensities of 150 $\mu\text{mol photons m}^{-2} \text{ s}^{-1}$."

7. Reviewer: Line 117: how "dim" was the light? Please quantify.

Response: This has been clarified in the text (line: 127-128): "The roller tanks were rotated on a rolling table at 3 rotations per minute (rpm) at 15 °C in constant dim light, 30 $\mu\text{mol photons m}^{-2} \text{ s}^{-1}$."

8. Reviewer: Line 127: diameter of what? The cell? The chamber? Please clarify.

Response: This has been clarified (line: 137-138): "...remained suspended at a distance of one aggregate diameter above the net. . ."

9. Reviewer: Somewhere in the methods, please mention how many aggregates in total were tested. As I will mention later, Table 1 gives some numbers, but the figures have more data points than in the table. Please clarify.

Response: The table only includes aggregates on which several measurements were done. Hence, the aggregates referred to is shown in Fig. 4 and part of the aggregates in Fig. 3. We measured the sinking speed of additional aggregates and have included those in Fig. 3 since this method is less time consuming than the respiration measurements.

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10. Reviewer: Line 161: Please justify the choice of 1.2 as the respiratory quotient.

Response: This respiratory quotient is used in previous studies using similar methods (e.g. Grossart and Ploug 2001; Ploug and Grossart 2000) and returns realistic rates for carbon respiration. Further, by using the same respiratory quotient it is possible to make comparisons to the existing literature. This has been clarified in the text (line 172-173): "... , as also used in a previous study of O₂ respiration and POC degradation in diatom aggregates (Ploug and Grossart, 2000)."

11. Reviewer: Line 168: Check formatting here. Some italics and the number 12 appear to be misplaced.

Response: The italics and the number 12 has been erased. The title of subsection 2.6 has been changed to: "2.6 Aggregate dry weight and carbon content"

12. Reviewer: Line 169: please define "large number"

Response: The sentence has been changed to: "...determined by filtering 50 aggregates onto pre-weighted 25 mm..."

13. Reviewer: Also in the Methods section, probably in section 2.1, please indicate the strain and source of the phytoplankton used.

Response: This has been indicated in line 115-116: "Cultures of the diatom *Skeletonema costatum* (North Sea) and the coccolithophorid *Emiliana huxleyi* (strain PML B92/11, North Sea) were..."

14. Reviewer: Line 213: DW is already defined above and do not need re-defining here.

Response: This has been corrected.

15. Reviewer: Line 218: Do the authors mean to refer to Figure 2B (not 2A)?

Response: The figure reference has been changed to 2B-

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16. Reviewer: Line 261: Is POC-specific respiration rate the same as Carbon-specific respiration rate? Please clarify. I could not find an exact definition of Carbon-specific respiration rate anywhere in the text. Please clarify.

Response: POC-specific respiration rate is the same as carbon-specific respiration rate. To avoid confusion, we have change POC-specific to carbon-specific in the text. A definition of carbon-specific respiration rate has been added in line 263-265: "The carbon-specific respiration rate was calculated by dividing the carbon respiration rate with the total POC content of each aggregate."

17. Reviewer: Line 338: insert the word "only" after "size".

Response: This has been corrected.

18. Reviewer: Line 391: There is so much variability in the respiration rate measurements that I am not sure whether the authors should describe the rates as "uniform". It is difficult to tell.

Response: This has been changed and "uniform" is removed. The sentence now reads: "large influence on sinking velocities and the similar average carbon-specific respiration rates between the treatments indicate no protective mechanisms against remineralization".

19. Reviewer: Please check the references carefully. There are multiple mis-spellings, lack of capitalization, lack of italics, etc. Also, check the spelling of Jed Fuhrman's name (there are not two Ns).

Response: This has been corrected.

20. Reviewer: Table 1: please make the units consistent between the text and the table (e.g., mL in text, cells/mm³ in the table).

Response: The choice of presenting the cell concentration in /mL in the text is because this refers to the initial concentration, which is the amount of cells suspended in the

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water within the roller tank. In table 1 /mm³ has been used since this refers to the number of aggregated cells, hence, cells per volume aggregate.

21. Reviewer: Table 2: As mentioned earlier, the no. in sample column does not agree with the number of data points in the figures. (Do the figures include the triplicates of each aggregate?). Also, please define L in the Table caption.

Response: The table only includes aggregates on which several measurements were done. Hence, the aggregates referred to is shown in Fig. 4 and part of the aggregates in Fig. 3. We measured the sinking speed of additional aggregates and have included those in Fig. 3 since this method is less time consuming than the respiration measurements.

22. Reviewer: Figure captions: Please check the spelling of "huxleyi" in several places. There are typos in line 558, 559, 563, and 596.

Response: This has been corrected.

23. Reviewer: Figure 2: In figures 2B and 2C, I suspect that an outlying point is driving the regression (the large dry weight and large diameter aggregates). Please re-run excluding those points. My gut feeling is that the relationship in 2B will be weaker.

Response: We have run the regressions without the suggested data points, however, it is not so straight forward to place an aggregate as an outlier, since their fractal dimensions lead to not linear size-relationships. It is true that very few large E.h.-inc aggregates were formed during the study, so in that perspective the large aggregate in Fig. 2B is not a typical aggregate. The comparisons between the weakness of the relationships does not change when removing the large aggregate in Fig. 2B 2C. E.h.-inc still shows best relationship and mix-inc the weakest relationship. Hence, we would prefer not to manipulate with the data, but include all the measurements in both the regressions and the figures.

24. Reviewer: Figure 4: axis labels and ticks are missing in 4B.

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Response: This has been corrected.

25. Reviewer: I am not convinced that Figures 5 and 6 are necessary; the key points are already articulated in the text.

Response: We would still keep the figure in the MS since they clarify the points made in the discussion. There is a general tendency in the literature to use size of a wide range of aggregates and simply apply a function to estimate the in situ settling. Hence, figure 5 clearly shows that no straight forward relationship between sinking speed and aggregate size exist. Figure 6 shows that there seems to be a tendency between ballasting and degradation length scale (L), where dense ballast minerals, e.g. calcium carbonate and lithogenic material, may enhance the export flux. Hence, figure 6 is important since L relates the sinking speed and degradation into one parameter and shows clear tendencies depending on the ballasting of the aggregates. This method might provide a useful parameter for future modelling studies, and the figure offers some values for comparison.

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

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