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Comment

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

Anonymous Referee #1

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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. Upon

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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? 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.

Specific comments

1. 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”. 2. Line 68: insert “that” after “suggested” and “is” after “carbonate”. 3. Line 76: the authors should provide a reference for these observations. 4. Line 100: delete “finally”, “our” and “measured” and insert “collected” so that the sentence reads “We compiled previously collected data on . . .”. 5. Methods, line 107 and throughout the text, figures and figure captions: Please correct the spelling of “huxleyi”. It appears as “huxley” in several places. 6. 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 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. 7. Line 117: how “dim” was the light? Please quantify. 8. Line 127: diameter of what? The cell? The chamber? Please clarify. 9. 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. 10. Line 161: Please justify the choice of 1.2 as the respiratory quotient. 11. Line 168: Check formatting here. Some italics and the number 12 appear to be misplaced. 12. Line 169: please define “large number” 13. Also in the Methods section, probably in section 2.1, please indicate the strain and source of the phytoplankton used. 14. Line 213: DW is already defined above and do

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not need re-defining here. 15. Line 218: Do the authors mean to refer to Figure 2B (not 2A)? 16. 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. 17. Line 338: insert the word “only” after “size”. 18. 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. 19. 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). 20. Table 1: please make the units consistent between the text and the table (e.g., mL in text, cells/mm³ in the table. 21. 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. 22. Figure captions: Please check the spelling of “huxleyi” in several places. There are typos in line 558, 559, 563, and 596. 23. 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. 24. Figure 4: axis labels and ticks are missing in 4B. 25. I am not convinced that Figures 5 and 6 are necessary; the key points are already articulated in the text.

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