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Biogeosciences Discussions

Interactive comment on "Effect of CO_2 on the properties and sinking velocity of aggregates of the coccolithophore *Emiliania huxleyi*" *by* A. Biermann and A. Engel

Anonymous Referee #1

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General comments:

Biermann and Engel investigate possible effects of rising atmospheric CO2 on phytoplankton aggregate formation and the vertical downward flux of organic matter in the ocean. The authors measured aggregate formation and sinking velocities of E. huxleyi in rotating tanks. Coccolithophores were grown under 3 different CO2 concentrations that affected their ability to form calcite. This had consequences for the porosity and sinking velocities of aggregates: E. huxleyi grown under 'high' CO2 showed lowest PIC/POC, highest porosity, and lowest sinking velocities. The authors thus conclude that in the future ocean, slower sedimentation rates of phytoplankton aggregates will

reduce the vertical transport of organic matter from surface into the deep ocean.

Specific comments:

Measuring the effects of increasing atmospheric CO2 on the dynamics of phytoplankton and organic matter is an important step towards a better understanding of ocean acidification and climate change. The authors used a model organism, E. huxleyi, that is an important primary producer and calcifying organism in the ocean. E. huxleyi is commonly used in experiments to understand the effects of CO2 on growth and metabolism of phytoplankton as well as their ability to form fast sinking aggregates. That said, I am not sure that I understand the important finding that makes this paper unique/different from already published work. For example, Engel et al. (2009), here cited as Engel et al. (2009b), measured aggregate formation in roller tanks of calcifying and non-calcifying cells of E. huxleyi showing that the former had higher sinking velocities than the latter as a result of different ballast (calcite) concentrations. The authors of the present paper cite also other studies that measured calcite precipitation of E. huxleyi under varying CO2 concentrations showing that increased CO2 may result in reduced calcite (ballast) concentration (work by Riebesell and others). The present results of Biermann and Engel's paper support these earlier findings. They even chose a hypothesis (last paragraph, Intro) that has been tested by the above mentioned papers before. With this in mind, I have to ask myself: What is the 'punchy' new idea/finding of this ms that has not been tested before particularly regarding the Engel et al 2009b paper? If the authors can convince me that this manuscript has new ideas and unique results, I will suggest it for publication. I would also strongly suggest that the authors put the three CO2 treatments into perspective to predicted changes of atmospheric CO2; this is important for the overall significance of these experiments. In my understanding, the medium treatment represents present day values whereas the high treatment represents predicted levels for 2100. However, greatest differences in sinking velocities were found between the low (preindustrial?) and high CO2 treatment. Sinking velocities differed significantly between all three treatments (p.9826, l.1-2); however, the

authors start their discussion with the statement that the sinking velocities measured in MCT and HCT aggregates were similar (p.9830, l.3-4). With that, one of the most important result of this ms is weakened, i.e. future ocean aggregates will have lower sedimentation rates compared with present ocean ones. Please clarify this statement. I also found a couple of technical problems in the methods and results section that are listed below. Overall I suggest major revisions of the text and tables (see below).

Technical corrections:

1. Abstract: line 20: High bacterial abundance does not suggest enhanced degradation per se. This is a speculation as the authors did not measure enzymatic degradation, I would delete this part of the sentence. The abstract should in general be more precise: 'changes in PIC/POC influence. . .' how do they influence porosity and sinking velocity? Or: '. . . reduction of calcite content aggregates could affect . . .' how is the vertical transport affected?

2. Methods: p. 9821, l. 17-19: move this paragraph into results. p. 9822, l. 18-19: I strongly disagree with the authors that table 1 gives a more detailed description of the sampling campaign. There are no descriptions of the abbreviations used in the caption (this is also true for tables 2-4); what does '-' and 'x' actually mean? I suggest deleting the table. About table 2 and 4: could you combine the results of both to have the conditions before and after roller table incubations in table? To shorten the compiled table you could e.g. leave out cell abundance and give the numbers in the text.

p.9824, l. 9: what is GDA? Give the full name.

3. Results p. 9826, l. 5: this sentence should read: Sinking speed of LCT were the fastest with. . . . l. 11-17: move this paragraph into methods section. p.9827, l. 7-12: move to methods. p.9828, l. 2 '. . ., indicating that the amount . . . . p.9829, l. 11: '. . ., due to bacterial respiration in the tanks.' This sounds like you measured it, but you didn't; you can discuss it by saying: '. . . it may be a result of bacterial respiration because of . . . (following the statement from the next sentence).' This should all be

moved into the discussion anyway.

4. Discussion Last sentence of the first paragraph is misleading (see above). p.9830, l. 8:'. . ., and other factors.' This is not an appropriate expression. l. 10-17: Here you compare your sinking velocities with Engel et al.'s, both dealing with pure E.huxleyi aggregates. The conclusion in l. 15-16 is therefore not correct: your sinking velocities are not in the range of natural aggregates, but are similar to Engel et al.'s measured under the same conditions/with the same method. l. 20: delete 'of' p.9831-32, l. 8: This is what I meant at the beginning: your results support earlier studies, but you need to point out the new findings: what can we learn from your results about the effects of CO2 on carbon transport in the ocean that is new? p. 9832, l. 21-23: This is not interesting; we all know that aggregates formed in roller tanks from phytoplankton cultures not really represent natural aggregates. p. 9833, l. 6: you want to use 'coincided' instead of 'combined' l. 9: . . . lead to a more shallow export, . . .' doesn't sound good; instead use 'decreased export of organic matter' p. 9834, l. 6: '. . . large number of naked cells.

5. Conclusions This paragraph is not a conclusions paragraph; it is rather a short summary. For me, your conclusions start on p. 9833, l. 7: Applied to the future ocean . . .! Work the ideas that follow into a conclusion paragraph, and do not use '. . . are difficult to estimate.'

Interactive comment on Biogeosciences Discuss., 6, 9817, 2009.