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Comment

***Interactive comment on “Effect of CO₂ on the
properties and sinking velocity of aggregates of
the coccolithophore *Emiliana huxleyi*” by
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Response to Anonymous Referee #3 (Referee comments in quotation marks)

We thank the reviewer for providing several important suggestions for improving our manuscript.

Referee comment: “It is not clear to me where and how this study goes beyond the state-of-the-art. The effect of calcite on excess density and hence sinking speed was already documented by Engel et al. (2009).” Response: We explained the differences between the studies in our responses to referee #1 and #2. We will also add a section to the introductions paragraph, explaining how this study differs from previous experi-

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ments.

Referee comment: “The impact of mineral ballasting on aggregate properties including size and sinking velocity is also addressed in Passow and De La Rocha (2006). I am concerned about the general inappropriate consideration of published results from other groups (e.g. Passow et al., Ploug et al.)” Response: Passow and De La Rocha (2006) investigated organic aggregates that were exposed to different mineral concentrations (illite or calcium carbonate). At concentrations of minerals much higher than in our study (factor 1000), they observed a fragmentation of aggregates into thousands of tiny ones. Moreover, these authors did not investigate sinking velocities. A direct comparison of our findings with Passow and De La Rocha (2006) is therefore not intended. Ploug et al. (2008) measured sinking velocities and other aggregate properties. They conclude from their study that sinking velocities of aggregates depend on source and density rather than on size. We can confirm with our results that not size but excess density is the main control for sinking velocity (figure 2). We will add a new section to our discussions paragraph in which we compare our results with the findings of Ploug et al. (2008).

Referee comment: “The discussion of the future evolution of the biological pump is highly conjectural and needs to be carefully reworked. What would be the consequences to the C cycle of the suggested changes in export efficiency.” Response: What we infer from our results is that under high CO₂ conditions HCT aggregates sinking velocity is reduced, because of diminished ballast. Export depth of those aggregates will probably be decreased, since aggregates with low sinking velocities prevail longer in the upper water column and are more prone to grazing and decomposition. Export of POC depends on the degree of solubilisation of particulate organic matter into dissolved organic matter and the further remineralisation to CO₂. Higher POC concentrations will go along with higher oxygen consumption due to decomposition. Therefore transport efficiency of POC would be weakened. Büsseler et al. (2007, Science) showed that transport efficiencies can be variable, depending on the location

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and composition of particles. Thereby the proportion of biominerals is crucial. A less-efficient shallow export would presumably also lower the carbon sequestration. We will deepen the discussions paragraph with respect to the consequences for the carbon cycle.

Specific comments: Referee comment: “Langer et al. (2009) suggest that the variability in responses of PIC and POC production to increasing CO₂ in *E. huxleyi* ...might be due to genetic differences between strains.” Response: In this study we investigated the response of one strain to different CO₂ conditions. If the response of another strain is opposed, e.g. increased PIC/POC, our study still provides information on how porosity and hence sinking velocity would be affected. We will include this aspect in our discussion section.

Referee comment: “...specify on which scale pH values are reported, ...,dissociation constants..for CO₂SYS...stoichiometric solubility products was used for calcite. In general, the analytical section should be completed (calibration procedures etc.)” Response: We will include more information in the methods section according to the referee’s suggestions, e. g. pH values were measured on the NBS scale.

Referee comment: “I understand from the method section that the cultures were not acclimated to the different CO₂ levels.” Response: As we pointed out in the response to referee #2, we expect that the cells were acclimated to the different CO₂ conditions.

Referee comment: “Was the production of TEP measured during the experiment?” Response: Abundance of TEP was only inferred from microscopy. This indicated TEP production in all treatments. However, the data is insufficient for making a clear statement.

Referee comment: “...the authors use PIC to POC ratios published for natural assemblages to infer the validity of their experimental results obtained for monospecific cultures of *E. huxleyi*. This is quite confusing and might lead the reader to the wrong conclusion.” Response: We will clarify the text according to this suggestion to ensure

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there is no confusion.

Referee comment: "...viral infection: the last paragraph is pure speculation!" Response: Yes, but we like it! However, we will make it clear that this aspect of discussion is speculation.

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

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