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Comment

Interactive comment on “Not all calcite ballast is created equal: differing effects of foraminiferan and coccolith calcite on the formation and sinking of aggregates” by K. Schmidt et al.

K. Schmidt et al.

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Author response to the reviewers of "Not all calcite ballast is created equal..."

Response to reviewer #1 This reviewer raises some minor points, which we respond to as follows:

1) Their first point this reviewer makes is in fact a series of small objections to the foraminiferan tests used in the experiment and the size of the material collected. To answer the individual concerns brought up:

- concerning the size of the foraminiferan tests: the reviewer has missed the statement

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in the methods that the foraminiferans were hand-picked, using the tip of a paint brush, out of a sample of tests larger than the 250 μm mesh of the sieve. As such, the tests used were on the larger side of things and were easily visible in the tanks, to both the naked eye and the camera used for making the counts of the aggregates

- as we only used foraminiferan tests large enough to be easily visible, we did not exclude them from the analysis at the end. Because they were easy to see, we retrieved all of them at the end of the experiment. We will clarify this point in the paper.

- we did not select against particles < 1mm in diameter, we simply could not quantitatively collect the isolated smaller bits of barely visible fluff without vacuuming up half of the water in tank (and all the material in suspension in it). We have been clear about excluding the aggregates too small to be collected. We clearly state that our results only apply to the size range above 1 mm. This is fairly common practice in such experiments as this, because there is a physical limit to what can be feasibly collected.

- In answer to the question whether the aggregates formed during the experiment (average ESDs for the 4 types of aggregates ranged between 2 to 5 mm) fall within a natural size range: yes, they certainly do. The reviewer may be getting confused by the McCave 1975 paper, which is about particles in the DEEP sea, not the upper ocean where aggregates would be larger and more numerous. Particle size spectra from the upper ocean, measured back in the 80s and 90s using film cameras with a greater upper limit to the particle size measurable than the systems in use today, show most of the mass in aggregates in areas dominated by diatom blooms falls into the 0.1 to 3 mm ESD range (see, for example, Jackson et al 1997, DSR I v44, p 1739). Our aggregates are realistically sized aggregates. We will state this in the revised manuscript.

2) The second set of points raised by the reviewer concerns the used of calcite that had been cleaned of organic matter as opposed to something that was, in effect, covered with dead and rotting material. The reviewer mentions that coccoliths might be associated with polysaccharides and foraminiferans associated with "cellular material".

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Our answer to this is, yes, perhaps, but arguments can be made against this as well. Coccoliths shed from coccolithophores might initially possess an organic coating, but over time as the liths remain suspended in the water column, it is likely that they would lose it. But we are waving our arms on this point just as much as the reviewer is; we have yet to see any data on this issue one way or the other. Foraminiferan tests, however, are likely NOT to retain cellular material. Tests abandoned following gametogenesis at the end of a life cycle will not have organic matter left in them. It should have predominantly gone to the gametes released, leaving the bare test to sink to the seafloor.

We stand by our use of cleaned calcite in the experiment because this paper was about the interactions of phytoplankton and their exudates already sticky enough to form aggregates with (or without) calcite minerals, not the interaction of phytoplankton and their exudates with calcite minerals that came with their own sticky coating. That would be another entirely different study. The likely result of adding more goo would probably have been to drive more mass (of POM and minerals) into aggregates, but such an experiment wouldn't tell you about the effect of the mineral particles themselves. It would have been unpublishable to have done this experiment with such "dirty" calcite.

The reviewer's specific points-

1- too many references cited! 2- not enough references cited! Regarding these two points, as it is good to give credit where credit is due, we'll add the reviewers paper to the list but not kick out any of the others. At 43 references total, we have not gone crazy with the citations.

3- reference for the statement about shear: It's not a controversial statement, but we'll add the citation (Honjo et al., 2008).

4- The reviewer asks how we would "interpret the observations of Riley et al. (2012) of aggregate-protist complexes (their Fig 2) as part of the fast sinking marine snow particle pool seen in the North Atlantic. Here is our answer:

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Unfortunately, we are underwhelmed by the Riley et al (2012) paper, at least with respect to the "aggregate-protist" complexes. One problem, which isn't discussed in their paper, is that the "marine snow catcher" concentrates fast sinking material into a small volume over the course of the several hours it takes for the marine snow catcher to be hauled up to the surface where it then sits for several hours on the swaying deck of a ship while the faster sinking particles are separated gravimetrically from the slower sinking particles. This settling and concentrating of material under these circumstances must almost certainly result in the formation of additional aggregates and clumping together of particles that weren't previously associated. We understand how hard it is to collect and work with aggregates. We are in the same boat, as it were. But we wonder whether these "aggregate-protist" complexes are "real" or whether the protists got tangled up with aggregates inside the marine snow catcher.

The second problem concerning comparing the results of our experiment with the results from the Riley et al paper is that little information is given in the Riley et al (2012) paper about the small number (30) of "aggregate-protist" complexes they found during 9 deployments of the marine snow catcher. For example, what was the protist? Or were there different kinds of protists? The authors do not identify the protist(s). What the authors do is cite another paper that said were radiolarians observed in the area at the time of sampling. Radiolarians make silica (not calcite) skeletons that remain embedded in a gelatinous matrix (especially for the colonial forms). Radiolarians often have silica spikes that stick out beyond the radiolarian cell and would further encourage their entanglement with aggregates in the water column or in the marine snow catcher. Unfortunately, the Riley et al paper did not report any data for the opal or calcite content of the fast sinking particle pool that the "aggregate-protist" complexes fell into, or for the aggregate-protist complexes themselves, making it even more difficult for us to draw a comparison between their results and the results of our experiment.

Finally, Riley et al (2012) themselves conclude that: "Of the fast sinking particles identified across all 9 deployments 30 consisted of organic matter aggregated around a

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biomineralised protist (APC, Figure 2). This suggests that the presence of planktonic organisms can indeed act as nucleation points for individual particle aggregation and accelerate their sinking rate. However, since APC contributed such a small proportion of total sinking material collected, other 'catalysts' {for aggregation} must also be important."

Thus even if the "aggregate-protist" complexes are real (ignoring for the moment that diatoms are protists, too) and even if the protists in question are foraminiferans (a possibility not actually mentioned in the Riley et al (2012) paper), they are still not contributing much to POC flux... which is exactly what we have suggested in our manuscript.

Given all of this, we wouldn't want to say too much about this paper in our final manuscript. It would seem like an attack and it's not even clear if their work is relevant (since the protists were unlikely to have been foraminiferans). But we will add a sentence to note this work and that of Martin et al (2010) have both observed large (presumably) biomineralized protists (although not foraminiferans) contributing at least somewhat to POC flux through their incorporation into aggregates.

5- *Chateoceros gracilis* is larger than a coccolith but smaller than a planktonic foraminiferan test, like pretty much all marine diatom cells. Also, we'll add the accidentally left out space in Le Moigne's name.

6- There were not specific photos taken of aggregates used for the sinking speed measurements. There's only so much that can be done in a limited amount of time. The aggregates were selected randomly, so they looked like all the other aggregates of their type. We'll try to cut down on the use of squishy terms like "fluffy" and stick to terms that are tied to actual measurements.

7- See the more detailed response to comment 1, the first time the reviewer brought up the sampling the aggregates > 1mm. In short: the overwhelming majority of the aggregates were > 1mm. The stuff that was smaller was too hard to see and not numerous enough for quantitative collection, so we did not claim to quantitatively collect

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it. Attempting to do so would have added little "aggregate" material to the measurements of POC, etc. It would have probably added more "suspended" material to this sample, given the amount of water that would have been sampled along with these small, infrequent, hard to see aggregates. Undoubtedly, some of this smaller material was sampled during the collection of the > 1 mm aggregates.

8- We doubt that one or two seconds of exposure to milli-Q water during the rinsing away of the seawater from the filters would have dissolved much coccolith. On the other hand, failure to adequately rinse seawater (and its salts, including Ca) off the filters would have resulted in a grave overestimate of CaCO₃ as measured by the Ca content of the material collected on the filter.

9- The concentration of calcite used, 4.6 mg/L is higher than concentrations of calcium carbonate observed, for example, during coccolithophore blooms (which are on the order of 0.4 to 0.8 mg CaCO₃ per liter). But if an aggregate sinks 100 to 500 meters or so per day, in terms of the amount an aggregate might encounter during for the 48 hour duration of the experiment, the mass of calcite represents a relatively low number. We will clarify this in the paper.

Response to reviewer #2

The reviewer's main general comment is that the paper could have had more discussion of the implications of our findings. We had purposely toned the discussion down, not wanting to go too far in terms of extrapolating the results from the lab experiment in the rolling tanks (which allow material to remain in suspension and aggregate but have the drawback of being a closed system) to the real world. But in light of this being the reviewer's main comment, we will add a paragraph or two to the discussion to consider the effects on and of aggregate porosity, geographic variability in the importance of coccolithophores versus foraminiferans, etc.

Response to specific comments:

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1) The reviewer points out the Kwon et al paper, which we had overlooked. The Parekh et al paper was indeed about Fe, but it did include an updated "Strangelove" simulation that showed how effective the biological pump is as a whole in terms of lowering atmospheric CO₂ concentrations for a given total amount of CO₂ present in the ocean, atmosphere, and terrestrial biosphere and soils, which is what we cited it for (although in successive revisions of the introduction, the numbers dropped out). But the Kwon paper dealt with the carbonate pump as well, so it is more appropriate to cite in this paper. We will take this recommendation to refer to the Kwon paper instead of the Parekh paper.

2) It is no problem to replace the commas with parenthesis to add clarity to the sentence.

3) We will add in the information about the cylinder size and sinking distance.

4) What we refer to in the text are rolling tanks (i.e. tanks that roll) and roller tables (i.e. tables with rollers upon which the tanks roll). There was one instance where we mistakenly said "rolling table", which would be an intriguing piece of furniture. That mistake has been fixed.

5) insert full stop after mm: We couldn't see the problem here, so we haven't changed anything.

6) Did the reviewer mean page 14868, not page 14864? We've inserted the reference to Fig 6 there.

7) The reviewer has sharp eyes. It should be 210, not 21. 21 is a typo.

8) Particle porosity certainly plays a key controlling role over the sinking speed. We will mention this. Unfortunately, unless I am mistaken, the only way to calculate it is to back it out from the excess density-ESD-sinking speed relationship. It's a little bit circular.

9) We thank the reviewer for the tip about the Ziervogel and Forster paper, which we had missed. We have added their curve for *Skeletonema* containing aggregates to

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Fig. 8. Generally, it suggests aggregates that sank faster than ours for their ESD, although at some point, their curve intersects ours (i.e. at higher ESDs, our aggregates were a little bit faster than theirs). Their curve is actually most similar to the curve of Iversen and Ploug for aggregates that contained coccoliths and this may reflect the much more "mixed" composition of the Ziervogel and Forster aggregates, which were formed in large part from "fluff" at the seabed and as such almost certainly contained some lithogenic material (e.g. clay or silt). We will discuss this briefly in the manuscript.

10) The plot with all the lines on it seems too busy to make sense of in one step. This is why we presented it in two panels, one with our data alone and one including the curves of sinking speeds from the other studies. We'll seriously consider using only one panel when we revise the manuscript and will most likely adopt this change, although we'll just end up with a second "panel" anyway because the consolidated figure legend will be too large to embed within the panel.

11 & 12) The two technical corrections suggested by the reviewer have been fixed.

Interactive comment on Biogeosciences Discuss., 10, 14861, 2013.

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