



Interactive comment on “Temperature effects on carbon-specific respiration rate and sinking velocity of diatom aggregates – potential implications for deep ocean export processes” by M. H. Iversen and H. Ploug

Anonymous Referee #2

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Iversen and Ploug present an interesting data set from a carefully conducted experiment on the sinking, size distribution, and organic carbon oxidation of a set of aggregates at 15°C and 4°C. Unsurprisingly, the aggregates at the lower temperatures are oxidized more slowly, exactly the comparison that can be made between food on countertops in kitchens and food kept in refrigerators.

What seemed to me the most interesting thing about the work was the apparent increase in excess density of the aggregates at 15°C during the last 6 days of the experiment. I would have liked the manuscript to have considered this in more detail

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especially as it is a derived parameter and there is no clear corresponding increase in the POC/DW, in the solid hydrated density, nor in the DW/ESD of the aggregates in question. It would be worthwhile for the manuscript to delve into this more quantitatively and with an expanded discussion compared to what is there now, which is mostly freestanding, unrelated statements here and there in the results and discussion that are left to a careful reader to patch together for themselves.

It would also be good if this trend towards compactness in some of the aging aggregates could be at least briefly postulated on. Physically or chemically, what is happening? Do we have enough of an idea to even begin to make suggestions? Is it worth further study? Does it have a significant effect on carbon flux within the ocean? Is it the same thing that is happening with, for example, discarded larvacean houses as they sink? Is it the same effect that causes aggregates overloaded with inorganic particles (like clays or calcium carbonate) to fragment and lose porosity? The authors need not add a treatise, but some consideration of these things would be valuable, even if only to acknowledge any as yet unanswerable questions raised.

Major comments: 1- p374, line 26- f/2 contains 105 μM Si and something like 880 μM N. Just to be picky, if Si and N were added in a 1:1 ratio, this is technically not f/2 but some sort of modified f/2 medium. Or more concern is exactly which concentration was used for Si and N, 105 μM or 880 μM ? It should be noted in the manuscript because this is a huge difference. I'd like to throw in some food for thought for the authors: What was their intention with the 1:1 ratio? Given what we know about this species (although not necessarily this strain) would a 1:1 ratio drive the culture into Si or N limitation? Looking at the two S cosatum cultures tested in Brzezinski (1985) at less than continuous illumination would suggest that the cultures Iversen and Ploug grew into stationary phase were Si limited, which has different implications for cell quotas of C and N, for mortality rates, and presumably, then, for remineralization (e.g., see De La Rocha et al., 2010 MEPS 412, 57-68). It might be worth including some brief words on this in the paper at some point.

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2- p383, line 23-24- "Our measurements of respiration represent community respiration on aggregates, i.e. it includes respiration by diatoms and associated bacteria and protozoa." Are there really protozoa in GF/F filtered seawater?

BGD

10, C203–C207, 2013

3- Discussion- To rephrase what was said in the general overview statement above: The observation of an increase in excess density that is related to (and calculated from) an increase in the sinking velocity vs ESD relationship in some aggregates on day 17 at 15°C and in all aggregates measured on day 23 at 15°C is quite interesting. As the authors note, this is not due to a change in the POC/DW of this material or to a change in the solid hydration density of this material. Thus it can only be due to a change in the porosity, or as they put it, "compactness" of the aggregates. However, I am confused as to how this could happen with no clear corresponding change in the DW/ESD of the aggregates. Is DW/ESD not sensitive enough or not determined with enough accurately (ESD is an approximation after all)? Or is there some circularity in the calculation of excess density from sinking velocity and ESD? It would be good to have a brief discussion of this in the paper.

Minor comments: 1- p374, line 24- The species name *Skeletonema costatum* should be either italicized or underlined. And if I'm not mistaken, the name has been changed to *Skeletonema mainoi*.

2- section 2.8: Just a comment about the centrifugation to determine the solid hydrated density of the "aggregated particles": what does this mean? The term is ambiguous. Does "aggregated particles" mean "aggregates" or "particles making up the aggregates". There is a significant difference between those two things. Did the aggregates remain whole during the centrifugation? Does it matter whether or not the aggregates remained unbroken during the centrifugation? If they did break, did material of different densities (e.g. organic matter versus silica) end up in different density layers (presumably not from the text). What is the significance of the two endmember densities chosen (1.05 and 1.43 g/cc)? Also, just curious, one only 1 ml of each 2 ml particle-containing density layer was removed for weighing? Were the particles evenly distributed through-

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out the layer? Some clarification of the above issues is needed in this section. Also "constitutes" in the header should be "constituents".

3- section 2.9 header: Is it exopolymer or exopolymeric? Traditionally it has been the former (e.g. Passow 2002 Prog Oceanogr 55, 287-333).

4- p379, line 21- It should be "onto each filter".

5-p380, line 10-11 "...showed an increase in the abundance.....and a decrease in the abundance...."

6-p380, line 14-16 "Lowering the temperature to 4°C for half of the roller tanks after 3 days did not have any effect on the aggregate size distribution (Fig. 1b)". What the authors are trying to say is correct, but how they say it is not accurate. Compared to leaving the temperature at 15°C, the lowering of the temperature DID have an effect on aggregate size distribution in that the aggregates at the lower temperature did not disaggregate between days 9 and 12 as they seemingly would have done if the temperature had remained unchanged. It would thus be better to simply remove the sentence quoted above from section 3.1.

6- p380, line 24- It sounds better as "The amount of TEP within...." The "content of TEP" implies what TEP contains, not the quantity of it.

7- Are the figures referenced in order? Fig 4 seems to be invoked before Figs 2 and 3, or have I just missed something?

8- The symbols used for the same day and treatment in the different figures is different (e.g. sometimes day 23 at 15°C is a white square, and sometimes it is a black square). This needs to be made the same from figure to figure (for the relevant figures) to facilitate comparison.

9- p378, line 5- "...where CD is the dimensionless drag forced defined in Eq. (1).." That should say Eq. (2).

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