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Discussions

Interactive comment on "Rapid formation of large aggregates during the spring bloom of Kerguelen Island: observations and model comparisons" by M.-P. Jouandet et al.

Anonymous Referee #2
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This manuscript presents an interesting study of particle size distribution through the water column measured using the an UVP. These results were compared to the results of a one-dimensional modelling of aggregate coagulation. I find that the study is very interesting and presented good in the manuscript. It does give some confirmations to aggregation and export processes and shows that in some situations coagulation theory can be a powerful tool to understand vertical export flux. I find that some points could be discussed more detailed in the manuscript. Especially the point that the authors found good comparisons between observations and modelling, despite that the used a stickiness of one and ignored all degradation and grazing. To some extent the grazing issue is addressed in the paper. That the model works though it clearly ignores important processes makes you wonder if it works for the wrong reasons? Would it only work for this system at this time, or can we generally consider coagulation as the main driver for export?

General comments: The dominant diatom was Fragilariopsis kerguelensis and the authors
chose to base their model on a model-diatom matching the size of F. kerguelensis. As far as I know, nobody have ever observed marine snow formation from F. kerguelensis and it is generally believed that this diatom species will either settle as individual cells or in chains. It is of course possible that scavenging of F. kerguelensis occur by already formed marine snow. I find that a part in the discussion about the good fit between model and observations despite the assumption of aggregation by a seemingly non-aggregating diatom (even with stickiness of 1) and that the model ignore degradation and grazing. Does this mean that modelling a simplified system can still provide good estimates of export?

Detailed comments:
P. 4952, L. 20-23: Please explain in one sentence what you mean with indirect export. Pellets are still directly part of the exported material, but just due to biological aggregation and not physical aggregation as is the case for marine snow.

We have changed the text in the 4th paragraph to explicitly state what we consider to be direct export.
"(Note that we consider direct export to be the flux of phytoplankton cells, either alone or in aggregates)"

P. 4954, L. 15-17: Maybe change "pixel surface area" to "pixel area", "surface area"
could be confusing for the reader.

"pixel surface area" has been changed to "pixel area",

P. 4956, L. 8: Please change "Fragiliaropsis kerguelensis" to "Fragilariopsis kerguelensis".

This has been corrected.

P. 4956, L. 8-10: Fragilariopsis kerguelensis is not a typical marine snow forming diatoms, generally those seem to sink as individual diatoms or in chains. They might be scavenged by already formed marine snow, but it seems a bit unlikely that F. kerguelensis will form marine snow on their own, especially with a stickiness of 1. Do you know of any literature which can support your assumption of marine snow formation by F. kerguelensis?
Maybe you can provide a bit rationale to the model diatom, it is interesting that the model fits so well to the observations considering that the dominant diatom dont seem to aggregate and when all degradation and grazing mechanisms are excluded. I miss a discussion of this in the manuscript, only the influence of zooplankton grazing is briefly discussed.

Particle aggregation experiments were conducted during the KErguelen Ocean and Plateau compared Study (KEOPS2). Water sampled by Niskin bottle at Station A3 was incubated in roller tanks to form marine snow by physical aggregation via differential settling (Laurenceau et al., submitted). The aggregates formed were a mixing of different species: Chaetoceros subgenus Hyalochaete spp., Fragilariopsis spp. including the species kerguelensis and rhombica, small centrics represented mainly by Thalassiosira spp., Pseudo-nitzschia spp and Eucampia antarctica. While this study didn't find that only one species was aggregating, it did demonstrate that F.Kerguelensis does aggregate.

P. 4957, L. 8-10: The fractal dimension value of 2 is not really in the middle of the range between 1.3 and 2.3. I assume you have tried different values for the fractal dimension until the model results matched the nVd distribution obtained with the UVP. Why not write that and say that it is in the range of the reported values for fractal dimensions?

This has been modified in the section 2.3:

"The value of d_a is calculated from dc using the fractal relationship and a fractal dimension of 2 (Appendix A). Note that reported values of the fractal dimension vary widely, from 1.3–2.3 (Burd and Jackson, 2009). The value of 2 used here is in this range and yields peaks in the nVd distributions similar to those determined from UVP measurements, unlike values of 2.1 and 1.9 (not shown). "

And this is discussed in the section '4.2 Limitation of the model

"One important parameter that was varied during model development to adjust the results was the fractal dimension. Decreasing it decreased the diameter of the peak value of nVd. The value that was chosen, D_{fr}=2, was similar to some of the estimates of fractal dimension noted above and did provide the correct nVd distribution when coagulation occurred. "

P. 4958, L. 10: Chaetoceros is known to aggregate at high rates and, therefore, often chosen for laboratory work on aggregates. Have you tried basing your model on that species?

Previous studies have mainly focused on the formation of the diatoms Thalassiosira and Skeletonema (Hamm et al., 2002; Grossart et al., 2006; Ploug et al., 2008; Gardes et al., 2011) but unfortunately not on Fragilaropsis and Chaetoceros, which were the dominant species in our studies. As noted above, aggregation experiments made with Fragilaropsis during the cruise showed that this species does form phyto-aggregates.
Our model is a simplified view of the ecosystem that does not explicitly describe multispecies aggregation. The model was also run with different initial cell sizes but the results did not affect our interpretations. Aggregation in both the model and the observations occurred over similar very short periods.

P. 4964, L. 17-20: There is no journal, volume or page number for the Laurenceau et al. 2014 publication. If they worked with gel traps from the area and time of this study, did they observe any F. kerguelensis in the aggregates?

The reference has been corrected. Unfortunatley, microscopic analysis conducted from the gel trap could not provide the species of diatoms included into phyto aggregate (Laurenceau et al., 2014).

P. 4966, L. 25 to P. 4967, L. 3: During your high temporal measurements of particle abundance and volume (A3-2/1 to A3/2-7) you observed large changes in the vertical distribution of particles between day and night and during a few days. In figure 11, you compare single vertical profiles of particle volume from different months. Except for the January profile, the differences observed between October, November, and February are not much larger than the differences in total particle volume through the upper water column between the 15th and 17th of November.
This indicates that these results do not really provide seasonal insights, but rather show the important of continuing measurements over time at much higher temporal resolution than once a month?

A paragraph dealing with this issue was added

"Combining KEOPS cruises to describe temporal scales of particle production and export (transient versus seasonal) is useful as a first step, but our limited observations highlight the need for high frequency data collection over long periods."

P. 4967 to P. 4969 "Possible impact of artificial iron fertilization on coagulation" I find the list of findings from the different iron fertilization experiments a bit boring as it is now, just ending with three lines stating the coagulation is important. You already indicate some of the issues of having sediment traps below the euphotic zone as the only mean of flux estimates. Can you maybe go a bit further into the importance from your observations and modelling study about the depth of traps and how you can miss the flux and flux attenuation when choosing the wrong depths?

The discussion has been improved. We focus the comparison of our results to those from other iron fertilization experiments to understand the relative roles of coagulation and zooplankton grazing on particle export during different parts of the bloom cycle. The section "4.2.2 Potential impact of coagulation after iron fertilization (L 458-502)" has been rewritten. The review shows that phyto-aggregation was the mechanism responsible for large particles formation in two experiments among the whole iron fertilisation experiments. Both were at bloom onset and with low stirring.

Here we modelled diatom coagulation in the MLD that corroborated observations that show how fast aggregation can change particle sizes and export. While we were able to show the rapidity of aggregation, we were unable to follow the export because of cruise constraints. We acknowledge that following the fate of the aggregates requires a more elaborate model that would include a better turbulence description capable of improved predictions of phytoplankton distributions as well the bacterial and zooplankton distributions and their effects. Such a model requires more information than was available

References :

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