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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.

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

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

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 litterature 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.

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?

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?

P. 4964, L. 17-20: The 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?

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?

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?

Interactive comment on Biogeosciences Discuss., 11, 4949, 2014.