

## 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 #1

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Jouandet et al present an interesting study of the evolution of particle and biogeochemical properties over the course of a 1 month study during the Spring bloom near Kerguelen Island. In general, the study is well presented and provides a few new insights into these processes. In particular, the use of models and observational data side by side should enable the critical evaluation of each, yielding new insights and understandings.

However, the lessons derived from the comparisons done here are somewhat weak in that they emphasize similarities but do not clearly discuss how these similarities lead to improved understanding of the system. Differences between the model and observations also were not fully described. These differences could be used to strengthen the interpretation of both the observational data and also the realism of the model.

The issues I raise below have the potential to affect the interpretations of the study results and models, and therefore, I recommend that the details as well as the implications of these issues should be thoroughly addressed.

The study assumes a one dimensional (depth) view of the temporal evolution of particles and plankton. How might advective considerations influence the results, observations, and interpretations? Can advective processes be ruled out as one of the possibilities for explaining the temporal changes?

The discussion has been re written. The similarities between the model and the observation are fully described in the section "**4.1 Role of coagulation in the rapid changes observed**" (L341-350). The differences are addressed in the section "**4.2 Limitations of the model**" (L372-397). The importance of advection process as well as zooplankton grazing is now discussed in the section "**4.2 Limitation of the model**". The following paragraph was added:

"Other processes are known to affect particle concentrations and fluxes, most notably physical process such as advection and biological processes such as zooplankton grazing and fecal pellet production (e.g., Lampitt et al., 1993; Stemmann et al., 2000; Turner et al., 2002). The importance of advection could be inferred from time series measurements of LADCP. The results indicated a current below  $0.1 \text{ m s}^{-1}$ , with negligible changes over the survey in the 0-200 m depth layer (Park, pers.com.). The abundance and volume of zooplankton larger than 0.7 mm, as well as fecal sticks/pellets and aggregates, were estimated from the identification of organism in the vignettes recorded by the UVP using the Zooprocess imaging software (see Picheral et al., 2010). The volume of copepods did not increase through the early bloom survey, suggesting that they were not responsible for the observed rapid increase in particles. Ingestion rates were also estimated from zooplankton biomass using the relationship detailed in Carlotti et al. (2008) using the biomass results integrated over the 0–250 m layer. The ingestion rate was  $1.36 \text{ mg C d}^{-1}$  during the early bloom cast and lower than during the KEOPS1 summer cruise. In addition, fecal pellet production should have a diurnal signal (Carlotti et al., 2014), which was not observed in the  $V_T$  profiles. Lastly, fast sinking fecal pellets are much smaller than the aggregates observed here. For example, fecal pellets falling at  $100 \text{ md}^{-1}$  are typically  $2\text{--}5 \times 10^6 \text{ } \mu\text{m}^3$ , equivalent to  $d = 200 \text{ } \mu\text{m}$  (Small et al., 1979), compared to the mm sized aggregates dominating at A3. Thus, changes in zooplankton populations can be ruled out to explain the observed  $V_T$  increase at this time, although not through the entire season. Modelling the dynamics of the entire season would require integrating zooplankton activity. "

Table 3 shows estimates of POC fluxes that vary by almost 4 orders of magnitude. Are these estimates realistic? Are there no other flux values (trap-based) available from the other KEOPS studies?

POC flux could be derived from the gel trap analysis by Ebersbach et al. (2008) but using different algorithms from the ones used by Laurenceau et al (2014). Therefore we didn't report the POC flux derived from the gel during KEOPS1. PPS3 Trap was deployed during KEOPS but was unable to measure the carbon export flux for the event scale that we observed.

It would be useful to combine the model and observational time series countour plots into a single side-by-side figure. This would make it easier to compare and contrast.

We changed Figs 6 and 10 to make the comparison easier between the observations and model results by using common scales and plotting styles. We believe that this will facilitate comparisons.

One significant difference between the model and observations appears to be the depth-time series of particle volume.

In the observations it appears that the particle maximum develops initially around 150 m depth, followed by increases in particle volume at more shallow depths between 50-120m.

We emphasize that there are deficiencies in the results from the phytoplankton growth model before the aggregation event and that this influences the depth distribution of aggregate formation.

Little flux is expected at deeper depths below this particle maximum.

Why? Our point is that there has not been enough time for the flux to reach the sediment trap, not that there will be no flux. The fact that the particle maximum is so deep argues that the particles are not neutrally buoyant. In fact, we show evidence of the particles falling out of the mixed layer in Fig. 8., being exported to the region between 150 m and 200 m. This is clear evidence that flux does occur.

However, the model predicts that the particle volume maximum develops around 30m and get progressively deeper as they flux out of the system. These conflicting results are not mentioned in the text and seem to suggest that there may be some processes dominating that were not accounted for in the model.

This discrepancy seems to limit the utility of the model in this case. What can we learn from the model about the processes that actually happened in the water column during the time series?

This point is discussed in the first paragraph of the section "4.2 Limitation of the model":

"There are, not unexpectedly, differences between model results and observations. To start, fluorescence profiles are relatively constant through the surface mixed layer in the observations, but have a pronounced shallow subsurface chlorophyll maximum in the model because of the higher light levels near the surface. Increased mixing in the model could smooth the chlorophyll profiles, as well as the distribution of particle volume. Simulations made using a much larger mixing coefficient ( $1000 \text{ m}^2 \text{ d}^{-1}$ ) yield a smaller difference in chlorophyll between the surface and 150 m, but there is still a difference of  $0.8 \mu\text{g Chl L}^{-1}$  over the depth range (results not shown). The vertical mixing rate estimated for the iron fertilization experiment EIFEX,  $29 \text{ m}^2 \text{ d}^{-1}$ , was actually smaller than that used in these simulations,  $100 \text{ m}^2 \text{ d}^{-1}$  (Smetacek et al., 2012). A previous model of phytoplankton growth in the Kerguelen region discussed large scale horizontal patterns but unfortunately did not display vertical distribution (Mongin et al., 2008). Whatever the reason for the relatively uniform fluorescence profile, it is not simply a faster diffusive mixing rate. Those differences illustrate the difficulty of building a realistic phytoplankton growth model in the region to drive the coagulation model. The shallower phytoplankton distribution does affect the distribution of aggregates as well. "

Another way to think about this discrepancy is perhaps the aggregates that are being produced are mostly neutrally buoyant and don't contribute to the sinking flux of material at depth.

If this was the case, the parameterization of flux from the PSD used in Table 3 might not be applicable for this collection of particles. This would also have implications for the realism of the model that prescribes particle densities and sinking velocities from aggregation theory.

In the section 3.1.5, we discussed the relationship between the fluorescence and size of phytoplankton.

Conclusion of this section is that

"In the second layer, immediately below the surface mixed layer, fluorescence and  $V_T$  increased together, with a positive correlation coefficient (0.68) and a slope of  $0.036 \mu\text{g Chl mm}^{-3}$  (Fig. 8). This is consistent with no phytoplankton growth in this depth layer, but with phytoplankton and aggregates arriving together from above, presumably in aggregates. There was no correlation between fluorescence and  $V_T$  below 200 m during this period."

The authors devote significant space to comparing results with other iron fertilization experiments. Lots of facts are covered, but the paper only briefly discusses the implications, significance, and generalizability of the findings. Similarly, after a manuscript that thoroughly describes the details of the observations and model outcomes, I ended the reading not really sure of the definitive take-away lessons from the paper. The discussion does a rather weak job in emphasizing the important lessons, and focuses more on comparisons between various data sets without a clear purpose for doing so.

We focus now 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 conclusion has also been improved to highlight the lessons of our study:

"It is clear that particle flux in the ocean is the result of many interacting processes, and none of these has been identified dominant across systems. In the present study, we were able to observe rapid aggregate formation and sedimentation of high concentrations of diatoms from the euphotic zone. Our observations are consistent with results from a one-dimensional model that includes only phytoplankton growth and coagulation. Our results demonstrate the utility of coagulation theory in understanding vertical flux and its importance to initiate the formation of large particles in the mixed layer and their subsequent transfer to depth during a bloom. Nevertheless, efforts are still required to measure large aggregates distribution at a high frequency to fill the temporal window between these short time events taking place during the early bloom and the possibly slower dynamics of summer. In addition, more effort is required to understand better vertical variations at a fine scale for all times and particularly to estimate the transformative roles of microbes and zooplankton in decreasing the total particle volume exported from the euphotic zone."

# 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 4<sup>th</sup> 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 don't 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  $d_c$  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. Unfortunately, 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 importance 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."

This issue is also addressed through the manuscript : in the abstract (L52-54), in the discussion (L434-345) and in the conclusion (L510-513) sections.

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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