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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 #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?

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?

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

One significant difference between the model and observations appears to be the depth-timeseries 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. Little flux is expected at deeper depths below this particle maximum. 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? Another way to think about this discrepancy is perhaps the aggregates that are being produced are mostly neutrally buoyant and don't contributing 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.

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.

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