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Biogeosciences Discussions

Interactive comment on "Ecosystem function and particle flux dynamics across the Mackenzie Shelf (Beaufort Sea, Arctic Ocean): an integrative analysis of spatial variability and biophysical forcings" *by* A. Forest et al.

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Referee: In this manuscript, the authors report on results estimates of organic matter fluxes across the Mackenzie Shelf area of the Arctic Ocean. One of the many nice things about this study is the combination of the instruments used and the lengthy analysis performed. However, the latter is also to my mind a weakness of the manuscript. Much of the data analysis makes extensive use of regressions and is exploratory in nature. This makes the interpretation of the analysis difficult - how firm is a particular result? So I find myself with mixed feelings; I generally like the manuscript and find that

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the extensive sets of measurements and interdisciplinary nature of the material very interesting. However, the detail of the data analysis leaves me uneasy.

Reply: We would like first to thank Referee #1 for his/her comments on our complex work and for the insightful remarks. We understand that the extensive series of analyses might leave a feeling that the uncertainties associated with the data were not well tracked. Indeed, we want to underscore that the error in our study has been assessed wherever it was possible and presented in the initial manuscript wherever it was appropriate (e.g. confidence intervals in all regression lines and associated parameters). In the revised manuscript, we have further improved the detail of information on both the accuracy and precision of all results. You'll find below a point-by-point description of the changes made to the initial manuscript. In this respect, the comments made by the referee definitely helped us to provide a strengthened and more rigorous manuscript.

Referee: In more detail, there is little mention of uncertainties in the values used. This is important when looking at trends, and in exploratory data analysis in general. For example, on line 10 the authors state that they use average particle abundances when more than one profile was available. This would naturally lead to an estimate of the uncertainty of the data in such cases, but this seems not have been used.

Reply: All figures, tables and result sections were reexamined to provide an enhanced level of information on the uncertainties. In particular, we have removed the old Appendix A (as suggested by Referee #2) and we have inserted a new Appendix (new Appendix A) that describes the rationale and uncertainties related to the relationship between particle abundance as measured by the UVP and sediment trap fluxes. This new Appendix includes the results of a random permutation exercise (multiple resampling of the dataset) in order to show the range of the most probable parameters of the exponential relationship linking UVP measurements and trap fluxes (new Figure A1). This enabled us to provide also the error associated with the parameters A and b resulting from the optimization procedure that yielded initially only one solution made of two parameters (Table 2). In addition, the error associated with the vertical flux across size-classes (Figure 10) is now plotted on each histogram bar to show the variability of fluxes across the region. Also, confidence intervals of regression parameters and a legend describing the confidence intervals of observation bounds and from the functional fit for every regression lines were added (e.g. Figure 8 and figures in Appendixes A-D). Finally, many additions in the text were inserted in order to document the degree of variability associated with the average values used in the variation partitioning analysis (see e.g. lines 416-417, Figure A2 and Figure A3).

Referee: There are many regressions and fits to data used in the manuscript, yet none of them include uncertainties on the fitted parameters. Calculating these parameter uncertainties is going to be critical, especially if one intends to use the resulting relationship to derive additional quantities (as the authors do). If uncertainties are calculated (and almost all modern fitting programs will calculate 95% confidence limits on the parameter values) then a propagation of errors allows one to calculate the uncertainties on any derived quantities.

Reply: As mentioned above, the error associated with the parameters A and b from the optimization procedure were added to Table 2; and the confidence bounds from all parameters obtained through data regression (Fig. 8 and Appendixes A-D) were added to the resulting parameters. Initially, the error associated with those parameters was not included in the figures for clarity purpose, as they add complexity to the various equations. They are now included.

Referee: In addition, R2 parameters are used a great deal as measures of goodness-of-fit. This is ok for linear relationships (though there are better measures) but it is known that R2 is a biased measure of the goodness-of-fit for non-linear relationships (e.g. in Appendix A). Given the exploratory mature of some of the analysis conducted here, it may be better to use an information theory measure such as the Akaike measure.

Reply: We would like to stress here that the AIC measure is a way to select a model from a given set of different models. The statistical approach we have chosen here (Borcard et al., 2011, Springer) does not use the AIC criterion, but the R2-adjusted and a forward selection procedure (e.g. Table 3) in order to retain a given set of models for a subsequent canonical ordination and variation partitioning. This approach is suitable because redundancy analyses, which are at the core of the partitioning, are based on the general linear theory and were performed with datasets that were adequate for such analysis. In a sense, our methodology provides a similar framework than the AIC, as the goal is also to select the best models with parsimony. Hence, we do not see the need to add further complexity to the different results by providing additional measures of fit. As for the discrete goodness-of-fit presented in Appendixes B-D, we argue similarly as the datasets were linearized before analysis and as the R2 of those fits were not used directly in further computations, but simply used as an indication of the robustness of the fits.

Referee: Some of the data analysis techniques used in the manuscript requires some additional explanation. For example, redundancy analysis. For example, what does "(scaling= 3)" mean (after equation 2)?

Reply: Agreed. Scaling in redundancy analyses refers to the way the ordination of scores is plotted in the triplot that includes at the same time information on sites, species and independent variables (eigenvectors). The scaling 1 and 2 (respectively for sites and species) means that either the sites (1) or the species (2) are scaled by the eigenvalues, while the other set of scores is left unscaled. The scaling = 3 is a thus a very convenient approach to show the overall variability for both sites and species, as it means that both species and sites are scaled symmetrically by the square root of eigenvalues. Such information is documented in the FAQ of the R Vegan package used for statistical analyses here. This FAQ is now included in the list of reference and serves also as an additional resource on our statistical approach in addition of the other references.

Referee: On Page 10903, the authors refer to Figure 11, and say that the settling speeds are plotted as functions of A and b. But values of A and b do not appear on the figure. Also the caption to the figure refers to “cinematic viscosity” which should be “kinematic viscosity”.

Reply: The A and b values are presented in Table 2 as mentioned in the legend of Figure 11. We have improved the wording to make it clearer than before. Adding all the parameters directly in Figure 11 would make the figure less clear. The caption has been also corrected.

Referee: The first sentence of the conclusions makes no sense: “linear trend” of what?

Reply: As mentioned previously, the redundancy analyses are based on the general linear model theory. This means that the significant linear trend identified between the vertical flux size-classes (used as species) and sampling date (Figure 14a) has been incorporated as a source of variability in the variation partitioning analysis (Figure 16). Hence, it is appropriate to mention a “linear temporal trend over July-August” in the first sentence of the conclusion.

Referee: Many of the figures reporting the results of regression fits need to be looked at again. For example, in Figure 9, what are the additional lines? I’m assuming that the innermost grey lines are 95% confidence limits, but this needs to be stated. Similarly with Figure A2, though in that case I find the innermost grey lines hard to believe if they are indeed 95% confidence limits: if they are, then I suspect that the regression is being driven by the extremes of the data values, particularly given the spread in the middle, which can be up to two-orders of magnitude. So all in all, I like the aims of the paper, and find the results intriguing. However, I would like to see more rigor and detail in the exposition of the data analysis.

Reply: Yes, we have looked at all our regression fits again and documented/enhanced the level of information of those figures. In particular, we have inserted a legend to document what the innermost and outermost lines mean. In brief, the outermost lines are the 95% confidence bounds for new observations (“normal” prediction bounds on

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response values); while the innermost lines are the 95% confidence interval for a new function, which corresponds to the confidence on the fit itself.

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