

Interactive comment on “Downward particle flux and carbon export in the Beaufort Sea, Arctic Ocean; the Malina experiment” by J.-C. Miquel et al.

Anonymous Referee #1

Received and published: 16 February 2015

General Comments

This manuscript shows results on POC flux measured by the drifting sediment-trap moorings and the zooplankton contribution to the POC flux in the Beaufort Sea during late summer 2009. I think that the observations using drifting sediment trap is rare in the Arctic Ocean. The data quality in this paper is good, and it will be helpful for further research on the lower trophic ecosystems and biogeochemical cycles in the Canadian Beaufort Sea. Careful quantitative and qualitative observation on many fecal pellets in the trap samples are well evaluated. Therefore I think that this manuscript is valuable to publish in Biogeosciences. On the other hand, I feel that some sentences should be considered to revise. According to the introduction, the goals in this paper are to

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present the observed POC flux, the composition of POC flux, and the most sensitive forcing factor on the variation of POC flux. The description of observed results are well written. However, the further discussion may be required on the major forcing factors to explain the variation of POC flux. I could understand on the influences of zooplankton community structures in this study. However, I could not find whether there are other important forcing factors or not. I have listed the specific comments. Because I am not a native-English speaker, I apologize if my comments were difficult to read. I hope some comments are useful to improve the manuscript.

Specific comments

p.1249, line 9 “generally low fluxes” - What kind of low fluxes? Total mass, clay, organic matter, or POC? The contrast of low and high fluxes should be represented by the comparison of flux values from the cited references.

p.1251, line 13 “Fig. 2b” - If possible, the explanation of Fig. 2a should be done at first. Of Fig. 2b should be Fig. 2a by re-organization of two panels in Fig. 2.

p.1251, line 21 “The pH was checked” - Could authors describe the pH value?

p.1252, line 5 “Foraminifera and empty mollusc shells . . . passive sinking flux . . .” - I assume that the gelatinous “house” of Appendicularia was also treated as passive sinking flux if there were abundant Appendicularia in the samples.

p.1253, line 6 “Faecal pellets were counted. . .” - The number of counted pellets may be required.

p.1253, lines 12-26 subsection 2.3. - It can be read that the authors analyzed POC of the long-term trap samples using the method by Forest et al. (2013). However, POC flux of the same (?) trap samples were already published in Forest et al (2013). I have confused whether the authors newly analyzed POC of the trap samples or the authors have applied the POC flux data analyzed by Forest et al (2013). Some explanation will be required.

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p.1255, lines 4-6 “As explained above, fluxes were. . . 70m above ground (Table 1).” - This sentences can be combined with the description in the subsection of 3.1. In order to avoid the repetitive impression in the manuscript, reorganization of some sentences should be concerned.

p.1255, line 17 “(15-20 mg m⁻² d⁻¹)” - The unit should be the same as that in Fig. 4 (mg C m⁻² d⁻¹).

p.1256, line 7 “resuspended matter . . . abiotic flux” - resuspended matter might be composed of not only “abiotic” lithogenic materials but also old carbon and microbial materials.

p.1256, line 8 “where we observed no difference” - What did authors mean? No difference in vertical pattern of total mass flux among three sites?

p.1256, line 14 “The obtained values” - Are these two values showing the arithmetic mean of analyzed %POC data from all three sites or two sites (345 and 235)?

p.1256, line 16 “The inorganic carbon (PIC). . . difference between total and organic carbon” - This sentence can be moved to the method section.

p.1256, line 17 “relative importance. . . high at depth.” – If the term “relative importance” is used as the same as % of mass flux (DW) in Table 3, this description is not correct at Site 235 because the highest %PIC was observed at 85m.

p.1256, section 3.2 - No description on the result of particulate nitrogen flux shown in Figure 4 (the lowest panel).

p.1256, line 25 “Using the long-term trap datasets. . .” - If authors use the datasets by previous studies such as Forest et al. (2013), appropriate reference citations are required here or figure caption.

p.1257, lines 1-4 “In general, POC flux were higher . . . except under ice cover. . . 100m depth” - If possible, the time-series data of sea-ice concentration around the study area

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should be added in Figure 5.

p.1257, line 23 “almost absent at site 135.” - According to Table 4, the relative abundance of amorphous pellets were also absent and few in upper layers at Site 235.

p.1258, section 3.4.2. - Are the observed microplankton assemblage typical in the study area during the late summer? Are the result description and the percentage in Fig. 7 based on the component of cell numbers for each plankton group in total cell numbers? Or are these data showing the contribution of each microplankton group to POC flux? I feel that more precise description is needed for the text of this subsection and the caption of Figure 7. In addition, description on the abundance and distribution pattern of calcareous zooplankton (Foraminifer and empty Mollusca shells) may be required to estimate their contribution to PIC flux.

p.1259, line 19 “importance of diatoms observed before (see Sect. 3.4.2)” - According to the last sentence in Sect. 3.4.2, absolute abundance of phytoplankton at Site 135 was lower than that at other two sites. I could not find the description about vertical distribution of diatoms at Sect. 3.4.2. The citation “(see Sect. 3.4.2)” may be changed as (Fig. 7), or additional documentation is required in Sect. 3.4.2. In addition, the fluxes of major phytoplankton and microzooplankton species should be presented in POC flux value, if possible.

p. 1260, lines 1-2 “Among the other groups, only appendicularians (mostly *Oikopleura* spp.) . . .” - Just in case, if the gelatinous “house” of *Oikopleura* was commonly observed in the samples, the “house” of appendicularians can be treated as a component of passive sinking flux, and it should be described in the appropriate subsection in this manuscript. pp. 1259-1260, section 3.4.4. - It may not be the matter for this manuscript, can the obtained data of swimmer fauna and their abundance in trap samples be corresponded to plankton net data in the Malina project? Were there any differences between drifting trap and plankton net samples?

p.1260, lines 23-24 “This is most likely due to the ice conditions. . .” - I think that sedi-

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ment trap mooring can be hanged from sea-ice.

p.1261, lines 11-13 “Moreover, the traps of our drifting mooring (collecting surface: 0.125 m²) were lower . . . above mentioned studies (collecting surface: <0.03 m²).” - I could not understand how the difference of collecting surface area is related to explain the lower POC flux in this study.

p.1261, line 21 “such peak events happen at short time scales. . .” - In general, the origin of trapped materials in bottom-tethered deeper trap are broader than that in shallow trap. The author’s idea may be more reliable if the authors could confirm the satellite color images around Site 135 for the deployment period of drifting trap. I am not sure that the drifting trap data at Sites 345 and 235 can be directly compared to the data of bottom-tethered deep traps because of the long distances between the moorings.

p.1262, line 18 “Lowest quantities of pellets” – According to Table 4, I am not sure that this description is written for the “pellet flux (all)” or “pellet flux (only 100% full pellets)” at sites 235 and 135. Precise description is needed.

p.1263, line 2 “the depth variations at sites” - Is there relationship with the distribution of swimmer assemblages in the studied trap samples?

p.1263, line17 to p.1264, line10 - As the authors show, %elliptical pellets are relatively abundant in deeper trap samples. I agree the possibility that the elliptical pellets at deep layers was mainly produced by deep layer dweller *Oncaea*. On the other hand, as far as I calculated the numerical pellet flux of elliptical form at Site 345 based on the data in Table 4, the numerical flux of elliptical pellets at 40m was the highest (although the many elliptical pellets might be degraded at 40m), which was higher or comparable to the elliptical pellet fluxes at 150 and 210 m. I am not sure the presented discussion in this paragraph is totally enough to explain the observed data from shallow to deep layers. What is the main producer of elliptical pellets in shallow traps? In addition, the occurrence information of *Oncaea* in the studied materials may be described in the

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result section for swimmers.

p.1265, line 20-. I think that the sentences are the kind of summary on this manuscript. I think the conclusion is not completely corresponded to the objectives listed in Introduction (p.1250, lines 18-24). Some of goals were well discussed. Although the influences of zooplankton community structure to particle flux variations are well documented, are there any influences of hydrographic conditions to the horizontal and vertical variations in particle fluxes and plankton assemblages? In addition, although there are description on the pigments, the quantitative estimation on the contribution of phytoplankton to POC flux should be shown in the results or discussion.

Figure 4 (lowest panel) - Please check the unit of nitrogen flux and correct it if needed.

Figure 5 - If possible, time-series data of sea-ice concentration around the trap area should be included in this figure.

Figure 7 - Is this percentage based on numerical cell number or carbon content of each microplankton group? Additional explanation is required in the caption, the figure legend, or the label of graph axis.

Interactive comment on Biogeosciences Discuss., 12, 1247, 2015.

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12, C217–C222, 2015

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