

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

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Dear Referee,

Thank you very much for the review and the constructive comments for improving the manuscript,. Please find below our reply to each one of your comments.

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 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.

REPLY: Don't worry about your English. Your comments were well comprehensible and even more comprehensive.

There was some misunderstanding of the goals of the study. The objective was not to identify forcing factors but processes shaping the vertical flux. We clearly identified 2 processes at the end in a conclusive paragraph. The forcing factors were already widely discussed and reported in the study of Forest et al. (2013) and the last sentence of our introduction was supposed to put forward the link between the present and that study.

Although we discussed our findings in the broader context of the study by Forest et al. (2013), we did not discuss these processes explicitly in connection with these forcing factors. In order to avoid confusion and to emphasize that processes are one of the objectives of our study, we may therefore as well remove the forcing factors from the last sentence of the introduction, which now reads:

Namely, we aim at documenting the composition of sinking particles throughout the water column to highlight some processes that shape the transit of these particles from the surface to the deep ocean.

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.

REPLY: This was corrected with the corresponding adaptation of the text:

Vertical flux of particulate organic carbon (POC) in the Arctic Ocean shows multiple facets. There is a marked regional variability illustrated by generally low POC fluxes ($< 100 \text{ mg m}^{-2} \text{ d}^{-1}$) in the central Arctic Ocean (Lalande et al., 2014; Olli et al., 2007) and high fluxes ($> 100 \text{ mg m}^{-2} \text{ d}^{-1}$) over the continental shelves and in polynyas (Amiel et al., 2002; Lalande et al., 2009).

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.

REPLY: Yes, we agree and went even further in clarifying this point. We considered that the mooring line scheme (fig. 2b) is part of the material and methods and the profiles with the environmental parameters (fig. 2a) part of the results. Accordingly, Fig. 2b becomes now Fig. 2 and Fig. 2a becomes Fig. 3. Also, we moved the descriptive paragraph about the mooring configuration in Sect. 3.1. (page 1254, lines 19-23) to Sect. 2.1. to complete the mooring line description as follows:

Figure 2 shows the schematic drawing of the moorings. Each mooring line was equipped with four traps at nominal depths of 40, 85, 150 and 210 m. Based on the data from the hydrographical casts the trap depths were chosen to monitor (1) the fluxes out of the PML likely to be influenced the most by the inputs from the Mackenzie River, (2) the fluxes out of the layer with highest phytoplankton biomass and production, (3) the fluxes out of the euphotic zone and (4) the fluxes monitored at a depth of other long-term moorings in the area and therefore best comparable with these data. For each deployment, the length of the mooring line and sampling intervals were adapted to the constraints imposed by bottom depth, ice cover and survey schedules. For instance at site 135, the trap at 210 m had to be removed from the mooring to avoid its grounding.

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

REPLY: Range of values is given and the text reads now:

The pH was checked with a pH-meter after the deployment and immediately before processing the samples in the laboratory. All samples showed a value between 8.3 and 8.5.

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.

REPLY: Yes, appendicularian houses are treated as passive sinking flux. They are however extremely difficult to identify and remain mostly together with the other organic aggregates and do not need to be picked separately. We completed the sentence accordingly:

Swimmers were handpicked from the samples with forceps under a stereomicroscope. Foraminifera and empty mollusc shells were considered part of the passive sinking flux and hence returned to the main sample, as well as eventual unidentified gelatinous material (e.g. appendicularian houses).

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

REPLY: The number of counted pellets is now reported and the text completed as follows:

Absolute numbers of faecal pellets counted were between 25 and 75 per sample. The pellets were sorted into three morphotypes, cylindrical, elliptical and amorphous.

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.

REPLY: As stated in the text, the data set from the long-term study was used to put the results from the Malina short-term study in a broader context. Both studies were part of the integrated ArcticNet programme, within which the planning of the field activities is coordinated. The use of almost identical sampling gear in the vicinity of each other gave us the opportunity to compare the data sets from the short-term and the long-term surveys. As mentioned in Forest et al. (2013) the samples and data from the long-term study were obtained and treated by the "Canadian" group of co-authors, while those from the short-term Malina study were taken care of by the "Monaco" group.

To avoid confusion, we rearranged the text of section 2.3. and emphasized on the difference between the two studies. The new text is:

2.3 Samples from long-term moorings

With the aim of putting the results of the Malina study within a broader context, we used a three-year record of vertical POC fluxes (2008-2010) as sampled with long-term sediment traps moored at ca. 100 and 200 m depth in the vicinity of the drifting stations (Fig. 1, Table 2) within the framework of the ArcticNet programme. The data from 2009 were reported in detail by Forest et al. (2013), where a full description of the methodology associated with long-term trap samples can be found. The traps were of the same kind (i.e. TECHNICAP PPS3/3) as those used in the short-term deployment of the present

Malina study, though the long-term traps were equipped with a 24 instead of a 12 sample cup carousel. Otherwise, the design of the trap was exactly the same as the one described in Sect. 2.1. Sample cups from long-term traps were filled with filtered seawater (GFF 0.7 μm) adjusted to 35 salinity with NaCl and poisoned with formalin (5 % v v⁻¹, sodium borate buffered). Long-term trap samples were processed (i.e. “swimmer” picking, fractioning, filtration), weighed for total mass, acidified and analysed for POC and nitrogen, similarly to short-term trap samples.

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.

REPLY: We agree with the referee’s comment. In fact, as stated above (reply of p.1251, line 13), we moved all of the mooring line description to sect. 2.1. Lines 4-6 of p.1255 were therefore removed and the text in the following paragraph completed accordingly:

3.2 Flux and composition of settling particles

Mass fluxes measured at the three sites showed different depth patterns (Fig. 5). Although fluxes increased in all cases from the trap at 40 m to the trap below at 85 m, at 150 m depth, mass fluxes were from lowest (15.45 mg m⁻² d⁻¹ at site 345) to the highest ones (43.20 mg m⁻² d⁻¹ at site 135) measured at the corresponding sites. At this latter site, the high flux value registered is possibly due to effects of matter resuspension, the trap being just 70 m above the ocean bottom (Table 1).. The overall highest mass fluxes of 54.20 mg m⁻² d⁻¹ and 51.94 mg m⁻² d⁻¹ at 85 m of sites 345 and 235 respectively, were observed just below the depth of relatively high phytoplankton biomasses shown by the fluorescence profiles in Fig. 3.

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).

REPLY: Units were corrected for all three values of this paragraph:

The vertical distribution of the total carbon fluxes was similar to the mass fluxes, with highest values (15-20 mg C m⁻² d⁻¹) at 85 m and a sharp decrease in flux towards depth, except for site 135 where the trap near to the bottom recorded a relatively high flux of 9.51 mg C m⁻² d⁻¹. At 210 m depth of the two other sites, fluxes were lowest (< 5 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.

REPLY: Yes, we agree with the referee and have clarified the text:

This is most likely due to the vicinity of these two traps to the bottom where resuspended matter including inorganic carbon may have been added to the vertical flux. This shows that the resuspended material was mainly of abiotic origin.

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?

REPLY: The point we wanted to make is that at the two sites 345 and 135, the vertical flux patterns of total and organic carbon showed a decrease in flux in the deepest traps compared to the traps above, while mass and inorganic carbon showed an increase. This difference was not observed at site 235. To make this clearer, we modified the text and the paragraph reads now as follows:

The only notable difference in the vertical flux pattern between total and organic carbon on the one hand and mass and inorganic carbon on the other hand concern the results from the deepest traps at sites 345 (210 m) and 135 (150 m), where fluxes of both mass and inorganic carbon were higher than at the above sampling depths. This is most likely due to the vicinity of these two traps to the bottom where resuspended matter including inorganic carbon may have been added to the vertical flux. This shows that the resuspended material was mainly of abiotic origin. Interestingly, at site 235, where all components of the flux, mass, total, organic and inorganic carbon decreased from 150 m to 210 m, the deepest trap was the furthest away from the bottom among the moorings at the three sites (Table 1).

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)?

REPLY: This was the arithmetic mean of all sites, but we put it more precisely now:

The obtained values were between 23 and 29 % for the traps at 40 and 85 m with one exception (17 % at 40 m, site 135), but decreased to 10 and 11 % in the traps at 210 m of 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.

REPLY: The sentence was modified and the method section completed accordingly:

The importance of inorganic carbon (PIC) relative to total carbon was, unlike POC, rather low at the surface and high at depth.

...and that same sample could be used after dismounting, for the analysis of total carbon and POC. Prior to POC analysis, inorganic carbon was removed from the sample by acidifying twice with a 1 M H₃PO₄ solution in excess, the filters being dried at 50 °C overnight, after each step. POC and N analysis was performed with a "vario EL" elemental analyser (elementar Analysensysteme GmbH) (Miquel et al., 2011) Inorganic carbon was calculated by the difference between total and organic carbon.

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.

REPLY: No, we meant the relative importance compared to total carbon. This is why we had introduced the sentence, which you wanted to be put to the methods section. The modified sentence is the same as in your previous comment:

The importance of inorganic carbon (PIC) relative to total carbon was, unlike POC, rather low at the surface and high at depth.

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

REPLY: Yes, this is correct. Although we were not using these data, we thought to put this graph, in order to complete the data set on the flux, since the absolute values were not given in Table 3. In fact, we will replace this graph by the one with the C:N ratios, which was also a suggestion by another reviewer.

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.

REPLY: As explained above, only part of this dataset (year 2009) was published in Forest et al. (2013). We used the 3-year time series to create a composite year. We therefore mention the reference now as a comparison:

Using the long-term trap datasets (cf. Forest et al., 2013) available for the region from 2008-2010 (Table 2), we have created a composite time-series of mean POC fluxes at ~ 100 and ~ 200 m depth (Fig. 6).

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 should be added in Figure 5.

REPLY: Adding the sea-ice cover data to the figure would overload it unreasonably. Also, we consider that this information does not give any clue to explaining why under ice cover, the flux at 200 m was higher than at 100 m depth. Since during the winter months, the ice cover is extremely high (mostly >90%) in any case, ice cover was mentioned more as an apposition to winter months than anything else. We therefore removed it from the text.:

In general, POC fluxes were higher at 100 than at 200 m depth, except during the winter months when the background flux was higher at 200 than at 100 m depth.

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.

REPLY: At site 135, we did not talk specifically about the total absence of pellets at two depths, but only about the near-absence at this site since they were only present at one depth sampled. The text is now modified to be more precise about the zero numbers:

Amorphous pellets were generally observed at relatively low numbers and were absent at 40 and 85 m of site 135 and at 40 m of 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.

REPLY: The cited references give some information on microplankton assemblages in the study area. But a particle interceptor trap not being the adequate sampling device for microplankton, we did not want to compare our data with a typical situation in the study area. We are assuming that organisms commonly present in the water column are likely to be present in the traps. This is why we were surprised by the near absence in our samples of the mentioned and other large-sized diatoms, which are commonly found in arctic regions.

Yes, the caption of Fig. 7 was wrong and a source of confusion. We did not show the contribution of the different groups to the downward flux, but only the relative abundance of each group within the microplankton at the different sampling depths. The absolute counts varying by orders of magnitude we preferred to plot the relative abundances for a better comparison between the samples.

We only counted and measured foraminifera, and not empty molluscan shells, which occurred rarely in our samples. The estimation of the foraminifera contribution to the PIC flux requires either direct measurement of calcium carbonate or an indirect estimation using a conversion factor and weight or volume measurements. We did some calculations with this latter method and obtained values as high or even higher than our PIC estimates from the total and organic carbon measurements. This suggests that such calculations need to be interpreted with care, but seemingly most of the PIC flux can be attributed to foraminifera. Still, as Sect. 3.4 announces, we were seeking to report the qualitative and not the quantitative aspects of the particles, with the first two subsections describing the identifiable particles and the following sections describing the eventual origin and the potential fate (through grazing) of the bulk particles, thus giving some information on the trophic conditions.

We restructured the text of this subsection to clarify the type of particles we describe, then by giving some taxonomic details on the three main groups, which we describe in the last paragraph site by site with reference to the figure.

The recognizable particles other than faecal pellets were protist cells which were counted and identified to genus level. This identification revealed a near absence of large sized diatoms (e.g. Chaetoceros sp., Thalassiosira sp.), which are commonly found in polar regions (Coupel et al., 2014; Lovejoy et al., 2002). Among the diatoms observed, the epiphytic genus Licmophora was the most abundant one. Flagellates, especially the dinoflagellate Pronoctiluca often showed high abundances, and only in a few traps, some tintinnids (loricate ciliates) and naked ciliates were important.

The identified taxa were classified into three groups, diatoms, flagellates and ciliates, of which the relative abundance in each sample is shown in figure 7. At site 345, the dominant groups were flagellates and diatoms, the former group being mainly composed of dinoflagellates and the latter of the genus Licmophora. Ciliates were present at all but one depths but at low relative abundance, except at 40 m where they reached 32 %. At site 235, microzooplankton (dinoflagellates and ciliates) were most abundant and diatoms were relatively important at 40 m depth. Similar to site 345, diatoms and flagellates showed highest relative abundances at site 135, while ciliates represented <10 %. At 150 m, the percentage of diatoms (53 %) was the highest of all three sites.

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.

REPLY: The reply to the preceeding comment (p. 1258, sect. 3.4.2) actually contains all the necessary explanations to this comment. The modified text (see above) describes the relative importance of diatoms at site 135. We therefore do not think that there is a need to change the citation to "(Fig.7)". Also, for similar reasons as for the foraminifera (see above), we think calculating the flux of major phytoplankton and microzooplankton species would go beyond the

scope of the present manuscript.

However, we added more detail to the sentence in sect. 3.4.3, in order to clarify the citation linking fucoxanthin and diatoms:

Fucoxanthin being a typical pigment of diatoms (Wright and Jeffrey, 1987), this could correspond to the increase in the relative importance of diatoms observed before (see Sect. 3.4.2).

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?

REPLY: As we already mentioned above (comment p.1252, line 5), distinguishing appendicularian houses in a plankton sample is next to impossible.

Appendicularians produce houses, actually their filtration net, in permanence. They reject their house either when the new house is sufficiently well developed, i.e. the old one more or less used/clogged, or at the slightest perturbation, e.g. when sampling with any device. Therefore, talking about observing appendicularians in samples implies that only the trunks are observed. We consider it was sufficient to mention in the material and methods section (see above) that houses are considered as passive flux, and we do not think that this needs to be repeated in section 3.4.4.

Again, as mentioned above (comment p.1258, section 3.4.2) a particle interceptor trap is not an adequate device for plankton sampling. As the reviewer mentions, the comparison of plankton data obtained by a net and by a trap is not the matter of this manuscript. The swimmers' data in the present manuscript are used as a support for our findings. We consider that the swimmers indicate the presence of these organisms in the surroundings of the trap, and we use the data to characterize the planktonic feeding regime (p.1264, line 2) of at least the observed plankton in the vicinity of the traps.

Yes, plankton net tows were done during the Malina project and data were published in this same special issue by Forest et al. 2012. These tows however integrated the entire water column sampled and a depth distribution cannot be done. The data confirm however the overwhelming importance of big calanoid copepods in the area, though by biovolume and not by numbers, by which the small species were dominating. The data also confirm the relative importance of appendicularians and pteropods. Only echinoderms, which had a notable relative importance in the net tows, were not observed in the traps of the present study.

p.1260, lines 23-24 “This is most likely due to the ice conditions. . .” - I think that sediment trap mooring can be hanged from sea-ice.

REPLY: Yes, drifting trap moorings can be tethered to ice floes, and we did not exclude this type of mooring in our text. We talked about drifting moorings in general (line 22). The point we made was to say that compared to moorings anchored to the bottom (line 20), a drifting mooring is exposed to surface and thus ice conditions. We completed the sentence as follows:

This is most likely due to the ice conditions not favourable to such deployments during most of the year and over a vast area, while for fixed moorings, ice conditions are only critical during deployment and retrieval.

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.

REPLY: Yes, we agree, the text is suggesting a misleading interpretation. We only wanted to mention that different trap designs are a possible source of measured flux variability. The text was modified accordingly:

Another factor of variability might be the trap design, which was different from the above mentioned studies, in particular the collecting surface of our mooring. However, the same type of sediment traps as for our mooring, are used for fixed moorings that have been deployed in the framework of the ArcticNet programme, and which were moored in the vicinity of our deployments during the Malina campaign (Table 2).

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.

REPLY: Yes, there is a difference in time resolution of short- and long-term trap deployments and the monitoring of peak events as mentioned in the text would require a short-term deployment of probably a week or more, which in the harsh environment of the Arctic Ocean would represent a most difficult task. However, for the proposed alternative, linking satellite color images to the sinking flux, we would need to understand this linkage. Forest et al. (2013) have discussed this question in detail using a huge dataset and with an extensive data treatment, which is mentioned in the text (p.1261, line 20).

Our point was only to put our data in the broader context given by the most similar data set possible (same traps, including the same time period and in the vicinity of our deployments). The reasons for the difference between the two data sets being explained in detail by the mentioned reference, we think that it is sufficient to recall the main points (low flux but a high flux regime during the sampling period “linked to episodic sinking flux events”, high standard deviation of the composite figure) and then to focus on what our data show, i.e. low fluxes, which are corroborated by the extremely low particle load obtained during the trap deployment (p.1261, line 26).

The trap deployments of sites 235 and 345 are indeed quite far away from the long-term moorings, but since our composite image includes all these moorings and since Forest et al. (2013) show that for each of the moorings, August 2009 was a period of high flux regime, we considered these results as representative for the area and compared globally our data with the fixed moorings.

In order to better emphasize the above exposed points we modified the paragraph accordingly:

The present study revealed the opposite situation with the POC fluxes recorded by the short-term traps relatively low compared to the long-term traps (Fig. 6). But as we already mentioned, the data of these latter traps are shown as a composite figure and the standard deviation is particularly high during the peak flux periods, while it is low during low flux regimes although we would expect spatial heterogeneities related to the distance between the different mooring locations, but also temporal variabilities between sampling years. Since we observed no spatial nor temporal trend in the flux differences during the peak periods, which occurred at all mooring sites, the high standard deviation reflects a general variability of the vertical particle flux during these periods. The 2009 period in particular was reported by Forest et al. (2013), who discussed these data in more detail and recalled us the fact that the “peak fluxes were presumably linked to episodic sinking flux events”. With respect to our data, we consider that, although the late summer months seem to be a period of an elevated flux regime (Forest et al., 2013), the vertical particle flux monitored by our traps during < 3 days is situated between or around these episodic flux pulses. From the data recorded by the particle camera (UVP5) we know at least that at that time, the particle load of the water column along the drifting path was very low (Fig. 4).

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.

REPLY: Yes, you are right the text is confusing and needs to be more precise. We did the necessary modifications:

The quantitative distributions of total pellets in the present study, be it numerically or in terms of carbon, indicate at sites 345 and 135 relatively high grazing activities above the trap at 40 m depth and above the deepest trap at 210 m and 150 m, respectively (Table 4). Lowest quantities at these sites in terms of carbon were observed in the samples from 85 m just below the phytoplankton biomass maximum.

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

REPLY: No, on the contrary, the swimmers are most abundant in the 85m traps where the pellets were least abundant (see also Table 4 and text p.1260, lines 5-7). We suppose that the referee was thinking about the swimmers’ contribution to fecal pellets. The relationship between the two is however not so straight forward. Swimmers data cannot be expressed as a flux, while pellets can. So, the eventual contribution of swimmers to the pellet flux is difficult to evaluate. We compared absolute numbers of swimmers and pellets counted and also the calculated “fluxes”, but none of the comparisons showed any relationship.

We think that our data (numbers, forms, taxa etc.) demonstrate nicely that there is no influence from swimmers on the pellet flux, and we consider that this does not need to be mentioned explicitly.

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 result section for swimmers.

REPLY: This whole part of the discussion was thoroughly overviewed. The data are now discussed in much more detail, taking into account the entire water column and a substantial amount of published data. Also, there was an issue with the misinterpretation of some of the copepod data from Forest et al., 2012. This does not change the findings of our study in any way. Instead of focusing solely on *Oncaea* spp., the discussion is now more generally including cyclopoid copepods and the genus *Microcalanus*, all of them producer of the same type of elliptical fecal pellets.

We did not add the *Oncaea* data description in the results section, since we do not have any data on *Oncaea*. As is explained in the text, *Oncaea* and other cyclopoid copepods were virtually absent from our traps.

Here is the new version of the paragraph:

Cylindrical pellets are mainly produced by large calanoid copepods (Carroll et al., 1998; Yoon et al., 2001), which were indeed the most abundant zooplankton observed in surface waters at this period of the year in the Beaufort Sea (Forest et al., 2012), but also elsewhere in the Arctic Ocean (Daase et al., 2008; Kosobokova and Hirche, 2000; Kosobokova and Hopcroft, 2010; Thor et al., 2005). Without any experimental evidence it is difficult to attribute a given type and size of pellets to a certain organism. The cylindrical pellets in our samples were between 40 and 170 μm wide (mean: 88 μm , ± 19 μm (SD), $n = 224$), which is within published ranges for large calanoid copepods (Sampei et al., 2009, Wexels Riser et al., 2008). Although we cannot exclude the presence of at least fragments of cylindrical pellets produced by euphausiids, the pellets in our samples did not show typical characteristics of these pellets (irregular cylindrical shape, filiform, colour differences, etc., see also Wexels Riser et al., 2002; Wilson et al., 2008). Moreover, we did not observe them as swimmers or their fragments as passive flux in our trap samples, and they were not reported from zooplankton observations made during the same study (Forest et al., 2012).

*The smaller elliptical faecal pellets are attributed to small copepods, but also to appendicularians (Carroll et al., 1998; Yoon et al., 2001). While for the cylindrical pellets we had at least indirect evidence for potential producers of the pellets (see Sect. 3.4.4 swimmers), for the elliptical pellets, we had only evidence for appendicularians but not for smaller copepods. Again, we can only speculate on the origin of elliptical pellets observed in our samples. Their mean width was 115 μm , ± 43 μm (SD), $n = 236$ (range: 44 – 282 μm). Some values reported from the literature are: 100 μm (Beaumont et al., 2001), 30-100 μm (Gonzalez et al., 1994) and < 60 μm (Sampei et al., 2009). This puts the size of the pellets in our samples at the high end of previously measured values. It is however clearly below the width size range (250-900 μm) of some values reported for elliptical pellets produced by appendicularians (Deibel and Turner, 1985; Lombard et al., 2013; Wexels Riser et al., 2008). We assume therefore that most of the elliptical pellets in our samples originate from small copepods, e.g. cyclopoids. However, given the size range of the pellets in our study, the bigger sized ones were probably produced by appendicularians (Fig. 7d). The example in Fig. 7d matches well the description given by Wilson et al. (2013): "The ellipsoid pellets... were slightly pointed on both ends and readily recognizable as larvacean fecal pellets." Forest et al. (2012) reported cyclopoid copepods of the genera *Oncaea*, *Triconia* and *Oithona* as being among the most abundant copepods in the study area. Together with the small calanoid copepod genus, *Microcalanus*, they represented close to 70 % of the zooplankton assemblage caught by a plankton net. Also, *Oncaea* is well known to dwell in and to be adapted to the meso- and bathypelagic zone below the euphotic layer (Kosobokova and Hopcroft, 2010; Thor et al., 2005). Individuals of the genus *Triconia*, too, tend to occupy mesopelagic depths (Kosobokova and Hopcroft, 2010), while *Oithona* and *Microcalanus* seem to have a preference for shallower depths even if they can be found over almost the entire water column (Kosobokova and Hopcroft, 2010). Although appendicularians were most abundant above 100 m depth during the study (Forest et al., 2012), we observed large elliptical pellets in all our traps, but they were too scarce to detect any depth pattern.*

*We cannot exclude that the elliptical fecal pellets produced at shallow depths reach the deeper traps at 150 and 210 m. We know however that this production was of minor importance compared to the cylindrical pellets. Also, given the relative as well as absolute increase in abundance towards the trap at 145 m, elliptical fecal pellets are produced at greater depths, most likely by cyclopoid copepods. The depth distribution of the swimmers in our traps corroborates these findings, though not quantitatively but with respect to the planktonic feeding regimes. Large herbivore copepods (*Calanus gracilis*) and appendicularians were the main swimmers in the traps at 40 m; a typically omnivorous copepod species (*Metridia longa*) prevailed in the intermediate traps, and in the deepest traps a carnivorous species (*Paraeuchaeta glacialis*) was most abundant after *M. longa*. Herbivorous conditions can be expected in the surface layer where primary production takes place, as well as an omnivorous or*

omnivorous/carnivorous regime in mesopelagic depths where deep-dwelling organisms and the vertical flux of organic matter are the main food sources. But together with our fecal pellet data, we can now put forward that below the euphotic zone, there was omnivorous activity sustained largely by cyclopoid copepods, which were also present at the surface in a rather herbivorous environment, thus adding a heterotrophic component and suggesting epipelagic retention of fecal pellets as described in a review by Turner (2015) as a possible reason for the drastic reduction of cylindrical pellets in the deeper sediment traps.

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.

REPLY: As we put it already in the reply to the general comments, there was some misunderstanding of the goals of the study. The objective was not to identify forcing factors but processes shaping the vertical flux. We clearly identified 2 processes at the end in a conclusive paragraph. The forcing factors were already widely discussed and reported in the study of Forest et al. (2013) and the last sentence of our introduction was supposed to put forward the link between the present and that study.

As already explained above, we removed the second part of the last sentence in the introduction to avoid any confusion and to put more emphasis on the processes. Therefore there is no discussion on the influence of hydrographic conditions on flux and plankton assemblages, which, as we said was already done extensively by Forest et al. (2012).

With regard to the flux contribution of phytoplankton, the pigment data were used to give more information on the quality of the carbon flux. As described in the text (p. 1259, line 3), the low flux did not permit to detect many of the pigments probably present in the particles. So, the discussion of phytoplankton contribution to the POC flux would be biased by the limited number of pigments detected. However, the pigments data are given in ng mg^{-1} POC and the contribution to the POC flux is therefore directly given by our results.

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

REPLY: Thank you for the remark. However, we replaced this graph by the one showing the C:N ratios. So, no correction is needed any more.

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

REPLY: As explained in our reply to p.1257, lines 1-4, we did not modify the figure, since we consider that this would overload it unreasonably and especially, the information would not add to the context of the paper.

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.

REPLY: As explained in our reply to p.1258, the caption is wrong and confusing. We only show the relative abundance of each group within the microplankton at the different sampling depths, and the percentage is based on cell numbers counted. Here is the modified version:

Figure 8. Relative abundance of phyto- and microzooplankton in the sediment traps at the different deployment depths. The identified taxa were grouped into diatoms, flagellates and ciliates.