Dear editor,

We thank the anonymous referees for their thorough comments, which helped improve our manuscript. Please find below our responses carefully addressed to each comment and following the sequence: (i) comment from the referee (in italic), (ii) authors response, (iii) modification made to the manuscript.

Responses to anonymous referee #1

Comment received and published: 2 October 2014

General comments

Comment #1:

(i) *A short comment on your study design: You deployed PPS3/3 sediment traps first to determine the POC flux and then compare the POC flux directly with the POC flux estimated from the particle composition of the gel traps (deployed after recovery of the PPS3/3 traps). I am very well aware of the logistical challenges when deploying free- drifting sediment traps, but my own experience from the Barents Sea however indicates a high patchiness in phytoplankton and zooplankton distribution and a frequently higher POC flux in free-drifting sediment traps (KC Denmark) deployed for 4-5 h compared to subsequently deployed traps for 24 h (data from the Barents Sea – Wiedmann et al. 2014, JGR: Oceans, In Press – and a fjord at the Western Coast of Svalbard, unpublished data). Similar challenges may exist in your data set (13638, section 3.3, line 12-13; 13639, section 4.1, line 7-9)? For further studies, you may therefore consider to deploy gel traps and sediment traps together (if technically possible) to be really able to compare the two different ways to estimate the POC flux.*

(ii) We agree with this comment. We are aware of the potential biases that a high patchiness in phytoplankton and zooplankton distributions can cause to the evaluation of particle fluxes collected in gel and standard free-drifting sediment traps. At each station, our decision to combine or not the deployment of gel and PPS3/3 traps was first constrained by the logistic (as noted by the referee#1), but also by the required different durations of deployment for each kind of trap. The PPS3/3 traps, were deployed over an average of 3.5 days to provide sufficient material for the bulk chemical analyses, while the gel traps were deployed over an average of 1.2 days to avoid an overloading of the gels (p 13631, lines 5-6), which would have complicated considerably the image analysis and decrease its accuracy. As a consequence, at a same station, most of our separate deployments of gel and PPS3/3 traps were of different duration but were overlapping in time and space to collect the same particle fields. Our only gel and PPS3/3 trap combined deployment presented the same differences in POC flux estimations from the two collection methods suggesting that another factor might play as we discussed in section 4.1 (p 13639, lines 9-14).

(iii) We added a sentence p. 13631 line 9 to explain our deployment strategies: "Due to different required deployment duration (shorter for gel traps to avoid overloading, see above), each category of trap was deployed on separate arrays, except at A3-2 (combined deployment; Table 1). All separated deployments of gel and PPS3/3 traps overlapped in time and location (except at station E-3 where they were successive), to maximize the collection of similar particle fields. The arrays had broadly..."

Comment #2:

(i) *In your analysis, you exclude large, rare particles due to statistical reasons. I understand absolutely your decision, but you may also take into account that these large, rare particles may stand for a substantial carbon transport. We just conducted a similar study in the Barents Sea (Wiedmann et al., 2014, JGR: Oceans, In press) and including the rare, large particles improved our results substantially (POC: volume ratio of the sinking material under different physical and biological situations then met the literature values of fecal pellets or diatom aggregates). Large, rare particles may be worth considering.*

(ii) We did not exclude the large rare particles from our POC flux estimations. The large rare particles have been excluded only from our spectrum analyses which required a binning of the particles. As noted in page 13634, lines 25-28, the bins containing 5 or fewer particles were not included in the analysis for statistical reasons. All POC flux estimations, however, included all the particles except the very small unidentifiable particles potentially deriving from small gel imperfections (p 13633, lines 21-23). We apologise for this confusion due to a probably too unclear description of our image analysis method. We agree that not including these rare large particles could have led to a significant misestimation of the carbon flux.

(iii) Page 13634, line 26: we changed "... fewer particles were not included in the analysis, ..." to "... fewer particles were not included in the flux spectrum analyses, ...".

Comment #3 (part1):

(i) *I would appreciate, if the authors describe more clearly, at which stage of the spring bloom they expected to meet at the different sampling station of KEOPS II. Section 2.2 gives a coarse characterization of the different stations (may be it would be more natural to place it in the results section?), but for me, not knowing the study area, it is however very difficult to decide the bloom stage based on the presented details.*

(ii) Bloom stages encountered at the different stations during the sampling is a central question of the KEOPS2 project and presents a very high interest for the whole community. A phytoplankton community sampled at one station can possibly be an early stage of a local bloom or a later stage of a distant bloom initiated at another location and then advected. As a consequence, address this question needs to consider several complex parameters: the age of the water masses since iron-fertilisation at a sampling time and location and thus implies an accurate knowledge of the water mass circulations (d'Ovidio et al., 2014; Zhou et al., 2014), iron supply modes (Sanial et al., 2014), and sources (van der Merwe et al., 2014; Quéroué et al., 2014; Bowie et al., 2014), potentially responsible for the various phytoplankton assemblages encountered at each stations (M. Lasbleiz, personal communication; Armand et al., unpublished data). Moreover, deriving bloom stages from these parameters is still insufficient due to observed contrasted plankton community responses to natural iron fertilization (Trull et al., 2014; Sackett et al., 2014; Closset et al., 2014).

Unfortunately, it was far beyond the scope of this study to build this overview of the extremely complex spatio-temporal dynamic of the KEOPS2 blooms, requiring a thorough synthesis of the KEOPS2 dataset.

(iii) p. 13629, line 11, we added: "For more information on the complex spatio-temporal evolution of the phytoplankton bloom over the full 2011-2012 annual cycle, we refer the reader to an animation of NASA MODIS Aqua chlorophyll images that can be found as a supplementary material in (Trull et al., 2014)."

Comment #3 (part2):

(i) *Providing numbers instead of only a list of dominant species (Table 6) may here also help, as well as including Ez 1%. In addition, sampling at station E-1 to E-5 took place during a period of three weeks and I wonder if you observed any succession during this time? I assume so (13636, line 3-4, 13643, line 1-3: "temporal evolution of the flux between E-1, E-*

3, E-5."), but clarification would make it easier to follow and help to explain your data (the shift from a high to a low e-ratio at these stations). It would also be very interesting to include these stations in the discussion, instead of only focusing on R-2 and A3-2.

(ii) The 1% PAR Ez is presented on Figure 2, with other water column properties. Indeed, we observed changes in phytoplankton community at E-stations. They are attested by the increase at E-5 of the biomass of the small *Chaetoceros* subgenus *Hyalochaete* (22.5% of the diatom community) which was less represented at E-1 (10%) and E-3 (2.3%).

(iii)

- In Table 6, we added the fraction (%) represented by the dominant diatom species at each station (column "Diatom community").
- p. 13645, line 25, we added "... will be discussed in detail here..."
- p. 13646, line 11, we added: "At E-stations, used as a time series, the net primary productivity was moderate (Cavagna et al., 2014), and a shift from a high e-ratio at E-1 to a low e-ratio at E-5 was associated with plankton community shifts (e.g. increase of *Chaetoceros* subgenus *Hyalochaete* biomass at E-5; Table 6).
- p. 13646, line 12, we rephrased the sentence to: "At stations R-2 and A3-2, although presenting..."

Detailed comments

Introduction

Comment #1:

(i) *A definition of "fecal aggregates" the first time you mention it would help your reader to understand what you are talking about.*

(ii) - (iii) Thank you for drawing our attention on this omission. We added a short definition of "fecal aggregates" on page 13627 line 18: "... copepod fecal detritus (intact or degrading pellets and fecal material re-agglomerated with phytodetritus, called hereafter "fecal aggregates")

Material and methods

Comment #1:

(i) *13629, line 11: "...varying biomass levels..." Do you think here about "Chl a surface levels" or about biomass in general?*

(ii) Every use of the word "biomass", without any precision refers to the general biomass (i.e. including phytoplankton, zooplankton, etc.) and is described by POC concentrations (fig. 2).

(iii) p. 13629, line 11, we replaced "...varying biomass levels (Fig. 1)" by "...varying biomass and surface Chl. *a* levels (Fig. 1 and 2)".

Comment #2:

(i) *13630: How did you define the mixed layer? As the temperature curve in Fig. 1 is not further discussed in the paper, you may present a density curve here instead and document the mixed layer depth in this way in a more detailed way*

(ii) The mixed layer was defined by Y. Park et al. using the density difference criterion of 0.02 σ_θ (Park et al., 1998):
MLD is the depth where the potential density = potential density at 10m + 0.02 Kg m^{-3}. The value presented here at each station are averages of all CTD casts at this station. We agree that a density curve is more suited here to illustrate the water column structure.

(iii) Fig. 2, we replaced the temperature curve by a potential density curve.

Comment #3:

(i) *13632, line 19: "10 per gel" instead of "10 by gel"?*

(ii) - (iii) Certainly, thanks. We changed "10 by gel" to "10 per gel".

Comment #4:

(i) *13632, line 26: Can you describe a bit more in detail how you conducted this preliminary image analysis?*

(ii) The preliminary image analysis consisted in a binary conversion of the images and a determination of the morphological characteristics of the most represented categories of particles collected in all gels. The tests on particle area alteration from threshold and correct identification from a set of shape descriptors were also part of this phase of our work.

(iii) To improve the clarity of this paragraph, the following changes have been made:

- p. 13632, lines 26-27 was changed to "A preliminary image analysis was conducted to select the best analysis method in term of particle identification. The particles were classified into three main categories based on...".
- p. 13633, lines 4-5: "Particle characteristics were then determined by conversion to" was replaced by "Pictures were converted to".
- p. 13634 lines 27-28: "All particle characteristics investigated in this study and their units are reported in Table 2." was moved to p. 13633 line 20, to mark the begining of the actual image analysis presented here.

Comment #5:

(i) *13633, line 26: I am a bit unsure if I understand this sentence right: You assumed a) a spherical shape for the aggregates and computed the volume from the ESD, (does this mean V= 4/3 * (ESD/2)^3 * pi?) b) and a cylindrical shape for the cylindrical fecal pellets (Why did you calculate the cylinder section in exactly this way?) Did you also use an ellipsoidal volume calculation? (It is included in Table 2)*

(ii) a) yes, we used the formula *V= 4/3 * (ESD/2)^3 * pi*, to calculate our aggregate volumes assuming spherical shapes; b) yes, we assumed a cylindrical shape for the cylindrical fecal pellets.
The best fitted ellipse determined by Image J, was used to access to particle lengths and calculate the ESD. Because of their frequent "V" shape (not straight), this method could not be used to accurately measure cylindrical fecal pellet volumes. We used the fecal pellet area and perimeter (independant from the "V" or straight shape of the pellets), and calculated the radius using the formula (1) p. 13634, and the length using the formula (2) p. 13634.

(iii) p. 13633, line 28: we added "...not always straight, their volume could not be accurately measured directly from their length, and was calculated...".

Comment #6:

(i) *13634, line 12: Writing "Figure 4, Line 2" also would make it easier to understand for the reader, where to look for line 2.*

(ii)-(iii) Certainly. We added "Fig. 4" to each reference to the lines displayed on this graph.

Results

Comment #1:

(i) *13636, line 16-19: "At all sites, most of the volume flux of phytodetrital material was carried by middle sized particles". I was just curious on which data you base this sentence – I assume you use the maxima in Figure 7? According to Jackson (Deep-Sea Research I (1997) Vol 44, No 11, pp. 1739-1767, Figure 13), I think that you would have to divide the volume flux by the image diameter to make the area under the curve proportional to the volume flux. I suggest having a look at that, as it might change your results.*

(ii) - (iii) We think that a mistake was made here. We already divided our volume flux by the image diameter, (the difference between the upper and lower diameter defining the bin width), as done previously in similar studies of the volume flux spectra or volume spectra (Jackson et al., 1995; Iversen et al., 2010; Ebersbach et al., 2011). In our figure 7 we divided the volume flux spectra (expressed in $cm^3\ m^{-2}\ d^{-1}$) by the bin width (cm) to obtain units in $cm^{-3}\ m^{-2}\ d^{-1}\ cm^{-1}$. This normalisation is needed to remove the effect of bin width variations accross the whole size range; the bin are logarithmically spaced to account for the decrease of the number of particles collected when size increases (McDonnell and Buesseler, 2010).

Discussion

Comment #1:

(i) *13643, line 15: A "." is lacking after the bracket*
(ii) - (iii) We added the missing full stop.

Comment #2:

(i) *13643, line 25-29: Are these statements deduced from the results of the present work or they general statements? Please give some references*
(ii) - (iii) This statement refers to the fact that the ^{234}Th method averaged the carbon flux over a longer time period than the trap deployments. All these time averages and their citations are already indicated on p. 13643 lines 15-24.

Comment #3:

(i) *13644, line 5: Please state what Ez stands for.*
(ii) - (iii) We added "Euphotic zone" inside the brackets.

Comment #4:

(i) *13644, line 13-15: "carbon export efficiencies up to 10 fold lower during the early (spring) than late bloom stage (summer)": I have difficulties to reconstruct on which data you base this argument on. Could you please clarify it?*
(ii) - (iii) It is a comparison between the KEOPS1 and 2 data of the carbon export efficiency. The data and their references are indicated on the previous sentence (p. 13644 lines 6-11).

Comment #5:

(i) *13646, line 12 and following: You state that physical aggregation seemed to dominate over the biological aggregation, due to the rarity of fecal pellets. This appears a*

very general statement to me. Please include more details, which kind of physical processes you would suggest to dominate in aggregate formation and why? In the following you start a paragraph on the inverse relationship between net primary productivity and export efficiency, which is in my opinion a very interesting topic. However, your discussion of this observation is rather difficult to read (long sentences) and ends unfortunately a bit pointless. I would suggest rewriting this paragraph, and possibly including ballasting effects/ different abilities of the microalgae to produce EPS as "natural glue" for the aggregates/...?

(ii) p. 13646, line 12: This sentence is a transition between the previous paragraph and the next. The statement of the dominance of physical aggregation (i.e. forming phytodetrital aggregates and fecal aggregates) over biological aggregation (i.e. forming fecal pellets) at stations R-2 and A3-2 is inferred from p. 13645, line 27 to p. 13646, line 11.

The next paragraph (p. 13646, lines 17-21), cites aggregation experiments conducted at six stations over and downstream the Kerguelen plateau (Laurenceau-Cornec et al., In press, 2014). This study focused on the influence of contrasted phytoplankton morphologies accross species variations on physical aggregation processes, resulting phytodetrital aggregate structures and sinking velocities. This article includes the investigations on potential ballast effect and TEP importance in aggregation processes suggested by reviewer#1.

We did not address these perspectives here since they are largely developed in Laurenceau-Cornec et al. (in press, 2014). We agree that reference to this work should occur earlier in this paragraph and more clearly.

We acknowledge that the discussion on the relationship between phytoplankton aggregate sinking velocities and carbon export efficiencies (p. 13646 lines 17-29), lacks of evidence at this stage. The evocation of the work from Laurenceau-Cornec et al. is however essential to discuss the potential role of the physical aggregation on contrasted export efficiencies. We understand that concluding that these experiments, unfortunately cannot be used here to explore this aspect any further is disappointing.

However, our discussion on the inverse relationship between primary production and carbon export does not end on p. 13646 line 29, but on p. 13648 line 4, providing the reader with other hypotheses.

(iii) p. 13646, line 17-21: we changed this sentence to: "In parallel of this study, roller tank experiments have been conducted to explore the influence of different phytoplankton communities on physical aggregation forming the phytodetrital aggregates and on their sinking velocities (Laurenceau-Cornec et al., 2014). These experiments consisted in the physical aggregation of natural assemblages sampled with Niskin bottles at high and low biomass sites during KEOPS2. The results suggest that the proportions of different phytoplankton types forming the phytodetrital aggregates could influence their sinking velocity (and potentially their efficiency at exporting carbon), via a control on their structure and excess density. However, ..."

Comment #6:

(i) *13647, line 1-9: "...unexpected high export efficiency considering its high zooplankton biomass". Zooplankton is doubtless an important factor in carbon attenuation, but they also can produce fast-sinking pellets, and contribute to an enhanced POC flux. You might include this.*

(ii) p. 13647, lines 6-9: we wrote "In this perspective, however, E-3 presented ...". We do agree that the usual view is that zooplankton, via fast-sinking pellets is an important contributor to the carbon flux. Here, we explore the possibility that this mechanism does not always prevail and that zooplankton grazing associated with a degradation of fecal pellets at depth could make zooplankton grazing as a factor potentially reducing the carbon export and possibly explaining the inverse relationship between primary production and carbon export.

(iii) We moved our hypothesis of a zooplankton grazing reducing the carbon export p. 13647 lines 10-13 "In the case ... Iversen and Poulsen, 2007)" to p. 13647 line 2.

Conclusion

Comment #1:

(i) *Conclusion 1 is absolutely right, however I think it is important to also point out that the conversion to carbon makes fecal pellets to an important carbon flux vehicle (as named in the abstract).*

(ii) - (iii) We added p. 13648, line 11: "However, when converted to carbon content, and where their degradation was limited, cylindrical fecal pellets still represented the dominant fraction of the flux."

Comment #2:

(i) *Conclusion 2: "The decrease of productivity..." You may add "from KEOPS 1 and the present study" to clarify that these are your results and not a general statement ..."shift from autotroph to heterotroph-dominated regimes": Is this shown in your study, a KEOPS 1 vs. KEOPS 2 result or a general statement? Please state it more clearly.*

(ii) The "shift from autotroph to heterotroph-dominated regimes" refers to the modes of carbon export either via phytodetrital aggregates (autotroph-dominated regime observed during KEOPS2) or fecal material (heterotroph-dominated regime observed during KEOPS1).

(iii) p. 13648, line 15: We added: "The decrease of productivity from bloom initiation (KEOPS2) to its decline (KEOPS1), related to a shift from autotroph- to heterotroph-dominated regimes (i.e. production exported via phytodetrital vs fecal material), could explain..."

Comment #3:

(i) *Conclusion 3: You may use "appear to" instead of "could"? Conclusion 3 is very general, and I miss a bit the relation to you study. May be you would like to change it to something that is more specific for your study – perhaps that you observed more phytoplankton aggregates and fecal aggregates at depth?*

(ii) This last conclusion is intentionally very general. It refers to the discussion relating plankton (phyto- and zoo-) communities to the productivity regimes observed at different stations (high vs low) and contrasted carbon export efficiency. This discussion is detailed from p. 13645 line 19 to p. 13647 line 20 and uses Table 6 as main support.

(iii) p. 13648 line 19, we added: "... and zooplankton grazing pressure (Table 6)".

Comments on Tables

Table 2:

(i) *Was your 10th bin actually including all larger particles since it has no upper limit?*

(ii) - (iii) Yes, this bin has no upper limit and included all particles with an esd bigger than 0.1752. As detailed on general comments (comment #2), our study excluded particles from the spectrum analyses only if 5 or fewer particles were included in a bin.

Table 5 - comment 1:

(i) *Minimum export efficiencies in italic are rather difficult to find. May be underlined numbers would be better?*

(ii) - (iii) Maximum and minimum export efficiencies are now both indicated in bold.

Table 5 - comment 2:

(i) *The maximum e-ratio of 0.32 is in bold, but not the e-ratio of 0.34. Was this on purpose?*

(ii) - (iii) We fixed this error. The highest e-ratio at E-1, 200m, is now in bold.

Table 5 - comment 3:

(i) *Footnotes: Some of the abbreviations presented here are not used in the table*

(ii) - (iii) These are notations from a previous version of this table. Thank you for signaling this mistake. We removed "PA, CFP, FA, O and Ez".

Table 6 - comment 1:

(i) *Include full name of T. nitzschoides once.*

(ii) - (iii) We added the full name at its first reference in the Table (site R-2, column "Diatom community").

Table 6 - comment 2:

(i) *Would be very interesting to include the depth of the euphotic zone here as well as numbers of phytoplankton and zooplankton.*

(ii) - (iii) We made the following changes to Table 6:

- we added a column for the mixed layer and euphotic zone depths.
- we added the relative fractions of the total diatom community represented by each diatom genera or species displayed in Table 6, column "Diatom community"
- we added the relative fractions of the total biomass represented by the mesozooplankton groups, genera or species displayed in the column "Mesozooplankton community"

Table 6 - comment 3:

(i) *A3-2: "Appendicularians" should be in one line.*

(ii) - (iii) Thank you for signaling this error introduced during the production process. The word "Appendicularians" now appears on the next line after "*Paraeuchaeta* sp. C1-C3".

Table 6 - comment 4:

(i) *"sp." not in italics.*

(ii) - (iii) All "*sp.*" have been put in regular style.

Comments on Figures

Figure 2:

(i) *Unsure about the unit "µm L^-1" for fluorescence. Perhaps you should use "Chl a" instead?*

(ii) - (iii) The parameter displayed here is the fluorescence converted to Chl. *a* values using a calibration with CTD Niskin bottle data. The new fig. 2 (modified according to general comment #2), displays now "Chl. *a*" instead of fluorescence.

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