Reply to anonymous referee #2

Referee: This paper combines an array of measurements (ship-based, moorings and underwater profiler, and remote sensing) together with elaborate computational, statistical and modeling approaches to “assess the variability and forcing factors of vertical fluxes of particulate organic carbon on the Mackenzie shelf” (in 2009). The paper represents an extensive integrative effort to amalgamate such a diverse range of measurements and techniques and should be commanded as such. Integration of multidisciplinary data sets using multivariate statistic& models is in line with the recent work of first author A. Forest, with recent papers dealing with zooplankton grazing/carbon cycling in the Beaufort Sea (e.g. Forest et al. 2011; Forest et al. 2012). The extensive data treatment in the current paper, using multiple data sources (e.g. annual hydrological time series, annual time series of particle sinking fluxes and short-term traps, remote sensing estimates of ice area, wind maps, underwater profiler for sinking flux estimates) and different resolution and/or approaches, makes it particularly challenging for critical review. Being a convinced advocate for the need for data integration, I strongly support the use of multidisciplinary tools to help provide insights into complex and often interrelated bio-physical processes that impact the biogeochemical cycling of carbon in the Arctic (and the global Ocean). That said, while fancy computational treatment of data may at times come at the price of losing track of the original datasets, it should never override the validity and reliability of the data. Simply put, the best modeling exercise (and following interpretations) can only be as good as its data feed. Unfortunately, this is one of the fundamental caveats of the paper. The objective here is to assess bio-physical controls on particle sinking export, therefore the variable “sinking export” is key to the overall analysis and interpretative approach. I have serious concerns with respect to the sinking flux estimates and derived estimates from the profiler and which form the core of the paper. These fundamental aspects, explained in more details below, need to be addressed in order to validate the approaches used here and solidify interpretations.

Reply: We would like first to thank Referee #2 for the energy and time invested in reviewing our extensive study. We have carefully read and taken into account all the comments, as we fully agree that the best modelling exercise is not better than the data fed in the exercise. Hence, we have addressed the need of validating our approach in order to strengthen our various interpretations. We have listed below the different changes made to the manuscript. In particular, we have removed the old Appendix A as suggested (i.e. the large meta-data table) and we have inserted a new Appendix that describes our rationale and documents the variability of fluxes that formed the crucible for the computational analyses.

Referee: - The authors use a combination of flux measurements from short-term and long-term deployments to estimate parameters for an algorithm to derive sinking fluxes (carbon and dry weight) from underwater video profiles of particle distributions (UVP) (see Figure 8). These UVP-derived flux estimates, as well as the equation parameters, are further used for graphic/statistical analysis and interpretations. In recent years, there has been quite a few studies reporting sinking flux measurements with short-term (Juul-Pedersen et al. 2010, Sallon et al. 2011) and long-term sediment traps (O’Brien et al. 2006, Forest et al. 2008) in the same area. These studies are known to the authors and cited in the text. Yet, the authors do not use this knowledge of sinking fluxes in their study area to assess (compare) their own estimates.

Reply: The main reason why we have not performed a comparison of our flux measurements with previous studies is linked to the fact that one of the co-authors of this manuscript (J. C Miquel) together with other co-authors (e.g. Gasser, Martin, Forest) is currently in the process of writing a manuscript that will be actually devoted primarily to this kind of comparison. Since this paper in preparation is also intended to be part of the Malina scientific corpus, we wanted to avoid too much overlap by going into a descriptive comparison of vertical flux variability in the present manuscript. That being said, we can see the need to document the "numbers" that we obtained in a larger context in order to strengthen their use for a modelling exercise. Hence, we have now inserted as part of the Appendix A a comparison of the flux variability in the upper 200 m obtained in our study against 12 previous studies from different areas of the Arctic Ocean (including Beaufort Sea, Chukchi Sea, Central Arctic Ocean, Laptev Sea, Barents Sea). This comparison took the form of an exhaustive Table of flux measurements obtained with both long-term and short-term traps as well as a complete section explaining our rationale. In this table (new Table A1), we can see that the fluxes measured by the sediment traps in our study are within the range of flux variability expected for the Beaufort Sea, and for low productive areas of the Arctic Ocean in general. Also not mentioned in Table A1, relatively low fluxes (0-66 mg C m^{-2} d^{-1}, with a mean value of 5 mg C m^{-2} d^{-1}) were reported by Cai et al. (JGR, 2010 – using 234Th) for the 100 m horizon in the Barents, Laptev and Kara seas, and central Arctic basins for the summer-fall period of 2007.

Referee: Focusing on short-term sediment traps only, the 50 m fluxes obtained by Juul-Pedersen et al. (2010) at 21 stations in the study area between June and October (2002-2004) range between 14.8 to 258.4 mg C m-2 d-1 (their Table 2). The 100 m fluxes measured by Sallon et al. (2010) at 12 stations in the study area in June/July 2008 range between 38.3 to 257 mg C m-2 d-1 (their Table 2). In the present study, the authors report extremely low sinking fluxes of POC based on their short-term sediment trap deployments, < 15 mg m-2 d-1. These fluxes are surprising low and somewhat dubious considering results from other flux studies with short-term drifting sediment traps (calibrated with 232Th) in the same study area and other Arctic regions (e.g. very low POC sinking fluxes, ranging from 37.7 to 77 mg C m-2 d-1 are reported for Hudson Bay, Lapoussière et al. 2009).

Reply: Vertical POC fluxes above 90 mg C m^{-2} d^{-1} in southeast Beaufort Sea are not that usual. In the studies of Juul-Pedersen et al. (2010) and Sallon et al. (2011 and pers. com.), roughly 90% and 70% of POC flux measurements were below 90 mg C m^{-2} d^{-1}, respectively. In fact, in the study of Juul-Pedersen, the results greater than 90 mg C m^{-2} d^{-1} were recorded close to the Mackenzie Canyon during the peak bloom period (30 June-8 July 2004, see their Table 1), a time-window hardly comparable to the ultra-oligotrophic conditions that prevailed in the third week of August 2009 (16-23 August) when our short-traps were deployed in the eastern region of the shelf. In the study of Sallon et al. (2011 and pers. com.), all stations were also visited during the bloom period (June-July) in a year of unusual increased productivity (Tremblay et al. 2011; Forest et al. 2011). Caution should also be exerted when comparing with the study of Sallon et al. (2011) as the authors in this study did not acidify the sediment trap samples before carbon measurements, so there might be some over-estimation with respect to the organic fraction. Also, it is difficult to really define a cut-off to separate what would be a 'dubious' trap measurement to one that would be 'accurate'. Following the argumentation made by the referee, a non-dubious measurement would be above 15 mg C m^{-2} d^{-1}, while those below are not likely. Here, we did not choose a specific cut-off for 3 main reasons as further explained below: (1) vertical POC fluxes below 15 mg C m^{-2} d^{-1} are not that unusual and have been already measured in the Beaufort Sea and in other areas of the Arctic Ocean (see previous reply), including also by the long-term traps used in the present study (see new Appendix A); (2) the relationship between the UVP5-derived dataset and sediment trap fluxes does not change much depending if the short-term trap dataset is included or not (see figure 1R embedded in this reply); and (3) we indeed recorded very low abundances of particulate matter at the stations where low fluxes were also recorded, especially at the stations where the short-term traps have been deployed in the third week of August (16-23 August, see new Appendix A). In addition, short-term trap measurements from the aforementioned studies are typically difficult to compare with sequential sediment traps because of difference in the trap design (e.g. aperture, aspect ratio – see new Appendix A) and operational mode. In our study, the short-term traps were in fact the same traps (Technicap PPS3/3) as the long-term traps, as correctly pointed out by the reviewer. Further info on these traps is given below.

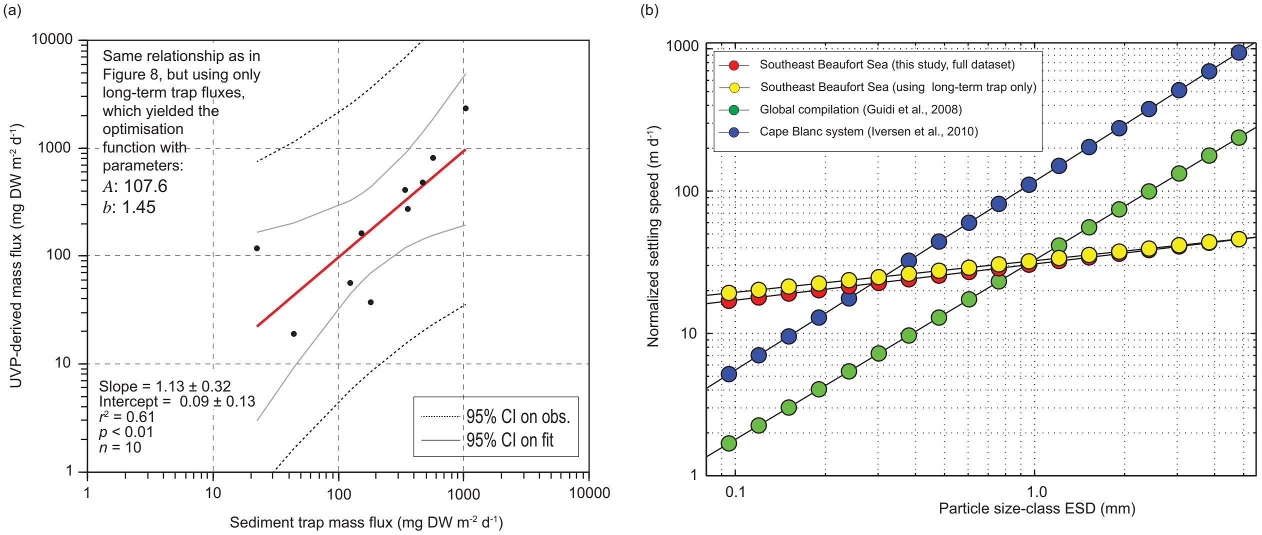

Fig. 1R. Same figures as figures 8-9 presented in the revised manuscript, but without the short-term trap dataset. Relationships between the UVP5 dataset and sediment trap fluxes remain relatively the same.

Referee: - I advise for caution when/if using the results from the long-term sediment traps deployed as short-term drifting sediment traps. This has important implications for the analyses of the UVP5-derived fluxes vs. the measured fluxes presented in Fig. 8. Low (< 15 mg C m-2 d-1) sinking fluxes in Fig. 8 are derived from the short-term deployments of long-term traps. As mentioned above, these fluxes are extremely low and should be used/interpreted with caution until the efficiency of the long-term traps in short-term deployments, according to the method used here, is demonstrated. The question to the authors then is does a significant relationship between the UVP5 and measured fluxes still exists if, as a cautionary measure, one removes data points <15 mg C m-2 d-1? This issue has great implications for analyses and interpretations in the paper (also see below), e.g. the result showing that estimated sinking velocities vs particle size the Beaufort Sea (this study) are at odd with other world regions could simply be related to the low flux estimates here, which drive the regression – parameterization between the UPV and fluxes, and further derivation of sinking velocities (Fig. 11).

Reply: Yes, the fluxes measured by the short-term traps in the third week of August (16-23 August) were in the lower range of vertical fluxes measured over July-August, which were themselves in a low range when compared with more productive areas of the Arctic Ocean (e.g. Barents Sea, Chukchi Sea) as detailed in the new Appendix A (Table A1). But very low fluxes are also common in the Arctic Ocean. Hence, a convenient way to demonstrate that, even if low, these fluxes represent simply the lower tail of the distribution is actually to present these results in the context of the particulate matter inventory (see new Appendix A). In this new section, we can see that there was a minuscule pool of particulate matter at the locations where the sediment trap fluxes were low. In addition, if we remove all data points of short-term sediment trap fluxes, we indeed obtain a similar relationship with a b value (1.45) slightly lower than the one obtained if the full dataset is included (1.51). This relationship is not presented in the revised manuscript, but as a supplementary Figure as part of this reply. In the revised manuscript, we have preferred to perform a sensitivity test of our relationship by doing a multiple random resampling of the UVP and trap datasets in order to estimate the zone of maximum likelihood for the A and b parameters of the relationship (see new Appendix A). This is also in line with some comments made by Referee #1. Finally, regarding the unusual sinking velocities, we need to underscore that this result is associated with the mean fractal dimension and should be seen as an idealized scenario if the settling speed would be a simple relationship of size. The relationship does not change even if we remove the short-term trap dataset (see figure in this reply). Obviously, the heterogeneity of conditions and particle types across the Mackenzie Shelf induced a relation that can look at odd with other regions. But in fact, we think that this is a very interesting result and shows the complexity of characteristics that might influence the sinking speed of particles with depth (see below for further details, and as now mentioned in new figure 8). However, we agree that we should read this plot from the perspective of a pure modelling exercise with the fractal dimension as the primary input variable, as also stated more clearly in the revised manuscript (see e.g. lines 765-767, 788-795). Indeed, sinking speeds obtained through our analysis (<50 m d^{-1}, Figure 8) are in agreement with the common values used in coupled physical-biological models that utilize a settling speed for detritus typically between 1 and 50 m d^{-1} (e.g. Wassmann et al., JMS 59:1-24, 2006). Further info on the obtained fractal dimension and low settling speed when compared against other studies is given below.

Referee: - The very low fluxes estimated with the short-term sediment traps raise concerns with respect to the methodology. There are no details on the deployment method (free-drifting, bottom anchored) in the Mat. and Method Section. Are the short-term moorings bottom-anchored (similar to long-term moorings)? Based on the trap line description, i.e. 4 sediment traps + instruments, with the traps weighing 40 kg each (17 kg in water, info not in ms), one would assume that the line is anchored to the bottom – especially considering the sub-surface floats requirements to achieve neutral buoyancy and the depth of the shallowest trap (ca. 50 m).

Reply: The design and deployment procedure of the short-term traps are presented in section 2.4 (second paragraph). Short-term traps were not anchored to the bottom, but attached to a drifting line equipped with an adequate series of Viny- and Nokalon floats at the top (with also an ARGOS buoy). The short-term drifting moorings were deployed and recovered by the IAEA team (Monaco), which has a long experience with both short-term and long-term mooring arrays in many coastal and oceanic environments. We added a new reference (Peinert and Miquel, JMS, 1994) to provide an example of study within which the use of sequential traps in drifting mode has been successful. Similar analyses combining UVP particle abundance, short-term and long-term trap datasets obtained with sequential traps were also published by Guidi et al., DSRI, 2008). In this study, they measured trap POC fluxes ranging from <1 to ~80 mg C m^{-2} d^{-1}.

Referee: There is extensive literature on hydrodynamic constrains on sediment traps design/deployment and the particular case of drifting sediment traps. The authors are certainly aware of published and recommended methods for drifting traps with drifting lines, either surface-tethered or neutrally-buoyant, hydrodynamic considerations, ways to minimize potential overtrapping/undertrapping, as well as other important technical considerations such as filling the complete trap with a seawater solution of higher density than the surrounding water (the use of added brine has been discussed & challenged in recent literature). These methodologies are quite different than long-term moorings where the trap line is bottom-anchored and the trap cups are filled with a brine solution to which a preservative is added (avoiding degradation of material over the long-term). The manuscript ought to present in better details the short term trap deployments and demonstrate that the methodology used here (using long-term traps, most likely bottom-anchored, with the main trap component not filled with filtered seawater or a brine solution) provided reliable estimates of sinking fluxes, especially given that the results obtained differ widely from studies using drifting traps in this and other Arctic regions.

Reply: Yes, as mentioned before, we have used the same traps (Technicap PPS3/3) in all deployments, which allows relative consistency with respect to hydrodynamical biases. An extensive analysis of the hydrodynamic biases affecting Technicap traps deployed in the epipelagic layer in the Beaufort Sea has been published within Forest et al (DSR1, 2010). In this study, the Technicap traps showed very good consistency against current velocity up to 20 cm s-1 (see their figure 8), which is also in the envelope of current speeds recorded during Malina (see Figure 5). In addition, all the trap sample cups were filled with a formalin solution of the same density and no brine to fill the whole sediment trap was added. This additional information is presented in section 2.4. In summary, we are confident that the use of the same kind of trap was in fact a good argumentation for the use of the entire dataset to conduct further statistical analyses (as mentioned in new lines 742-746).

Referee: - The extremely low short-term sinking flux results could also explain the very low A and b values obtained for the parameterization of the UVP-derived fluxes in this study compared to other studies (see lines 443-444). The authors note their much lower A and b scaling factors, with b twice lower, than in other studies. This also implies that the derived fractal dimension is much lower than in other studies. The authors draw an extensive interpretation from these results (lines 724 +), discussing the role of EPS in sinking fluxes, citing the study of Sallon et al. (2011). However, the authors fails to discuss actual results on flux magnitude and composition presented in Sallon et al. (2011) and which are most relevant for comparative analysis with the current study, including POC, EPS, fecal pellet fluxes, fecal pellet size-class distribution, etc. (applicable to this and other parts of the ms). These (and other fluxes) should be included in an in-depth review and analysis of the sinking fluxes obtained here and their impact on the estimates of A, b and other derived estimates. As a first step to constrain second and third-level interpretations based on UVP-derived fluxes, one needs to take a solid aim at validating them.

Reply: Agreed. Our new Appendix A aims at validating our approach and characterizing the error. When the error is taken into account, the parameter A found in our study was actually similar than the one obtained by Guidi et al. (2008) (see revised Table 2). But the b value (and associated fractal dimension) was significantly lower. A fractal dimension of 1.3 (as found here) is not impossible, it is just at the low end of the values ranging from 1.1 to 2.3 typically observed in marine environments. In fact, it is much likely that the very high fractal dimension around 2.4 (i.e. compact and less porous particles) found by Guidi et al. (2008) and Iversen et al. (2010) was the result of particle collection at greater depths (~400 m for Guidi et al. 2008; and ~1300 m for

Iversen et al. 2010) than here. The average collection depth in our study was ~125 m, a few tens of meters below the weak subsurface chlorophyll maximum (~60-70 m, when observed). To point out some similarities, results from Alldredge (DSR1, 1998) for particles sampled offshore California showed that the fractal dimension of marine snow aggregates within the surface layer following a phytoplankton bloom have a typical fractal dimension around 1.1-1.3. Thereafter, different physical and biological mechanisms can lead to an augmentation of the compactness of particles as they sink (e.g. coagulation, grazing, microbial degradation, etc.; see Burd and Jackson, 2009 for a review). Hence, our results are simply in line with a setting reflecting the post-bloom conditions and relatively shallow collection depths. We have corrected the Figure 8 and inserted some sentences in the revised manuscript to reinforce the fact that our results represent a distinct ensemble of biogeochemical and physical conditions (see e.g. lines 765-769). Regarding the impact of EPS, we simply cite the study of Sallon et al. (2011) in order to support our suggestion on the link between POC fluxes, EPS exacerbation in low-nutrient conditions, and the low fractal dimension of particles (i.e. porous, filamentous) as observed in our study. In fact, we think that our discussion on this aspect is relatively short (lines 797-805) and that we would gain very few information by going for an in-depth review of particle composition from previous studies that were actually conducted in very different conditions (i.e. bloom period). However, we do it when it supports the context of our field campaign (e.g. Lapoussiere et al. 2011; Kellogg et al 2011 for the linkages between bacterial production and vertical fluxes, see new line 916). Also, for conciseness and coherency across the Malina Special Issue, we prefer to discuss the different complementary datasets on lipid degradation products and other geochemical/biological proxies as recorded across the Mackenzie Shelf at the same time as our study was conducted in order to constrain our interpretations (e.g. Rontani et al., 2012; Tolosa et al., 2012; Ras et al. 2011; Ortega-Retuerta et al., 2012).

Referee: - A similar comment as above applies to other derived results/interpretations in the ms. It is somewhat disconcerting that the authors may have missed/ignored the red flags associated with their short-term flux data set (e.g. extremely low fluxes, extremely low derived parameter b and derived fractal dimension, uncommon settling speeds & trend, Fig. 11), in favor of their use to derive UVP flux estimates, and then to further derive 3-D maps (Fig. 9), 3-D visualizations of fluxes (see lines 499 +), and numerous other interpretations (e.g. see point above, multidimensional analyses – Figs. 14-16). While the exercise is technically and visually attractive, again the question of its validity undermines the scientific output and interpretation.

Reply: We agree that the 'numbers' from the short-term traps are low, but this is because the deployment of these traps has been done at stations where the upper water column was almost 'empty' (as shown in the new Appendix A, and as also recorded at times by the long-term traps). While being low, and thus subjected to increase uncertainties, the fact that we have somehow many of these points for our analysis make it possible to use them in further computations. As mentioned above, if all the data points from the short-term trap measurements are removed from the UVP-trap flux regression fit (Figure 8), the parameters A and b from the exponential equation do not vary much (see figure 1R embedded in this reply). Furthermore, if we proceed to a random resampling of our database (with at least 5 points to allow for statistically coherent results), we obtain a solution that converges toward the parameters found initially when using the full dataset (new Appendix A). Finally, a low b value, a low fractal dimension and low settling speeds do not transgress any of the previous results from the literature, but reinforce the fact that the speed and compactness of settling material might increase with depth. In the revised manuscript, we have strengthened this aspect (see section 4.2 and the full new Appendix A) that was not well explained in the initial manuscript, so we would like to thank Referee #2 for this insightful remark.

Referee: On a few occasions, the authors indicate that their results may require to be taken with caution, e.g. "... the abrupt transition from a high-to-low POC flux regime was linked to a real shift in the particle abundance from Cape Bathurst to Banks Island – and not to an artifact of the visualization." (lines 515-517), "If true, the amazingly high vertical fluxes..." lines 785-786. Unfortunately, these brief statements hint that the authors are aware of their unusual results (we expect no less from the experienced list of authors) but opted for the "high-tech" road, with intricate analyses and fancy visual representations. Unfortunately, this approach ends up undermining the exercise and the derived interpretations and conclusions.

Reply: The 3-D plots and other associated figures should be seen as a "photography" of the studied system in late July-August 2009. The strong discontinuity in productivity and vertical fluxes observed between Cape Bathurst and Banks Island is not a new result. Here, we wanted to underscore that what we observed was not a product of the visualization, but a real transition. On the side of Banks Island, the physical/geochemical regime is influenced by the anti-cyclonic branch of the oligotrophic Beaufort Gyre; while on the side of Cape Bathurst, upwelling is topographically enhanced and primary production is always higher than on the "other side" (e.g. Tremblay et 2011). As for the amazingly high vertical fluxes on the inner Mackenzie Shelf, those have been recorded very close to shore; so many processes can modify the "pure" vertical signal in this zone (such as resuspension, river plume material, primary production just above the bottom, etc.) so our cautionary remark is actually relevant in this context. The particle concentration as observed by the UVP was really high at some of these stations, resulting in very high fluxes when transformed with the empirical equations. Indeed, similar results were obtained by O'Brien et al. (2006) who recorded POC fluxes up to 7000 mg C m^{-2} d^{-1} on the inner Mackenzie Shelf in summer 1987 (see their Figure 8 and our new Table in Appendix A). Of course, those extreme values are minor observations in the overall flux dataset recorded in Beaufort Sea as well as within our UVP-derived POC flux dataset that ranges mainly from 10-100 mg C m^{-2} d^{-1} (Figure 9), a range that is acceptable for the southeast Beaufort Sea, especially beyond the shelf-break (Appendix A).

Other points of importance:

Referee: -The justification for a log-log relationship rather than a linear relationship for estimates of the same variable (sinking flux, Fig. 8) needs to be clarified. This is a classic approach for remote sensing estimates of chl a in the ocean since they range over a few orders of magnitude. However, for POC (and mass) sinking flux, the range of variation is much narrower, ranging typically from ca. 100 mg C m-2 d-1 or a bit less to < 1000 mg C m-2 d-1.

Reply: Yes, we provide a justification in the revised manuscript (new lines 1050-1052). In brief, the use of a log-log relationship is justified by (1) our wish of consistency with previous studies using a similar methodology (Guidi et al. 2008; Iversen et al. 2010); (2) by the fact that mass fluxes ranged actually over two orders of magnitudes (10-1000 mg DW m^{-2} d^{-1}), such as POC fluxes in the range ~1-100 mg C m^{-2} d^{-1}; and (3) because our minimization procedure makes use of the log-transformed fluxes to give equal weight to high and low fluxes when searching for the best parameters. As mentioned above, vertical POC fluxes higher than 100 mg C m^{-2} d^{-1} are not common in the Beaufort Sea, especially in late summer (Sallon et al. 2011, pers. com.; Juul-

Pedersen et al. 2010; Forest et al. 2007, 2010). In fact, no one of these studies have recorded fluxes higher than 250 mg C m^{-2} d^{-1}, such as those measured in e.g. Barents Sea (Reigstad et al., DSRI, 2008). See also our new Table in Appendix A for an in-depth comparison.

Referee: - Lines 774 + : “High fluxes (> 50 mg C m-2 d-1).” These are not high fluxes – In particular for this area, the authors should compare their flux values with those from other studies in the area.

Reply: Agreed. As we said above, this statement is made relatively to the overall dataset recorded in Beaufort Sea. If we accept that a common range for POC fluxes in the area is below 100 mg C m^{-2} d^{-1} (e.g. Juul-Pedersen et al. 2010), the 50-100 range can be seen as relatively “high”. Of course, this is debatable, especially in a larger context. So for clarity purpose in the revised manuscript, we have changed such a statement by “fluxes higher than 50 mg C m^{-2} d^{-1}).

Referee: They could also compare with other regions where high fluxes are often observed, i.e. Barents Sea. At the other end of the spectrum, the extremely high POC fluxes of 1000 – 5000 mg C m-2 d-1 should definitely be put into perspective (and perhaps challenged?). The authors hint to the questionability of these fluxes on lines 785-786 “If true, the amazingly high vertical fluxes...”; yet, they avoid any comparison with previous fluxes in the area or in other coastal areas - which are again, necessary to validate these results.

Reply: Yes, we have included a reference for vertical fluxes observed in the Barents Sea (Reigstad et al. DSRI, 2008) in the new Appendix A as a perspective from another system. However, here, we do not want to go for a full review of vertical POC fluxes across the Arctic Ocean even if we provide an exhaustive range for 12 different studies (Appendix A). This would be the subject of a full stand-alone review manuscript that we could indeed make as a community in a further step. Some effort in that direction has been already made by Honjo et al. (PIO, 2010), but no short-term trap measurements were included in this review, unfortunately. For now, we prefer to keep focus on the variability in our area and associated forcings.

Referee: - Fig. 10 Example of extremely low (and improbable POC fluxes). The vertical flux of POC is < 30 mg m-2 d-1 (panel c) and between 0.5 and 5.5. in panel d)!

Reply: We would like to add some precision on the histograms presented in this figure. The Figure 10 presents the fluxes for every size-classes as derived from the UVP profiler. The “total” POC fluxes should then be calculated as the sum of all size-classes, which provides a mean flux of 240 mg C m^{-2} d^{-1} over the shelf itself; and 45 mg C m^{-2} d^{-1} beyond the shelf break. We have now inserted these cumulated values in the legend of Figure 10. In fact, our study was one of the few among previous studies to provide actual estimates of vertical POC fluxes on the Mackenzie Shelf itself during the summer period.

Referee: - Table A1 is unecessary. This table is appropriate for a database (e.g. Polar Data Catalogue) rather than a paper.

Reply: Yes, we agree. We have removed Appendix A and we inserted a new Appendix to explain the rationale and uncertainties of our approach relating UVP particle abundance and sediment trap fluxes.

Referee: - Fig. 12 Are the structures well defined or a result of the interpolation of points? Please indicate points on this figure. Also, zooplankton panel; the authors mention that they are confident that their zooplankton biomass estimates represent the zooplankton biomass "since large zooplankton represent the bulk of zooplankton in this region" – However, the small zooplankton could represent a significant component. It would therefore be appropriate to indicate "Large zooplankton biomass" and indicate the size class in the figure/ figure legend. Also appropriate to indicate that the zooplankton biomass is derived from the UPV5.

Reply: The main structures of Figure 12 are well defined, but the interpolation also resulted in the extension of these structures in depth/day when no sampling was performed in any of the two zones. The goal of this figure is actually to give a general contrast between the shelf and offshore environments in terms of main biological features. We think that this figure enables to grasp easily the patchiness of these features over time and across space. We have tried to indicate the points (profiles) on this figure, but we lose rapidly clarity and we would like to keep the figure like this. But if the Editor insists, we will provide a Figure with the different profiles. In fact, the interpolated data were not forwarded in the various statistical analyses, as they did not correspond to any vertical POC flux profiles. As for the zooplankton biomass, we agree. We will mention that it corresponds to large zooplankton and obtained from the UVP5 (see also next reply). We would like also to underscore that the zooplankton dataset as recorded by the UVP5 was removed from the particle dataset before conducting the various statistical analyses.

Referee: - Did the authors attempt a correlation between their zooplankton biomass estimate and actual in situ estimates. Some of the authors are zooplankton specialists and one would assume these data are available.

Reply: Yes, this has been validated and has been the subject of a paper already published by the main author in collaboration with a team of zooplankton specialists (Forest, A., Stemmann, L., Picheral, M., Burdorf, L., Robert, D., Fortier, L., and Babin, M.: Size distribution of particles and zooplankton across the shelf-basin system in Southeast Beaufort Sea: combined results from an Underwater Vision Profiler and vertical net tows, Biogeosciences, 9, 1301-1320, doi:10.5194/bg-9-1301-2012, 2012.)

Referee: - Lines 812 +. The authors cite other sediment traps studies in the same area (but do not compare their sinking flux results with these studies). Their conclusion is that "none of these studies had a multi-parameter dataset with a spatial resolution fine enough to proceed to a state-of-the-art" variation partitioning analysis of vertical flux predictors, corollaries and spatial patterns." I would argue that the current study, while having the "multi-parameter dataset that could help address fine-scale resolution of vertical fluxes, fails to do so because due to the lack of validation of the data that are at the core of the analysis. This validation, in the form of comparisons with other datasets in the same region –especially short-term drifting traps recent studies, critical analysis of the methods employed, and analysis of error transmission during iterative mathematical procedures, is essential to support interpretations & conclusions.

Reply: A comparison is now made through the new Appendix A in order to support the use of those datasets in a second-level analysis. As presented above, we have confidence in our core dataset of sediment trap fluxes, especially given the "real" inventory of particles recorded by the UVP that ranged from minuscule particulate pools (offshore stations and in late August) to locations with very high concentration of particulate matter (e.g. nearshore and in the Mackenzie

Canyon). Definitely, the Mackenzie Shelf region is a very complex area with extreme biogeochemical gradients (sensu Carmack and Wassmann, PIO, 2006).

Referee: - Lines 958-959 "From a pan-Arctic perspective, the southeast Beaufort Sea might not appear to be the most interesting system to study with respect to primary productivity (e.g. Ardyna et al., 2012). "interesting" really is a matter of perspective. I surmise that the authors mean that the region is of low productivity?

Reply: Yes, maybe the term "interesting" was not well chosen. We have erased it and changed it for "low productive" (see new line 1008).