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 #2

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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.

Review of paper “Downward particle flux and carbon export in the Beaufort Sea, Arctic Ocean; the Malina experiment” by Miquel et al.

The MS focus on the downward flux of particles, and their composition during three short time sediment trap studies along the shelf break of the Beaufort Sea, August 2009. The study present interesting data, and the downward particle flux of carbon are of interest and importance to understand the biological carbon pump in this region. So is the mechanisms regulating this export, and the core discussion of this paper focus on the role of zooplankton and their contribution to particle flux through fecal pellets identified from the sediment traps. The results of downward carbon flux are compared with long-time sediment traps studies in the region, to place the present study in a broader context, and with a discussion of the relative lower flux rates observed from the short time traps.

I find this study of great interest, and especially the comparison with long-time sediment traps results, with identical sediment traps, is of great interest and importance to evaluate results from and comparisons with other studies that most often includes either short or long time studies. This kind of study is rare, but very important. The discussion of zooplankton as flux providers/ modifiers is also important, and there are still not that many studies investigating the fecal pellet composition in sediment traps from Arctic regions that well. The data therefor deserves being published.

There are however some issues I would have liked to see modified, further discussed or commented upon in the text, and recommend the paper for publication with moderate/major revisions.

Main issues: Language and clarity: The MS is well written, and the figure and tables informative and easy to read. But – I suggest a change for Figure 4- lower panel- It would be more informative if you showed the C/N ratio of the sediment trap material, instead of the PON export. First because the PON flux is never mentioned in the MS, second because the C/N ratio is of interest for interpretation of eventual resuspension impacting the deepest traps, and it takes some more effort to interpret from Table 3.

REPLY: Yes, we agree on this point and the graph was replaced and the figure caption modified accordingly. The description of the C/N results, which was related to Table 3 (at the end of section 3.2) was moved to the description of Fig. 5 (former Fig. 4) a few paragraphs further up.

Please note that due to addition of a figure, the numbering is now figure 5 instead of figure 4. Here is the revised caption:

Figure 5. Downward flux of mass, particulate carbon (POC, PIC and TC, the sum of both) and carbon : nitrogen molar ratio obtained from drifting sediment trap moorings. Colours correspond to sites in Fig. 1.

Title and abstract reflecting content of MS: I suggest the title should be slightly modified to better reflect the strong focus on the role of zooplankton as contributors to flux through fecal pellets in this paper. Both the abstract and the discussion have a emphasis on this rather the overall carbon flux. The abstract is clear and well written, but the last sentence of the abstract, is not discussed in the MS (but could have been to explain the limited role of diatoms, relative to flagellates).

REPLY: We modified the second part of the title, so that it evokes the implication of zooplankton in the vertical carbon flux. Concerning the last sentence, we agree with the referee. The autotrophy mentioned was related to another flux article from the Malina project (Forest et al., 2013) to which we refer frequently in our manuscript. The data of our study do not concern the “autotrophic” period of early summer and we therefore removed this aspect from the sentence. The discussion about heterotrophy is now rather extensive (see reply to comment on p.1264), less with respect to microzooplancton than to mesozooplancton. This is because our data on microzooplancton in the trap samples are not necessarily representative of the presence of these organisms in the water column. The use of these data to explain the role of flagellates within the ecosystem is therefore limited.

The title reads now:

Downward particle flux and carbon export in the Beaufort Sea, Arctic Ocean; the role of zooplankton

Methods and interpretations appropriate: the methods are appropriate for the aim of this study (given that the aims presented in the introduction is moderated; see comment below). The interpretations are in a few occasions contradicting (see specification below), and I miss a comment upon the aspect ratio of the sediment traps, given that the low sedimentation rates obtained. I also have some comments to the fecal pellets quantification, and the discussion on the vertical distribution of fecal pellets versus swimmers found (see specific comments below).

REPLY: All of the points mentioned in this comment (aims-discussion, contradicting interpretations, trap aspect ratios, fecal pellets quantification and vertical distribution and their producers) were addressed in the replies to the specific points presented below.

Introduction and aim: well written and nice focusing of story, but the last paragraph presenting the main aim of the study, the authors include aspects like identifying forcing factors of greatest importance to downward particle flux i.e sea ice cover, upwelling events or zooplankton community structure. Only the last factor is discussed in the MS at present. Sea ice cover and upwelling events are not discussed.

REPLY: There was some misunderstanding of the goals of the study. The objective was not to identify forcing factors but processes shaping the vertical flux, but which are sensitive to the mentioned forcing factors. These 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.

We clearly identified 2 processes at the end in a conclusive paragraph. 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.

Discussion: The discussion focus upon the discrepancy between the long-time and short time sediment traps, and the role of zooplankton (despite more ambitious aims in the introduction, and abstract). This may be a result of shortening of the MS prior to submission (?), but should be coherent with aims and results. It would be useful with some comments on the aspect of seasonality that could explain the minor impact of diatoms (as stated in the abstract), and also that the eventual impact of resuspension for the vertical flux pattern is discussed in light of the C/N ratios provided in the MS. I also miss a more thorough reference to other work regarding the potential producers of fecal pellets, with respect to size. The fecal pellets shown in the pictures (fig 6) are quite large (seen from the scale bar), and should intuitively correspond to large zooplankton, but the authors also suggest small copepods to contribute. Here, the fecal pellet diameter compared to other work could provide more substantial support or arguments in the speculations of origin (see more specific comments with reference to MS below).

REPLY: We already explained that there was some misunderstanding of the goals of our study we presented in the introduction. Many of the numerous text additions in the discussion (see below) focused on the different trophic interactions and processes involved in the vertical particle flux. There should now be a sufficient balance between the aims and the discussed results of our study. As mentioned above, the seasonal aspects and autotrophy (role of diatoms) were part of an article reporting results from the same project (Malina, Forest et al., 2013), where they are discussed thoroughly and to which we refer frequently. The discussion on the C/N ratios and the extensive text on size and producers of the different fecal pellets are presented hereafter in the replies to the specific comments.

Specific comments

p 1251, l 18; an aspect ratio of 2.5 is low compared to recommendation of >3, 5 or 8 dependent on current regime as by JGOF report (1989) to avoid under-trapping of particles. This is not commented upon or discussed, neither in comparison of long- vs short time traps (I am aware they are same type), or in comparison with other studies.

REPLY: As specified in the text, the ratio of 2.5 concerns the cylindrical part. The type of trap we used is however a mixed-shaped model. The top 100cm of the trap are cylindrical and the bottom 60cm are conical shaped, which makes a ratio of 4.0. Although it is a mixed-shaped trap, we consider the aspect ratio sufficient to comply with the recommendations mentioned by the

reviewer (we suppose it is the US GOFS report). We put more details in the material and methods section and also completed the discussion accordingly.

1) methods section:

The traps used were TECHNICAP PPS3/3 of cylindro-conical body shape with an aspect ratio of 2.5 in the cylindrical part and 4.0 over the whole length. The unbaffled aperture of the traps had a collection surface of 0.125 m^2.

2) discussion section (p.1261, line 10):

Annual and seasonal variabilities and also spatial heterogeneities may be at the origin of the flux differences between the present study and the published data. Another possible factor of variability is the trap design, which in our case was different from the above mentioned studies, in particular the collecting surface. However, in the present study the same type of sediment traps was used for both drifting and fixed moorings, and which were both sampling in the same region and during the Malina campaign (Tables 1, 2). Nevertheless, the traps of the two moorings are likely to collect different amounts of particle flux due to spatial heterogeneities and also due to the hydrodynamic environment. A fixed mooring is exposed to water currents, but not so a drifting mooring as the adjective indicates, although it appears that the direction of the drifting path sometimes differs from the current direction. In order to minimize collection biases, trap moorings should be deployed in tranquil regions and the aspect ratio of the traps should be 3 for water currents < 10 cm s^{-1} and higher if water currents are expected to increase (U.S. GOFS Planning Report 10, 1989). Current speeds at the fixed moorings site of this study were < 10 cm s^{-1} (Forest et al., 2013) and the aspect ratio of the traps was 2.5 in the cylindrical part and 4 for the overall length. We would therefore expect a minimum collection bias for both mooring types. Still, Forest et al. (2011) showed in a similar study that the fluxes measured with short-term drifting traps were always higher than those with the long-term fixed traps, but the collecting surfaces were different between the two mooring types.

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).

p 1253, l 10; It would be useful if you provided the actual C-conversion factor to inform the reader, and also provided an argument for why this factor (0.11 mg C mm-3) is chosen, as it is quite high compared to many other C-conversion factors used (Shatova et al. J. Plankton Res. (2012) doi: 10.1093/plankt/fbs053).

<u>REPLY</u>: Yes, the conversion factor we used is relatively high in comparison to published values. However, as stated by Urban-Rich et al. (1998, MarEcolProgSer,171), a constant conversion factor should not be used for calculating the carbon flux of fecal pellets. This statement was based on the extreme variability of the carbon content of fecal pellets spanning over 3 orders of magnitude, which reflects the complexity of the problem to determine a conversion factor. This is why we consider that arguments for choosing the factor are of rather formal than sound scientific character, but we can of course provide them. By the way, such arguments are rather scarcely reported in the scientific literature. Our modified text reads now as follows:

The dimensions of all pellets were determined with a semi-automated image analysis program, in order to calculate the form-specific volumes. To convert volumes into organic carbon we used a conversion factor of 0.11 mg C mm^{-3} (Carroll et al., 1998). Although higher than many values reported in the literature (e.g. González et al., 1994, González and Smetacek, 1994, Reigstad et al., 2005), this conversion factor represents organism and pellet diversity and especially, different density levels of the fecal pellets, which is mostly lacking elsewhere.

p 1256, l 5010 and 20-25: first (line 5-10) you argue that increased flux at depth may be due to resuspension. Further down (line 20-25), C/N ratios from traps (~ 7) are discussed and found to indicate presence of phyto-rather than zooplanktonic matter. Table 3 provide that C/N ratio of the deepest traps range 7-7.4 at all stations. This information, together with the fact that you argue for the importance of fecal pellets for vertical flux throughout the discussion chapter, is for me contradicting. It would be informative with a paragraph in the discussion chapter where this aspect is discussed, and maybe also with C/N ranges from the long time traps (if available), to compare the range of variability in this observation with the annual variability. To visualize and support this discussion, figure 4 lower panel, could show C/N ratio instead of PON flux.

<u>REPLY</u>: The comment on the C/N ratios (p.1256, line 20-23) was meant to be very general with respect to the values being above the Redfield ratio and referring to the reference presenting a very extensive dataset on global C/N ratios (Schneider et al., 2003). We agree that this statement was contradicting the more detailed discussions on the composition of the particulate matter. In fact, the comment was too general. We replaced the sentence by a more detailed statement directly related to C/N values of sediment trap particles and referring to the same reference. We also completed a paragraph in the discussion section as suggested by the referee. Finally, in figure 4, which is now figure 5, we replaced the PON flux bar graph with C/N ratios.

1) Section 3.2 (p.1256, line 21):

But the obtained ratios are above the Redfield ratio, a finding that was already reported by Schneider et *al. (2003) from a global data set of sediment trap samples (0-500 m). This study showed that the average C/N ratio (8.55, ± 3.58 (SD), n = 744) was significantly above the Redfield ratio despite the high variability of values in the data set.*

2) discussion section (p.1262, line 12):

We also do not expect any influence of the high C/N ratio (> 10) in river water particles (Emmerton et al., 2008) on our trap material, although the ratios we measured did not indicate any spatial trend. Such was the case in the traps of the long-term series (Forest et al., 2013), too. A study by Tamelander et al. (2013) in the European Arctic Ocean reported C/N ratios well above the Redfield ratio and suggested that the ratios varied according to new production, which depended on the nitrate availability and thus the trophic state of a given ecosystem. Other studies put forward the increase of the C/N ratio with depth (Copin-Montégut and Copin-Montégut, 1983; Schneider et al., 2003) related to preferential remineralisation of nitrogen. There is insufficient consistency in both the data of the present study and the data of the long-term study (Forest et al., 2013) to be able to relate them to any of the findings reported in those previous studies, but at least they confirmed that the C/N ratio of trap samples is above the classical Redfield ratio.

p 1257, l 0-10; maybe a paragraph in the discussion could elaborate if the high variability of the long-time traps showed some geographical patterns? They do represent a large area, most likely characterized by different ice and physical conditions, so variability is perhaps not surprising, but these aspects are not commented upon.

REPLY: Our goal was to put our data in a broader context given by the long-term trap series. This series showed that there are periods of low flux with relatively low variability across the sampling area and periods of relatively high flux with a high variability between the different traps and different years. This variability reflects mainly geographical differences, for which physical conditions play certainly an important role, but less so ice conditions since the peak periods are mainly ice-free. However there is no geographical pattern, which could be explained by e.g. upwelling nor other conditions. In order to be more precise about the different aspects of variability related to the long-term series we modified the last sentence of section 3.3 and completed correspondingly the discussion:

1) Section 3.3, last sentence:

However, the standard deviation associated with the values measured during the peak period in 2009 as well as in the other years, was particularly high at both 100 and 200 m depth.

2) discussion section (p.1261, lines 17-27):

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).

2) discussion section (p.1265, line 10-13):

Although late August 2009 was a period of elevated fluxes for that year, the high spatial and also temporal variability of particle fluxes (cf. Forest et al., 2013) does not exclude short periods of minimal flux and indicates the event-driven nature of the particle flux in this region, that is, most of the time-averaged flux is probably taking place during shortlived events that are easily missed by short-term sampling.

p 1258, l 0-12 and 1253 l 5-12; Are all fecal pellets (including fragments) counted, or only intact fecal pellets? From the text it is not clear, but no comments on fragmented pellets are made. Figure 6 b, do however show a fecal pellet fragment that I assume is included, enumerated and converted to carbon. Please specify.

REPLY: Fragments were included and this is now explained in material and methods and also in the results.

1) p.1253, line 8:

Pellet fragments, mostly cylindrical ones, were included in the counts.

2) p.1257, line 22:

The absolute numbers of cylindrical pellets but also their relative importance are slightly overestimated *owing to the fragmented pellets included in the counts. We accepted this bias in favour of the more important pellet carbon content, which is more precise when fragments are included.*

p 1259, l 19-21; this sentence is unclear, and I am not sure what you mean. Please clarify.

<u>REPLY</u>: The sentence was deleted and instead, the first two sentences reformulated more precisely:

At site 135, Chl a was still observed at very high concentrations except in the shallowest trap where it was not detectable. Fucoxanthin, however, was much more abundant than at the other sites and its concentration even slightly increased between 85 and 150 m.

p 1262, l 14; should "their" be "the"? reads strange as it is.

<u>REPLY</u>: We reformulated this sentence as follows:

Faecal pellets form a well distinct and sometimes major part of the sinking particles in the Beaufort Sea area (Forest et al., 2008; Juul-Pedersen et al., 2010).

p 1262, l 15-25; or can fecal pellet fragmentation be more prominent where feeding activities is higher and zooplankton abundances higher (more efficient retention of fecal pellets?) as you also discuss based on Honjo et al. 2010 p 1263, line 13. But see also J.T. Turner, Progress in Oceanography 130 (2015) 205–248), Svensen et al. MEPS 516:61-70 (2014) - doi:10.3354/meps10976).

<u>REPLY</u>: Yes, we agree that this process cannot be excluded as a possible explanation for the scarce amounts of pellets at this depth. We added this possibility to the discussion text:

It follows that, either grazing activity at this depth was low despite the relatively high food availability or the produced pellets were subject to enhanced coprophagy and/or coprohexy (Svensen et al., 2014), a process, which has already been reported from the nearby Baffin Bay (Sampei et al., 2004) and which we cannot exclude although we do not have direct evidence., or Another possibility for this observation could be that defecation from these grazers took place above the 40 m depth horizon.

p 1263, l 22; from Fig 6 and diameters indicated through the scale bars, I would assume that the fecal pellets origin from quite large zooplankton. Are your discussion of smaller copepods producing elliptical fecal pellets referring to pellets of similar size shown in fig 6c,d (d>100µm), or pellets of different diameter? Yoon et al. 2001 give pteropod fecal pellet diameter of 90µm (elliptical), and Wexels Riser et al. (2008) (Deep-Sea Res II 55: 2320-2329. doi: 10.1016/j.dsr2.2008.05.006) provide diameter of different zooplankton fecal pellets indicating orgin of pellets shown in fig. 6. E.g. 6a could match Calanus hyperboreus (d=94±), 6b be a euphausiid fecal pellet (d=131±), and 6d could be from appendicularian (d=257±). If the authors have information on the fecal pellets of Oncea that could support that this genus produce fecal pellets in the size shown at 6c that would give strength to the discussion of fecal pellet producers.

<u>REPLY</u>: The Oncaea issue was also commented upon by another referee and we did revise the whole part of the discussion. This was motivated by the misinterpretation of some of the copepod data from Forest et al, 2012, leading to the focusing solely on one genus, Oncaea. The discussion is now more generally including cyclopoid copepods and the genus Microcalanus, all of them being producers of the same type of elliptical fecal pellets. The modification or rather extension of the text does therefore not change in any way the findings of our study. It does however respond to the referee's comments on the size of the pellets. The pellets pictured in Fig. 6 (now Fig. 7) were chosen to represent much more the form and the fullness level (matter density inside the pellet membrane) than the size of the pellets in our samples. In order to avoid confusion, we reformulated the figure caption and especially, we reported average sizes of the pellets.

The new version of the paragraph now reads as follows:

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 (González 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 a small copepods genus, of the genera Oncaea, Triconia and Oithona spp., as one ofbeing among the most abundant copepods in the study area,. Together with the small calanoid copepod genus, Microcalanus, theyand which represented around 80close to 70 % in the size class < 1 mm (equivalent spherical diameter) 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.

Figure 7 caption:

Figure 7. Fecal pellets (a-d), fish eggs (e) and foraminifera (f) present in sediment traps. The two main morphotypes of pellets, cylindrical (produced by large calanoid copepods) and elliptical (produced by small cyclopoid copepods (c) and appendicularians (d), are shown at two different levels of fullness: cylindrical pellets (a: full, b: partly filled), elliptical pellets (c: full, d: partly filled). White bar is 100 µm.

p 1264; this is a long discussion based on very qualitative observations of fullness, and the discussion of a new producer like Oncea is important or if full appendicularian pellets are able to sink to deeper waters would be more substantial if a better description of Oncea pellet sizes could be provided and matched with sediment trap content. Not only the pellet shape.

<u>REPLY</u>: As we mentioned already in our reply of the preceding comment, we are now not only considering Oncaea but also other cyclopoid copepods in our discussion. Also appendicularian pellets are taken into consideration, although they were of minor importance among the elliptical pellets. Also the numerical aspect (average pellet sizes) is now considered, as already shown in the new text of the above reply. Concerning p 1264 and the discussion on the pellet fullness and shape, we added some considerations on the trophic conditions within the different depth horizons, which supports not only the observation of different types and fullness of fecal pellets at the different horizons, but gives a global image of the trophic state of the ecosystem at the study site. Most of the remaining text (p.1264 lines >10) was left as it is since the particular aspects mentioned by the referee in his comment were already taken into account in our preceding reply.

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 omnivourous 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.