

We would like to thank both reviewers for taking the time to examine our manuscript. We are happy to make the adjustments that both reviewers recommended, including the ordering of some paragraphs, grammatical errors and making edits to terminology and figures.

We have done most of the revisions required to the manuscript carefully considering each of the reviewer comments. We have revised the discussion part of the manuscript following the recommendation of both reviewers by adding a new paragraph introducing the discussion parts

Author comments:

We happy to revise our manuscript with respect to the following aspects:

Referee 1:

GENERAL COMMENTS

References seem to be quite focused on studies in the Mediterranean Sea. I am not enough of an expert on this topic to judge if this is valid or not. I included a suggestion for some references for other systems below.

Reply: It is true that most of our references are based in the Mediterranean, this is because our study area is in fact in the Mediterranean and easier to compare data closer our site than other places, although we have also include references in other regions of the ocean.

However, in the amended version we included also references from other oceanic systems.

SPECIFIC COMMENTS

18298, line 5: The meaning of “in diet” is not clear.

Reply: the concept has been derived from the paper published in Science in 1999 and entitled: Is the Deep Sea on a Diet? By Ellen R. M. Druffel, Bruce H. Robison, now to be clearer we changed this term into “nutritionally deprived”, i.e., characterized by limited nutritional resources

-18298, 8-24. This section of the introduction is quite focused on work done in the Med. It would be nice to include some references to other studies on biogeochemical changes in sinking organic matter in the ocean and/or degradation of OM in sediments. For example work by David Burdige, Stuart Wakeham, John Hedges, Cindy Lee, etc.

Reply: Only 3 of the references in these lines are from the Mediterranean sites, the other 7 are describing studies carried out in other ocean regions.

- 18300: “swimmers” is not a familiar term for all readers (and not very scientific).

Reply: The term “swimmers” referring to the zooplankton falling into the cups of the traps is largely utilized in the specific scientific literature (i.e. Buesseler et al. 2000; Lepore et al., 2009; Heusser et al., 1999; Karland and Knauer., 1989) but, considering the large audience of Biogeosciences with limited familiarity with this type of marine investigations, we are glad to

include a short explanation of this term like: “swimmers, including all large organisms that do not fall gravitationally through the water column, but enter actively to the trap).

-18300, line 17 :“(TMF)” after total mass flux.

Reply: Ok, done.

-18300, line 20: What does “116” mean?

Reply: It was a typo and as such eliminated

-18301: line 23: Make clear that pigment concentrations were converted to total phytoplankton-C concentrations. As written now, it may be interpreted as conversion to pigment concentration in carbon units.

Reply: Correct. The text has been modified as follows: “Extracts were analyzed fluorometrically to estimate chlorophyll-a, and, after acidification with 200 µl 0.1N HCl, to estimate phaeopigments.”

We indeed calculated total pigment concentration as the sum of chlorophyll-a and phaeopigments, which were then summed up and converted into C equivalents. The text has been modified as follows:

Chlorophyll-a and phaeopigment concentrations were summed up and reported as total phytopigment concentrations (Pusceddu et al. 2009). These were then converted into C equivalents using a conversion factor of 40 µgC · µg phytopigment⁻¹ (Witbaard et al., 2000; Van Oevelen et al., 2011). The algal contribution to BPC was calculated as the percentage of total phytopigments, once converted into C equivalents, to BPC.

-18303, section 3.1: This whole section and figure 2 seems quite a lot to just describe the environmental conditions, especially given that the manuscript is already quite lengthy. It would be more efficient and informative to focus on those aspects that are directly relevant for the interpretation of the main results from this study.

Reply: We agree. This section has been reduced and focused on the aspects directly relevant for the interpretation of the main results from this study.

-18304, lines 2-5: Here it says that some of the data are already presented in another manuscript that has not yet been published (“submitted”). I suggest clearly explaining the division of data and focusing between the two papers.

Reply: The other article is based on the main composition of particle fluxes (organic matter, carbonate, biogenic silica and aluminosilicates contents) and the total fluxes patterns during one year of investigations, from traps deployed at different water depths, inside the Blanes canyon (300, 900, 1200 and 1500 m) and in the open slope (900, 1200, 1500, and 1800 m) with a resolution of 15 days. This article is based on the drivers of particle fluxes in the bathyal zone and the effects of the weather in the particle fluxes. In other hand the present paper is focus on compare the bioavailability

of the organic matter from two traps at the same depth with a resolution of sampling interval in this study is of 7-8 days by trap sample, during all the year of the experiment with a total of 48 samples. This sampling strategy was proposed because at this depth is located the maximum of biomass, with the intention to meet a response to this special biomass-bathymetry pattern.

The text has been amended as follows: We have added “that paper is focus on the general total mass flux circulation and composition trying to understand the general drives of the fluxes”

-18304, line 10: *I do not see variation coefficients in Fig 3.*

Reply: variation coefficients have been added to table 1

-Sections 3.2 and 3.3. These sections contain quite some “dry” description of results that are already presented in the figures. There is no need to do this, especially given that this manuscript is already quite lengthy

Reply: Ok. We have reduced section 3.2, but kept intact section 3.3 as here are presented the most important insights of the study.

-Section 3.3: What I found missing is a figure showing the (changes in) relative composition of the OC pools. This is presented in the text in sections 3.3.1 and 3.3.2 but it would be nice to see it presented graphically. This could just be an additional panel in figures 4 and 5.

Reply: Ok. We have added two figures showing the (changes in) relative composition of the OC pools and reduced the text accordingly.

-Section 4.1: This rather long section may be split into two separate sections. First a general interpretation of results and then a section on the consequences for the animals.

Reply: We agree and the text has been modified accordingly.

18310: What I find missing here in the discussion of the quality of the OM is assessment of the composition of the material in terms of lipids versus carbohydrates versus proteins, and the related N content (versus C) of the OM.

Reply: We can understand the reviewer’s position, and what the ref suggests is indeed interesting, but since the aim of our study was to address it’s the OM bioavailability for the benthic consumers, we think that is more robust to focus directly on the BAOc and the direct estimates of OM bioavailability rather than providing indirect estimates. This is a point which somehow of relevance also for the Referee#2. Referee#2 was indeed concerned about the different origin of the material in the two contrasted stations, but, as also discussed below, we cannot reliably infer about the origin of the sinking OM using carbohydrate, protein and lipids.

-18313-14, lines 23-15: This section would be more at place at the start of the discussion because it really helps the basic interpretation of the results. This should be done before going deeper into the various aspects of the Discussion rather than at the end.

Reply: We agree and the text has been modified accordingly.

-Figure 1: The small globe is not very informative. A map of the Mediterranean Sea indicating the location of the canyon would be more useful. The locations of the two mooring stations are not very clear. A white text box with arrow (or similar) would be more clear.

Reply: Globe has been change to a clearer map of Europe, and the text for the mooring has been changed to clarify their position.

-Figure 2: Not sure if it is necessary to show all this information. It does not seem to be of key importance for interpretation of the results (also see comment to section of Results).

Reply: The Figure 2 has been modified to show just the current speed and direction

-Figure 3: Define % of what? is displayed (of TMF?).

Reply: Ok, done

-Figures 4 and 5: It would make more sense to merge these two into one figure as they show the same information for the two sampling sites. The yellow line and symbols for total lipids are difficult to see. Legends in the figures are not necessary.

Reply: figures 4 and 5 have been merged into a single one

-Figure 6, caption: Is there a word missing between “pairwise” and “of organic : :” ? Should “compound” be “contents” ?

Reply: Correct. Changed to: Pairwise test of organic matter fluxes and contents between the two morphologies at each sampling time/period and among sampling periods/times, separately for each seabed morphology.

Referee 2:

Major comments:

The two sampling site have a significantly different distance from the coast, I was wondering how much that influences the vertical flux and quality of organic matter to the deep sediment traps. This could be answered by comparing shallow sediment traps to the deep traps. Some sediment trap work was already published in 2012 by the first author, may be this information could be included in this manuscript to gauge the importance of surface fluxes to the deep sediment trap.

Reply: we thank the reviewer for this useful consideration, in the amended version we explored the relevance of this variable using a multiple regression analysis, which allowed us

to exclude that distance from the coast is a significant factor for the variable considered in the present study.

The analytical methods used in this study are somewhat dated and it would be essential for the manuscript to include some sort of comparison of their methods to the more. current methods of characterizing organic matter in the ocean (GC-MS, LC, NMR) otherwise the significance of this study is hard to judge. I bring this up because looking at the literature it seems that recent NMR work suggests a very different relative contribution of lipids, carbohydrates, and amino acids in deep ocean POM. The authors even reference that work (by Hedges et al. 2001) but don't comment on the differences.

Reply: the methods utilized are still widely used. Although we recognize that an in depth analysis of the lipid composition or DFAA could be useful, we have a large number of samples, which are difficult to investigate all in detail. Moreover, here we have the advantage of having the biochemical methods already set up specifically for the kind of samples taken (sediment traps) and previous data that enable a comparison. In addition, while the method for the determination of the bulk of lipids, proteins and carbohydrates are dated, the use of enzymatic digestions to mimic/simulate the bioavailability of the organic matter by benthic consumers is very advanced and has the advantage of looking at the organic pools not only from a chemical point of view rather in a biological and ecological perspective. To our knowledge this approach is the best in this specific perspective and recent works (2009 and 2010) we cited can possibly confirm that the methods we used are not dated. Thus, concluding, we are perfectly aware that the methods we used in this study can provide different results than those based on different methodologies, but still we have the chance to making reliable comparisons among data obtained in the past. Nevertheless, a cautionary sentence dealing with the different estimate that are possibly obtained using other methodologies has been included in the amended version of the manuscript, specifically referring to the results reported by Hedges et al 2001.

Another important paper in this respect is the paper by Wakeham et al. 1997. This one includes organic matter composition data for sediment traps and surface sediments.

We agree that the reference is important and indeed it has been cited in the amended version of the ms.

Without offering a plausible reason for the differences in organic matter composition of deep sea environments it is hard to put the findings of this study in context.

Reply: We can agree with the reviewer but in the early study we did not consider labile pools, so that comparisons can be reliably made only on TMF or OC fluxes. These information was indeed provided in the early version of the manuscript. Nevertheless, we have added to the manuscript the information reported below:

Overall, The presence of canyons drastically alters the regional bathymetry by reducing the distance between terrestrial sediment sources and the shelf break, introducing steep slopes closer to the shoreline and intersecting the along-shelf sediment transport system

(Mullenbach et al., 2004). Thus, submarine canyons may capture inputs delivered through the river mouths, littoral drift sediments and sediments from relict shelf bodies (Lastras et al., 2011). Such striking difference has been already described for the Blanes canyon, where the TMF continuously increases from the head of the canyon at 300 m depth until 1500 m depth, unlike in the adjacent open slope where the TMF decrease with the distance to coast (Lopez-Fernandez et al., submitted). Clearly there is a difference between the flows in the two sampling stations due to the location and distance of the "shelf-break." The station OS 1200 situated at approximately 20 km to the 200m isobaths, presents on average a TMF of 2380.98 mg m⁻² d⁻¹ with OC and opal fluxes on average of 40.57 and 49.87 mg m⁻² d⁻¹, respectively (1.70 and 2.09 % of the TMF, respectively). In the other hand this fluxes in the station BC1200 situated around 10 km to the 200m isobaths, presents higher values, 22898.17, 240.57 and 269.63 for the TMF, OC and opal fluxes, and the OC and opal contribution to the TMF were lesser (1.05 and 1.18 % respectively).

We can also understand the criticism raised by the referee#2 about missing explanations of the observed differences. Indeed, the results we provided in our study have been almost exclusively related to the potential hydrodynamic control of the bioavailability and composition of organic fluxes. Consequently, we provide in the discussion some more inferential factors that could co-act with hydrodynamics in controlling the differential composition of OM fluxes in the canyon and the open slope.

Minor comments:

18309 13-15: Lignin phenols are not carbohydrates, and carbohydrates are not the most refractory fraction of organic matter in the ocean based on most peer reviewed literature

Reply: Referee is right Lignin phenols are not carbohydrates we have deleted that sentence and we have specified: "Carbohydrates represent the most refractory fraction of organic compounds analyzed in this study".

Concluding, we think we considered carefully all suggestions made by the referees and made all changes requested. We hope that this amended version will be now considered suitable for publication and remain available for any other request that the Editor in charge might have.

Looking forward to hearing from you, we remain,

Sincerely yours

Pilar Lopez-Fernandez

on behalf of all co-authors