

***Interactive comment on* “Trophic state of sediments from two deep continental margins off Iberia: a biomimetic approach” by A. Dell’Anno et al.**

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We would like to thank the Referee#1 for comments and suggestions that allowed to clarify some critical points of the abstract and the discussion, thus improving the overall quality of the manuscript. Major critical points raised by the Referee#1 dealt with: i) some statements of the previous version of the discussion that were not completely supported by the data presented; ii) a number of key references on the Nazarè canyon that were ignored; iii) the high level of self-citation. All of these points were kept into consideration in the preparation of the amended version of the manuscript. Below we report point-by-point responses to the Referee#1 comments.

#1 Specifically, on p. 17633 the authors suggest that their results indicate circulation pathways and episodic events at the Catalan Margin fuel benthic systems with bioavailable N-rich compounds. My question is how? Hydrolysable protein concentrations to bioavailable C (Fig 4C) seem to me to be very similar in slope and canyon sediments. If this is a measure of bioavailable N-compounds, then how does it relate to episodic events? Of course we know that such events occur in the Gulf of Lions, but the data presented here really cannot be linked to any of them.

We thank Referee#1 for having questioned about this issue, which likely was not adequately addressed. We argue that the criticism of the Referee arises by the fact that in the previous version of the manuscript we did not sufficiently explain how episodic events could influence the quantity and distribution of organic matter in the sediments of the Catalan margin. In the amended version of the manuscript we thus provide additional insights supporting our results, which are based on available information synoptically acquired during our study and published elsewhere. In particular, we now report that previous temporal investigations carried out by mooring trap arrays deployed at ca. 1000 m and 1900 m depth exactly in our study stations provided evidence that material settling during cascading events occurring in winter/early spring 2005 and 2006 (a few months before our sampling) represent ca. 75% of the total annual OM flux and that such flux was quite similar in the canyon and in the adjacent open slope at 1000 m depth (Sanchez-Vidal et al., 2008 and 2009; Pasqual et al., 2010). During or just after the cascading similar fluxes were recorded both at the slope and canyon stations located at 1900 m depth. Such large inputs of organic material were represented not only by organic particles of marine origin (due to the phytoplankton bloom occurring concomitantly on the shelf), but also by huge amounts of organic particles of terrestrial origin (i.e. mainly from the Rhone river) resuspended on the shelf and transported down canyon and down slope (Sanchez-Vidal et al., 2008 and 2009; Pasqual et al., 2010). This material, using other proxies (i.e. stable isotope and biochemical analyses), has been indicated to be relatively labile (Sanchez-Vidal et al., 2008, 2009). Thus, our results are confirmatory of previous results and the presence of similar values of

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hydrolysable protein concentrations to bioavailable C pool (Fig. 4C) in the sediments of the canyon and adjacent open slope are therefore not surprising. In the amended version of the manuscript we better clarified this issue.

#2 As far as the Nazare canyon is concerned, the evidence for transport of bioavailable OM to deep in the canyon is stronger, but the discussion is obscured by the grand statement that “this may have profound implications on the quantity and distribution of bioavailable C pools on the sea floor”. How? Why?

We agree with the Referee#1 that our findings moved a little bit farther than the presented evidences. The lack of significant differences in bioavailable C concentrations with increasing water depth in the sediments of both investigated canyons and adjacent open slopes indeed contrasts with the general expectation of a significant decrease of organic matter associated to the vertical particle fluxes with increasing water depth. Thus, we could infer that the lateral transport of organic particles already documented in the two study areas appears a major process influencing the distribution of organic matter including the bioavailable fraction on the seafloor. Consequently, according to the Referee#1 suggestion, we modified and softened the text as follows: “These results suggest that the lateral transport of organic particles from shallower to bathyal depths, even amplified by episodic events, i.e., gravity flows in the Nazare canyon and DSWC in the Catalan margin (Heussner et al., 2006; Canals et al., 2006; Palanques et al., 2006; de Stigter et al., 2007; Arzola et al., 2008), may have important implications on the quantity and distribution of bioavailable C pools on the seafloor”.

#3 Finally, fluxes are never mentioned, yet ultimately, fluxes of bioavailable OM must be the key in understanding the benthic environments described, not concentrations.

The Referee is right that fluxes of bioavailable OM should be considered for a better understanding of the mechanisms influencing the quantity and distribution of bioavailable organic matter in the sediments. However, it should be considered that the benthic ecosystems integrate processes occurring at different temporal scales (depending on

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the sedimentation rates), which are not only dependent upon the vertical supply of organic particles but also upon advection processes and rebound of organic particles from resuspension which can be largely underestimated by sediment trap investigations. Data in the sediment also integrate to a certain extent the biological response to OM deposition (e.g. Gooday, 1988; Smith et al., 2008). Moreover, the nutrition of the benthic metazoan assemblages, mostly represented by deposit feeders in the deep sea, does not rely on the particle flux per se, but on the amount and bioavailability of organic matter deposited on the seafloor (Amaro et al., 2010). In the amended version of the manuscript more details on particle fluxes have been included.

#4 While it may well be true that there is a relationship between OM flux and macrofaunal abundance (biomass must be a better measure!), the environment at 940 m is very different to that at 3200 m in the Nazare canyon, in other words, other factors are also likely to play a role in controlling macrofaunal abundance.

We are aware that other factors (e.g. physical disturbance, habitat heterogeneity as already documented in the literature), besides food availability, can influence the distribution of macrofaunal abundance and biomass in the Nazarè canyon (and other canyons worldwide). Indeed in the previous version of the manuscript we just reported that the bioavailable organic detritus within the Nazarè canyon can contribute to explain the distribution patterns of macrofaunal abundances and that it doesn't represent the only causative agent. Other possible factors have been also cited in the amended version of the manuscript (along with some pertinent references).

#5 A number of key references on the Nazare canyon are ignored, for example those dealing with meiofauna, physical setting, carbon burial and organic geochemistry. The authors should refer to Deep-Sea Research II.

We thank the Referee#1 for the suggestion. All the relevant citations are included in the amended version of the manuscript.

#6 The level of self-citation throughout is unusually high and frankly detracts from the

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quality of the manuscript.

In principle, we do not fully agree with the Referee#1 position. Self-citations cannot per se affect the quality of a manuscript. We feel, rather, that self-citations, if pertinent, give an added value to a manuscript as they help the reader to allocate the study in a broader context of investigations carried out by the researchers team, whatever the topic covered. However, to meet the Referee#1 position, in the amended version of the manuscript we reduced as much as possible self-citations.

Specific points 1. The title of the paper is ambiguous. What does trophic status mean? I think nutritional is a better term, because “trophic” is usually associated with the biology, rather than the sediments themselves.

The use of the term “trophic” for the definition of the “nutritional” state of a certain ecosystem is a matter of debate since the end of the nineties of the last century (see Nixon, 1995). In the last 20 years, however, also thanks to an increasingly accumulation of research evidences, it has been clarified that the (nutritional) response of benthic consumers to organic matter supply is influenced more by organic matter quality (e.g. bioavailability) rather than by bulk concentrations (Cebrian et al. 1998; Huxel 1999; Van Oevelen et al. 2012). Thus, the assessment of the trophic status of benthic marine ecosystems (being ‘trophic’ a term that is translated from the classification and assessment of eutrophication in the water column) is now carried out also using indicators of the quantity and bioavailability of all (including detrital) nutritional resources (Grall and Chauvaud, 2002). These aspects appear to be also relevant not only in shallow benthic systems, but also in deep-sea ecosystems, where in situ primary production (generally used as a proxy of trophic state in coastal ecosystems) is not present (Pusceddu et al., 2009; Pusceddu et al., 2010). Consequently, we retain that the use of the term “trophic state’ in the title fits with the scope of our study and reflects the content of the manuscript. However, to partially meet the Referee#1 position, and also according to the Referee#2 comment (see below), in the amended of the manuscript, the title now reads: “Trophic state of benthic deep-sea ecosystems from two different

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continental margins off Iberia”.

2. Page 17620, line 21. “relatively high amounts of bioavailable organic matter” Relative to what? Other deep-sea sediments. The final sentence is not justified by the data presented, which cannot on their own be used to describe processes.

Correct. Accordingly, in the amended version of the manuscript we specify that the benthic deep-sea ecosystems investigated in this study were characterized by relatively high amounts of bioavailable organic matter when compared to other deep-sea sediments. According to the Referee#1 suggestions we smoothed the concept, and the final sentence of the abstract has been modified as follows: “Overall, our findings suggest that the intensity of primary production processes along with the lateral transfer of organic particles, even amplified by episodic events, can have a role in controlling the quantity and distribution of bioavailable organic detritus and its nutritional value along these continental margin ecosystems”.

3. Page 17622, line 29. “Benthic trophodynamics” is a pretty opaque term. Who eats what and how quickly? I would argue that on its own, the data presented here cannot be used to unravel these questions.

We agree. We partially re-shaped this part by deleting the implications on benthic trophodynamics and by better specifying the differences in primary productivity between the two contrasted continental margins (as also requested by Referee#2).

4. Page 17623, line 8. Does freezing of the sediments influence the measurement of enzymatically hydrolysable OM? In other words does it lead to cell lysis of sedimentary bacteria? Perhaps more significantly particularly for the Nazare canyon sediments where sedimentary meio and macrofaunal abundance and biomass can be quite high, do they contribute to the bioavailable pool of OM? These are not necessarily easy questions to answer, but I think that they are important.

We thank the Referee#1 for comments on this issue that we indeed considered in

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the previous version of the manuscript. The issues raised by the Referee#1 have been faced in previous studies we carried out in different marine ecosystems including the deep sea (Dell'Anno et al. 2000; Danovaro et al., 2001; Pusceddu et al., 2003). These studies revealed that even if all the prokaryotic biomass would be lysed by sediment freezing its contribution to the enzymatically hydrolysable OM would be negligible. Moreover, additional experiments we carried out by adding marine bacterial strains to fresh sediments before freezing demonstrated the lack of significant bacterial lysis induced by sample storage (Corinaldesi et al., 2008, in *Molecular Ecology*). To meet the previously commented issue raised by Referee#1, we avoided including an additional citation from our team. As far as the contribution to the enzymatically hydrolysable OM pool deriving from metazoans, we retain that the contribution of such component is also negligible since the majority of the benthic biomass in the deep sea is consistently represented by prokaryotes (up to 90%) and because larger metazoans, when present, were removed from the samples before the analysis.

5. Page 17625, line 21. “Muffled” is not a verb. Use “: : heated in a muffle furnace”.

The term muffled has been changed accordingly.

6. Page 17627, line 26. Add in a source reference for the statistics. In general the term “significance” is not used consistently through the manuscript, in that sometimes it is justified with a P value, or relevant test, but often is not.

In the amended version of the manuscript a reference for the statistical package and methods used has been added (Anderson et al., 2008). All of the outputs of the statistical analyses are reported in the Supplementary materials, so that no further indications in the main text are necessary. This allows keeping the reading easier.

Interactive comment on Biogeosciences Discuss., 9, 17619, 2012.