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Biogeosciences Discussions

Interactive comment on "Food quality determines sediment community responses to marine vs. terrigenous organic matter in a submarine canyon" *by* W. R. Hunter et al.

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This paper by Humter et al provides some of the clearest methodological approaches and narratives that I've reviewed in the last years. The authors used stable isotope labeling experiments to investigate the in-situ response of benthic communities with special emphasis on macrofauna and bacteria assemblages. An interesting conclusion emerging from the paper is the effect of the disturbance driven by faunal grazing on the bacterial consumption of organic matter. Particularly interesting is the short time scale when it occurs. Although this study and the relative conclusions would have benefited from a higher number of samples, this study shows the right direction for future in-situ

microcosm investigations of marine ecosystems. I have only one major doubt about the terrigenous OC used in the experiment (see my following comments) that was probably not completely representative of the terrigenous material that accumulates in deep sea sediments. However, this work has large implications as it demonstrates the role of short time-scale episodic deposition on macrofauna and bacteria assemblages in deep sediments. I believe that this study provides an interesting view of deep sea ecosystems, justifying its acceptance and publication in BG. I thus recommend that it be accepted with minor revisions. Please find my comments below:

1) The material that reaches deep sediments is everything but matrix-free terrigenous material as the one used in this study (i.e., Triticum aestivum). The only regions in the ocean where you have significant contribution of plant detritus are probably shallow deltaic environments. By contrast, the terrigenous material in deep sea sediments is exclusively associated and protected by the mineral matrix forming organo-mineral complexes. Also terrigenous OC that accumulates along slopes is extensively aged and degraded (see for example the difference in composition between lateral advection and passive sinking of marine phytodetritus in the Adriatic submarine canyons, Tesi et al, 2008. Deep Sea Research I 55 (2008) 813– 831). Therefore I am expecting that the "real" land-derived material is even less reactive than the terrestrial phytodetritus used in the study. The authors should be aware of this and maybe it would be appropriate to state this somewhere in the text. Indeed, it would be interesting to see if by using soil OC the conclusions would be the same. Considering the low reactivity of matrix-protected OC maybe the difference in phytodetritus use would be even more pronounced.

2) I think the methods can be improved by providing a brief explanation for each index used. In the methods the authors presented only formulas that I found it a bit sparse on details. However, the reader would benefit from a few comments about what high and low values means and what kind of information a certain index, say I-bacteria, can give you.

BGD

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Interactive Comment

3) Fig 1. Those dots are a way too small, so the labels

4) Fig 2. It would be interesting to see the downcore profiles of nitrogen and carbon stable isotopes next to or on top of these plots

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Interactive comment on Biogeosciences Discuss., 9, 11331, 2012.