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Biogeosciences Discussions

Interactive comment on "Microbial bioavailability regulates organic matter preservation in marine sediments" *by* K. A. Koho et al.

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This manuscript reports interesting results from experiments designed to investigate organic matter degradation dynamics in sediments from the Arabian Sea. The authors have found an interesting de-coupling of degradability from conventional measures of organic matter composition/quality, and this merits publication. I feel however that some aspects of the manuscript need to be revised before publication, to ensure that the mechanisms behind the observed trends are fully explored and discussed.

Main Comments

Introduction. The introduction provides a good review of the appropriate literature, but does not really identify a research gap, or any research questions. It would be helpful

if an explanation was added as to what motivated the study. The last section of the introduction tells the reader what work was done, but could also summarise the main finding. Section 3.2. I feel that the main issue here is the question of the extent to which the fact that all incubations were conducted under oxic conditions has affected the results. Presumably for oxic degradation to have occurred in sediments from the OMZ a new microbial community had to develop first. I realise that you have already acknowledged this point; however I feel that it warrants further discussion in light of the literature. For example, what do we know about whether oxic microbes are even present in OMZ sediments, and how long would it take them to develop into a fully functional community? How are the duration and conditions of storage of samples before incubation likely to have affected the microbial communities in samples from both within and outside the OMZ? I feel that the paper currently does not really come to a firm conclusion; therefore some might say that it is not clear what the central finding or idea is. The lack of correlation between biochemical quality and microbial degradability is intriguing and worthy of publication, however I feel that it requires considerable further discussion. It would be best if that discussion yielded a suggestion from you as to which of the mechanisms you discussed are actually controlling degradability. This discussion should include acknowledgement of the fact that previous degradation experiments have found that biochemical quality or freshness was linked to degradability or half life. The sections on macrofaunal populations and bioturbation (page 12 onwards) do not currently seem to serve a purpose. This is particularly true for the bioturbation section, which seems to report your mixing data without relating this back to the central question of how it might have affected degradability.

Other Comments

Introduction paragraph 1. It would be worth mentioning here that a significant number of studies have found oxygen concentration not to be a primary control on OM degradation/preservation. Canfield (1994) produced the best resolution of the two sides of the argument, with his emphasis on the importance of oxygen exposure time. This

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is particularly worth mentioning because at the start of section 3.3 you also appear to suggest that straightforward oxygen availability is not necessarily the main factor driving OM accumulation in OMZs. Methods. Make sure that OMZ is defined the first time it is used. I suggest additional proof reading, as there are several locations where words such as 'a' and 'the' have been missed out (e.g. page 5 line 10 'we performed series of sediment incubations...'). Section 2.1. I feel that the discussion of whether microbial communities will have changed due to experiments being conducted under oxic conditions should also acknowledge that communities will have altered during sample storage for 2 months. It would be helpful to know what conditions the samples were stored in (oxic or anoxic?). Page 5 last line: Please correct units in the phrase 'per wet sediment'. Methods: I feel as though I need a little more detail on how the sediment incubations were carried out, e.g. how may replicate incubations per site, what volume of sediment was used, and how were oxic conditions maintained? Section 2.2. I suggest including a little more detail here on THAA analysis, such as the fact that you produced acid hydrolysates from sediment samples and analysed them by HPLC. The same applies to PLFA analysis. Methods: I would suggest that some phaeopigments seem to have half lives of thousands of years (see reference in Woulds and Cowie 2009), which sheds doubt on your statement that downcore penetration of phaeopigments could only occur due to faunal mixing. Please state how pigment inventories were calculated. C accumulation rates. Please state how %Corg values were measured. I would also like to see further justification of the use of Corg data from the top three cm for calculating burial rates. I can see your point within the OMZ (although readers who have not worked in OMZs might not be able to take your point as read), but I remain sceptical that your approach is valid below the OMZ at oxic sites. I suggest that you describe the maximum downcore decrease in %Corg that you saw at an oxic site, and demonstrate how much difference it would actually make to your estimations of C burial if you used %Corg data from say 20 cm instead of 0-3 cm. Results. You could try referring to your stations by their depths, rather than by numbers (which although logical are still arbitrary). This might help your reader keep track of which site

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is which (although this is only a suggestion, as it is simply the system I am used to using). Page 9, paragraph 1: I think the relationships referred to as correlations here are actually regressions, as R2 values are stated. Please correct/clarify this. Please also state the p values for these relationships to show that they are statistically significant. Figure 3. Please add an explanation of what the closed and open circles in panel G mean. Section 3.2. The two most oxygenated sites seem to show bacterial biomass considerably lower than any other values. I feel this should be acknowledged in this section (even though it probably doesn't make a difference to your data interpretation). Page 11 line 25: Please further explain what you mean by '...macrofauna may provide catalyzers for microbial degradation...' Page 12 line 25: 'inferred' rather than'induced'.

Interactive comment on Biogeosciences Discuss., 9, 13187, 2012.

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