

## ***Interactive comment on “Assessing the biodegradability of terrestrially-derived organic matter in Scottish sea loch sediments” by P. S. Loh et al.***

### **Anonymous Referee #3**

Received and published: 28 January 2008

General comment: The ms. by Loh et al. investigates the role of terrestrial organic matter (OM) on the biogeochemical cycling in two Scottish sea lochs, with a particular emphasis on its biodegradability. If this subject is of broad scientific interest and is suitable for the readership of HESS, I find that the current interpretation of the dataset (partly published in a previous ms?) is very qualitative and lacks clarity. Sources and processes affecting OM are identified, but their relative importance for OM cycling remains unclear. I am thus recommending to provide more quantitative information about the origin and fate of OM in sea loch environments:

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1) Biodegradability of OM and contribution of terrestrial OM: The use of oxygen uptake rates as a measure of OM degradation is standard in the field of (marine) sediment biogeochemistry and is, thus, not novel. In addition, possible shortcomings of this approach have long been identified (a significant fraction of the oxygen consumption can be channeled in the re-oxidation of reduced products) and the authors currently do not provide any strong, comprehensive, evidence that these alternative oxygen-consuming pathways (from all possible reduced redox species) are negligible in their system. The authors claim instead that the variability in uptake rates is due to the amount of (terrestrial) OM. However, this seems to be contradicted by the absence of obvious relationship between oxygen uptake and % labile OM. The reactivity (instead of content) of this OM pool, and how it is related to the origin of OM, is however not discussed in detail. To broaden the scope of this ms., the values of oxygen consumption, OM contents and reactivity in the present study should also be compared with values from other terrestrial and marine ecosystems. Finally, an attempt at quantifying the contribution of the terrestrial OM to the overall biogeochemical cycling (a very vague objective identified by the authors &#8211; I would limit the scope to the dynamics of the OM pool only) should also be provided.

2) Combination of proxies as indicator of sediment biodegradability: The oxygen uptake rates,  $R_p$  and C/N ratios are each individually relatively simple indicators of OM degradability (with interpretative limitations for the oxygen uptake rates and the C/N ratios). The authors should however try to combine the information from the indicators to analyze, in quantitative terms, the fate of OM in the sea lochs: What is the contribution from the various terrestrial OM sources, the marine OM, how much is deposited, how much is decomposed during transport, etc... . In particular, no information is provided from the marine component of OM. As it stands, the analysis remains purely qualitative (f.i. stating that terrestrial OM dominates landwards while marine OM increases in importance seawards seems obvious to me). Establishing such tentative budget on the basis of the available information would improve the clarity of the text and provide a synthetic view of OM cycling in sea lochs.

3) The results about the lignin contents and behavior are interesting.

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However, after reading through the ms., the fate of this specific compound (especially in terms of in-situ degradation) still remains unclear (see below).

Specific comments:

Line 23 p 4006: Meaning of 'in the complexity of chemicals'; Line 8 p 4007: Statement unclear: is it meant OM degradation by the aerobic pathway, the oxygen being supplied by the activity of burrowing organisms? Line 10 p4007: I do not agree with this statement: many studies in the field of early diagenesis have relied on benthic chamber experiments or on direct, in-situ rate measurements (e.g. using isotope tracers) to relate these rates to the quantity and quality of OM. Line 24 -30 p 4007: This information is too technical for an introduction. Line 12 p 4008: ... the study showed ....: Which study? Line 15 p4008: The term 'sediment biodegradability'; is not very appropriate: I would prefer OM biodegradability Line 17-19 p 4008: Clarify this sentence. Line 19 p 4008: There are numerous observations of increasing C/N ratio during early diagenesis (as stated by the authors just above). The relationship between decreasing C/N ratios and diagenesis is thus misleading. Line 23 p 4008: to determine the potential for terrestrial OM: this is not a clear objective and it should rather be 'to determine the contribution of terrestrial OM'; (see general comments, above). Material and methods, p 4009: Mean freshwater inputs are exactly similar for the two lochs? Why not report the residence for both lochs? It is important to have a simple measure of the transport timescale in the two systems. No information is provided about the fate of solid particles in the sea lochs. This is relevant in the context of the present study. Line 12, p 4011: What are the implications of the chosen timescale for the oxygen uptake rate experiments (24 hours is a very short period)? On the basis of these rates and labile OM contents, could any kinetic rate constant for OM degradation be extracted? Isotope analyses p4013 & 4015: The isotope measurements are not used at all to support any argument in the discussion. They should only remain in the other sections if they are used in this context. Yields results p 4014: Are these results already discussed in Loh et al., 2007 (ref indicated

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in Table 2)? Line 17-19 p4014: The two sentences state similar facts. Line 21 p4014: Value for LC6 should read 0.45 (and not 0.54). For both sea lochs, these values show rather two groups of values rather than a decreasing trend. Line 24 p4014: range from 0.70 to 0.71. Please rephrase. Oxygen uptake rates, p 4015: The differences at LC1 seem minor to me (rather than significant). At LC1, the seasonal variability is attributed to the microbial degradation of fresh terrestrial OM. This does not explain why there are seasonal variations: Is it due to temperature effects that stimulate OM decomposition during the summer months or is it due to the sudden input of easily degradable OM? I would suspect that even if the OM material remains the same, the degradation rate would increase due to the physiological effect of temperature on bacterial activity. But then, how would you explain the higher rates in December at LC5 when temperature is lowest? Something should also be said about the small scale sampling heterogeneities. Line 2 p4016: no significant seasonal trend .... (add seasonal) Line 5-6 p4016: This qualitative statement is obvious to me: in an open system any chemical compound will be subject to reaction and transport. What about sedimentation (another form of transport)? Line 25-27 p4016: Clarify this statement. Sources of terrestrial OM p4017: What could be the other sources of terrestrial OM to the lochs? Without clear identification of alternative sources, the statement about the origin of terrestrial OM seems trivial. What are the source and composition of the non-lignin material (marine OM only?). In addition, I recommend plotting results of the correlation analyses Line 1 p4017: If this is true between Camas and RE6, why is there no decrease between RE2 and RE6, two stations which are also far apart. Line 20 p4018 ... than at other locations ... Oxygen uptake rates p 4019: The seasonal variation is not apparent at LC5. What about the potential role of other reduced species in oxygen consumption (e.g. ammonium)? Especially, the role of sulfate-reduction will only be significant in brackish areas and it is unclear to me how the marine water masses protrude into the lochs. The role of terrestrial OM should be quantified and I am not convinced that the oxygen uptakes can simply be related to OM contents, without addressing specifically the problem of its reactivity (see general comments). Line 21-26 p4020: Does

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this imply that the fate of lignin is only due to dilution and sedimentation and not to degradation (line 26)? Also, because all concentrations are reported with respect to the solid phase, undifferentiated sedimentation should have no effect on the lignin or OM contents. If possible, it is these various factors (dilution, preferential sedimentation, degradation) that should be quantified (see general comments). Line 1-5p 4021: This is a very vague statement Molar OC/N ratios p 4021: How can the effects of OM aging and sources be distinguished and quantified? Line 3 p 4023: ... terrestrial compounds constitute a significant fraction of the OM pool.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 4005, 2007.

**HESD**

4, S2117–S2121, 2008

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