

Associate Editor Decision: Publish subject to minor revisions (Editor review) (07 Jan 2017) by Dr. Silvio Pantoja

Comments to the Author:

7 January 2017

Review 2

Dear Dr. Hyun,

Thanks for your responses.

The comment below:

1) Reviewer 1: Question 4) Line 606: “The statement about the probable importance of bioturbation seems to be in contradiction with the well-defined utilization of the electron acceptors according to the order of decreasing energy yield for organic C oxidation that has been underscored in lines 412-417? Again, I suggest clarifying this point.”

Reviewer 1 asked clarification to the following: There is a clear biogeochemical zonation in these sediments (lines 412-417) and your response agreed with that, but still in line 606 it says “Thus, it is realistic that bioturbation drives Mn cycling in the UB. ”. To me is contradictory with lines 412-417 as well, unless you meant something else. Please clarify that and proceed accordingly in the revised version.

Refers to this:

If there is zonation, then bioturbation is not high enough otherwise biochemical zonation would disappear (or masked). This is the contradiction raised by Reviewer 1, which is not clarified in your response since the statement “Thus, it is realistic that ^[L]_[SEP] bioturbation drives Mn cycling in the UB” is still there.

Please revise accordingly

Sincerely yours
Silvio Pantoja
Associate Editor

Response to the 2nd comment by associated editor on bg-2016-222 (07 Jan 2017)

Dear Dr. Pantoja:

Thank you again for your comments on our revision. We had addressed the concerns about bioturbation vs. distinct chemical zonation in a new paragraph right after the statement about bioturbation as a realistic mechanism. However, from your comment and from when reading the text again, I realize that format of previous version could be confusing. According to your suggestion, we have revised the line 624 – 634 in previous version as follows: “This value is 3.6 times lower than the coefficient estimated for the Skagerrak (Canfield et al., 1993b) and consistent with estimates for other sediments with similar deposition rates (Boudreau, 1994). The estimated biodiffusion coefficient (D_b) of $9.5 \text{ cm}^2 \text{ yr}^{-1}$ at Site D3 corresponds to ~2 % of the molecular diffusion coefficient of oxygen ($388 \text{ cm}^2 \text{ yr}^{-1}$). Judging from the absence of major fauna in the UB sediments, the mixing is brought about by small organisms with each individual affecting only a small area relative to the size of our cores, and the D_b averaging many of these small and local but frequent events. Under such conditions, bioturbation can drive Mn cycling in the UB without substantial smearing of the redox zonation. Similarly, Hyacinthe et al. (2001) found that well defined profiles can be observed in both sediments with low and high bioactivity in the Bay of Biscay.”

Finally, I sincerely hope this revised version is acceptable for you. Thank you again for your precious time to improve the quality of this manuscript.

Best regards,

Jung-Ho Hyun