Associate Editor Decision: Publish subject to minor revisions (Editor review)

(23 Oct 2016) by Dr. Silvio Pantoja
Comments to the Author:
October 23, 2016

Dear Dr. Hyun,

Thanks for providing responses to three Reviewers of your BG discussion paper (bg-2016-222). I would like to invite you to submit a revised version of the article based on your responses, and considering the following issues:

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.

(Response): To clarify the systematic zonation of the electron acceptor at D3 where bioturbation derives Mn cycling, we have added a paragraph in line 628 – 635 as follows: "Meantime, the estimated biodiffusion coefficient of (Db) of 9.5 cm^2 yr^{-1} at Site D3 corresponds to ~2% of the molecular diffusion coefficient of oxygen (388 cm^2 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 Db averaging many of these small but frequent events. Therefore, we see no contradiction between the presence of bioturbation and the relatively distinct redox zonation at D3 (Fig. 5F). 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."

2) Reviewer 1: Minor 4) Line 276: I suspect that the units (ml/g) are erroneous?
(Response): "It is presented in Thamdrup et al (2000). "
Something being published cannot be a proper response to a colleague reviewer. What are unit ml/g of?

(Response): I am sorry for the inappropriate response. I was even wrong in citing the reference by stating Thamdrup et al. (2000). It was explained in Canfied et al. (1993b) and Thamdrup and Dalsgaard (2000). Here is our response. If you see the following figure (the Fig. 7 in Canfield et al. 1993b, GCA), the unit is derived from the slope of Mn

adsorption experiments (= μmol g^{-1} / μM = 10^3 ml g^{-1}) in the Skagerrak. Those references (Canfield et al., 1993b; Thamdrup and Dalsgaard, 2000) are listed in the line 282 in the revised manuscript.

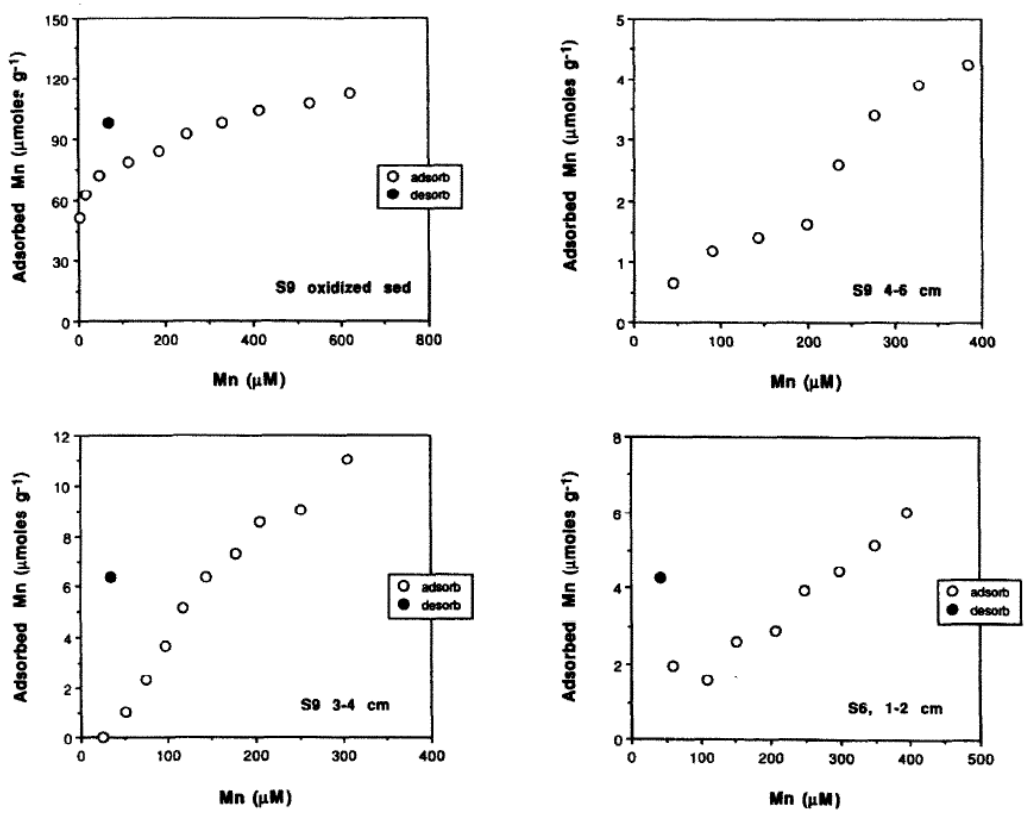

FIG. 7. Figure shows results from Mn adsorption experiments for sediment from various depths at S_9, and 1–2 cm at S_6. Included also are results from desorption experiments (see text).

3) Reviewer 2." 40) L. 638: I still do not understand why the Ulleung Basin is a "biogeochemical hotspot"? Is it because organic matter mineralization is dominated by metal reduction? This is not clear at all and I would therefore suggest to better explain or to delete this.

(Response): It is not because organic carbon mineralization is dominated by metal reduction. The reason that we stated the UB as a biogeochemical hot spot is that the overall organic carbon oxidation in the UB is higher than those measured in major up-welling system such as Benguela upwelling system and is even comparable to those reported at the continental slope of the Chilean upwelling system at a similar depth range of 1000 – 2500 m. Please see the line 639 – 661 in the revised manuscript. "

Please demonstrate that Ulleung Basin is a "biogeochemical hotspot" showing numbers to compare in the revised version.

(Response): To demonstrate the UB as a biogeochemical hotspot, I have added the number of SRRs reported in the Benguela upwelling system (0.14 – 1.39 mmol m^{-2} d^{-1}), Chilean (2.7 – 4.8 mmol m^{-2} d^{-1}) and Peruvian upwelling system (5.2 mmol m^{-2} d^{-1}) in line 640 – 643 in revised manuscript..

I agree with Reviewer 2 (S Kasten) that there is no need of spending time/space highlighting this issue since uniqueness of your scientific contribution is what matters here.

(Response 1): As you and the reviewer #2 pointed out, I agree that there is no need of spending too much time/space highlighting this issue. So, we have substantially curtailed the length of the paragraph by deleting the following 16 lines "The East Sea is often called as "a miniature ocean" because of the independent thermohaline convection system that is driven by the high density surface water sinking (Kim et al., 2001) in a manner similar to that of the Great Ocean Conveyor Belt (Broecker, 1991). The turnover time (ca. 100 – 300 years) of the thermohaline circulation is shorter than that of the global conveyor belt of 1000 – 2000 years (Broecker and Peng, 1982). Because of the shorter time-scale, together with the relatively small volume, the East Sea is expected to be much more sensitive to global environmental changes (such as global warming) compared with the open oceans. In this regard, the East Sea has been considered as a natural laboratory that provides a useful field for large-scale oceanographic experiments to predict the response of oceans associated with long-term climatic/oceanographic changes (Kim et al., 2001). Over the last two decades (1982 – 2006), a rapid increase of sea surface temperature (SST) of 1.09 ℃ has been recorded in the East Sea, which is the fourth highest among the 18 large marine ecosystems in the world ocean (Belkin, 2009). Increased SST reduces the soubility of O_2 in the surface mixed layer and enhances stratification, which ultimately affects biological production in the water column and suppresses transport of O_2-rich surface water into the deep bottom."

(Response 2): Nonetheless, we still think it is important to mention the UB as a biogeochemical hotspot in this manuscript. In two previous papers (Lee et al. 2008; Hyun et al. 2010), we have argued that the sediment of the UB is a place where benthic mineralization is exceptionally high, considering the water depth, due to the formation of highly productive upwelling conditions in overlying water column. Based on the

repeatedly high benthic mineralization rates in present study together with the previous results, we feel that this distinct aspect of the UB deserves mentioning in line 639 – 661 in revised manuscript, and we find that the term "biogeochemical hotspot" captures this well. We also believe it is important to stress shortly the significance of monitoring the variations of C_{org} oxidation pathways since the DO in the bottom water of the UB has been decreasing ~10% over the last 30 years as stated in line 662 – 671.

I sincerely hope this revision is acceptable for you.

4) Reviewer 3: 8). I suggest moving evidence in lines 526-527 to paragraph starting in line 498 to support your argument.

(Response): Thank you for the suggestion. I moved the sentence "As manganese reduction is thermodynamically more favorable than iron and sulfate reduction, the Mn^{2+} liberation (Fig. 4) is likely resulted from dissimilatory Mn reduction." to line 510 – 511 as you suggested.

Looking forward to hearing from you

Sincerely yours

Silvio Pantoja

Associate Editor

Thank you again for your time.

Jung-Ho Hyun