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Interactive comment on "Benthic mineralization and nutrient exchange over the inner continental shelf of western India" *by* A. K. Pratihary et al.

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The manuscript (MS) presents some new and interesting results on benthic solute exchange and sediment carbon decomposition over the inner continental shelf of western India under different oxygen regimes governed by the monsoon-intermonsoon cycles. The MS can only be considered for publication after a major revision, which should comprise responses to the following remarks/questions (given below in random order).

Response: We thank the reviewer for his comments. We have taken care most of his suggestions in the revision.

(1) Question/ comment: The MS is far too long. It can be shortened with ca 50 % without losing its main messages.

Answer: As per the reviewer's suggestion, we will try to shorten the manuscript to certain extent during the revision.

(2) Question/ comment: The linguistics and/or sentence structure is often inappropriate. This must be improved before publication.

Answer: This will be taken care of during the revision.

(3) Question/ comment: Laboratory incubations to measure benthic fluxes are often inadequate and in situ measurements should be preferred. This is especially true when trying to do anoxic incubations, since it is VERY hard, if not impossible, to maintain laboratory incubations truly anoxic. In situ lander fluxes may agree with lab incubation flux results under oxygenated conditions, but not under anoxic conditions. I would not at all trust the anoxic laboratory incubation flux rates presented in this MS unless the authors present very strong evidences or proofs that the incubations really were anoxic. If they cannot do that, I judge the anoxic fluxes to be biased, and hence the MS falls down, and should not be published at all. The literature in general is already containing too many biased results, and BG should not belong to the category of journals publishing such results.

Answer: There are many studies on the bays and continental shelves with depth >20m (Hopkinson Jr. et al., 2001; Baric et al., 2002; Dennis and Grenz, 2003; Jahnke et al., 2005; Christensen, 2008; Faganeli and Ogrinc, 2009; Lehrter et al., 2012) which have reported reliable benthic flux rates measured by intact core incubations including anaerobic/ anoxic incubations. Comparative studies carried out by Miller-Way et al. (1994), Hammond et al. (2004) and Woulds et al. (2009) show that benthic flux rates generated by in-situ incubation and intact core incubation are consistent and agreeable in the shallow marine environments such as continental shelves even in low oxygenated conditions. However, the considerable differences in flux rates may appear at deeper environments (e.g. slope and abyssal plain) due to pressure effect and change in microbial and faunal response upon core recovery.

Due to our logistic limitations, we preferred to carry out intact core incubations instead of in-situ incubations. Before we used the specially designed core liner for the sediment–water incubations, it was tested for the gas-tightness and found to be absolutely gastight. We got sulfide in the incubations during October (Exp.1, 2 and 3) which clearly shows that the system turned truly anoxic. If the set up were not air-tight or impermeable, we would not have observed decrease in oxygen and subsequent accumulation of sulfide in the overlying water. This itself is a strong evidence that the incubations were truly anoxic. Thus we believe that the benthic exchange rates under anoxic condition are reliable, as given in the manuscript. Same set up was used for the incubations during April with all necessary tests for gas-tightness and permeability prior to the incubations. We disagree with the reviewer's comment that the literature contains too many biased results.

(5) Question/ comment: Hypoxic-suboxic-anoxic. I get the impression that the authors do not use terminology in a stringent way. Please consult "Canfield, D. E., and Thamdrup, B. 2009. Towards a consistent classification scheme for geochemical environments or, why we wish the term "suboxic" would go away: Geobiology, 7, 385-392" for the correct use of these terms.

Answer: The terminologies such as hypoxic, suboxic and anoxic will be used appropriately in the revised manuscript referring the suggested paper by the reviewer.

(6) Question/ comment: All of the so called "anoxic" incubations to measure benthic fluxes were not anoxic. In some of them there was still O2 in the water at the end of incubation. It is then very confusing that they are called "anoxic". This must be changed in a possible revised version of the MS.

Answer: Dissolved oxygen reached 0μM (or below detection limit) in all the anoxic incubations during October. In Exp.1, O2 was 0μM after 6h and sulfide was detected from that time onwards. In Exp.2, O2 decreased substantially after 36h and became undetectable at 56h. It is highly possible that O2 became undetectable even before i.e.

between 48-56 hrs. However, H2S was detected at 36h which increased with time. In Exp.3, O2 fell below detection limit after 36h with H2S accumulation afterwards (Fig. 3). Thus these incubations were truly anoxic.

Exp.5 (after 24h) and Exp.6 were never termed as anoxic in the text as there was still 20μM O2 present at the end of the incubations and H2S was never detected, rather they were mentioned as anaerobic incubations. The explanation for incomplete O2 consumption has been given in the section 4.1 of the manuscript.

(7) Question/ comment: In Fig. 5 showing fluxes during oxic, suboxic and anoxic conditions, the O2 and PO4 fluxes were the same under oxic and suboxic conditions. This really needs a good explanation.

Answer: Fig.5b (showing benthic flux during suboxia) will be deleted from the MS as per suggestion of the reviewer 1.

(8) Question/ comment: How can there be an O2 flux under anoxic conditions (Fig. 5c)? If the situation is anoxic, oxygen is absent, no oxygen flux can be measured, and the oxygen flux must be zero.

Answer: We admit that it is a mistake. We actually meant to show the benthic mineralization rate in Fig.5c. O2 flux will be deleted in Fig.5c in the revised manuscript.

(9) Question/ comment: Oxygen penetration depths are reported (or guessed?) in the MS (page 9620). How were these results obtained? Or were they just guessed? If so, please state that.

Answer: We did not measure the oxygen penetration depth in the sediment during April, but we observed a brown layer of ~5mm thickness at the sediment surface. This brown layer is apparently the top oxic layer which is also rich in Fe and Mn oxide. Just below this layer, the sediment was gray/ olive green in color throughout the core. While sectioning a core we got sulfide smell just below 0.5cm top layer. Thus we assume that the oxygen penetration depth would not be more than 5mm.

(10) Question/ comment: Authigenic carbonate fluorapatite do not precipitate in the top oxic zone of sediment (page 9626). Please rewrite.

Answer: Ruttenberg and Berner (1993) have observed authigenic carbonate fluoroapatite (CFA) formation in the top sediment layer at Long Island Sound and Mississippi delta. Long Island Sound sediment has an oxic layer of 1-3cm and Mississippi delta sediment is oxygenated up to 10cm. Several other authors (Lucotte et al., 1994; Slomp et al., 1996, Van der zee et al., 2002; Cha et al., 2005) have also reported CFA formation in the surface sediments. Schenau et al (2000) argued that in the organic rich sediments, the CFA precipitation is confined to the uppermost layer of the sediment as increasing carbonate alkalinity with depth inhibits apatite formation and also CFA formation needs F- diffusing from overlying bottom water. CFA does form in the top oxic zone but its concentration of course increases steeply below the redox boundary due to higher availability of reactive P from reductive dissolution of Fe and Mn oxides.

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