

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

### **Anonymous Referee #2**

Received and published: 17 September 2013

Review of “Benthic mineralization and nutrient exchange...” by Pratihary et al. MS No.: bg-2013-183

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).

The MS is far too long. It can be shortened with ca 50 % without losing its main messages.

The linguistics and/or sentence structure is often inappropriate. This must be improved

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before publication.

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 If they cannot do that, I judge the anoxic fluxes to be biased, and hence the MS falls down, and should not be published at all.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.

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.

All of the so called “anoxic” incubations to measure benthic fluxes were not anoxic. In some of them there was still O<sub>2</sub> 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.

In Fig. 5 showing fluxes during oxic, suboxic and anoxic conditions, the O<sub>2</sub> and PO<sub>4</sub> fluxes were the same under oxic and suboxic conditions. This really needs a good explanation.

How can there be an O<sub>2</sub> 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.

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10, C5107–C5109, 2013

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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.

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

There are many issues with this MS. It must undergo a major revision before it may be considered for publication.

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Interactive comment on Biogeosciences Discuss., 10, 9603, 2013.

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