

Interactive
Comment

Interactive comment on “Importance of intertidal sediment processes and porewater exchange on the water column biogeochemistry in a pristine mangrove creek (Ras Dege, Tanzania)” by S. Bouillon et al.

T. Jennerjahn (Referee)

tim.jennerjahn@zmt-bremen.de

Received and published: 20 March 2007

General comments:

This paper presents information on the exchange of dissolved and particulate substances between mangroves and coastal waters. Mangroves are very complex open systems in the land-ocean continuum which exchange energy and matter with terrestrial and marine systems at the same time. Therefore, information on the processes and fluxes is difficult to obtain, but is critical for carbon and nutrient budgets of the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(coastal) ocean.

I like the multidisciplinary approach of the paper which is required to gain insight into the processes and the authors present a good set of data. However, I have some reservations on the calculations made and particular on the inferences made from the set of data presented here. Although many of the statements/inferences made by the authors sound plausible to me they are based on a set of data derived from sampling of one only one single tidal cycle during the dry season. The authors do not have quantitative data and the calculations made are often based on one or more assumptions. Moreover, the concentration gradients discussed and calculations made will most probably vary significantly over a spring - neap tidal cycle and between dry and wet season. For example, it is conceivable that concentration gradients are very pronounced during the dry season, but total fluxes are small. In the wet season concentration gradients could be much lower, but total fluxes could be higher. Additionally, freshwater runoff will definitely be an important agent in transformation and transport of organic matter during the wet season. The study as such is very well done, only the authors should be more conservative in the conclusions they draw from their single-tidal-cycle-investigation or at least discuss other scenarios, the differences between spring and neap tidal cycles and the differences between dry and wet season.

Also, there are a few other general points of criticism. First, the discussion is in many places circuitous and lengthy. Moreover, the authors are sometimes jumping from one aspect to another and back which makes it difficult to follow the story. Second, the part on methane looks a bit out of place in this paper. In the present state of the manuscript the methane part does not add to the story. One could get the impression methane was measured and therefore had to be added to this manuscript. It should either be better linked to the overall story or deleted. Third, particularly in the second half of the discussion the authors in many instances do not adequately consider the existing literature, but cite only their own papers some of which are even not yet published. This is rather unfortunate as well as the fact that in several cases coauthors of this paper

BGD

4, S145–S151, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

are cited as "unpublished data". One could get the impression that the cake should be sliced in as many pieces as possible to sell as single publications.

In summary, the authors present a very well done study on the tidal exchange of dissolved and particulate substances between mangroves and the coastal ocean. The new and most important aspect of this study which otherwise is seldomly investigated is the porewater exchange of DIC originating from organic matter mineralization. This is very important information. However, the story told here should not go beyond the limits of a single-tidal-cycle study. I think, this manuscript needs some substantial (something in between minor and major) revision, but will afterwards definitely make a valuable contribution to the literature and therefore should be published in Biogeosciences.

Detailed comments:

Abstract:

The abstract is too long, a bit unfocussed and in places unclear.

P. 319, line 7: "highest water flow" could be either ebb or flood tide. This is unclear, but important with respect to erosion of surface sediments.

P. 319, line 11: What do you mean by this, how can a concentration "follow tidal variations"? Be more specific.

P. 319, line 13: Could you define any end-member pool sizes? As I understand there are no quantitative data.

P. 319, line 20-22 "Furthermore,...of tidal variations.": This sentence can be deleted.

The final part of the abstract is quite speculative and partly unclear. You may be right in concluding that "pathways of dissolved and particulate matter exchange are fundamentally different", but on the one hand this is not new and on the other hand it is not clear from the preceding paragraphs. Why don't you specifically describe your results?

BGD

4, S145–S151, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The final sentence does not contain important information and can be deleted.

Introduction:

The introduction is very long and in many places too detailed. It can be shortened substantially. This will make it easier for the reader to follow the thread.

P. 323, lines 1-3: This and the whole preceding paragraph are a bit unfortunate. First, you provide the reader with information on a larger scale investigation, but then leave him in the dark when specific information is expected. This can either simply be reduced to the objectives of this particular story/manuscript or it can be extended providing the overall frame of the larger scale study in more detail. However, I would prefer to have it reduced to the objectives of the study presented here.

Material and methods:

P. 323: More information on the study area is required, particularly with regard to the hydrology. Although - or better, because - only one tidal cycle was sampled during the dry season, it is important to have detailed information on tide characteristics (amplitude, spring - neap variation, diurnal - semidiurnal) and the seasonal variation of precipitation.

P. 324, lines 24-25: Measuring PN in acidified samples bears the possibility of obtaining erroneous values. It is better to measure PN in untreated samples. Also, it is helpful to use a standard substance which has a similar matrix like your "natural" sample, i.e. some kind of "standard sediment" rather than an artificial standard like acetanilide.

Results:

P. 325, lines 9-10: Didn't you say there is no freshwater input during the dry season? This means it is all oceanic water that is moving between the mangrove creek and the coastal ocean.

P. 325, line 27: How do you know the different pool sizes?

BGD

4, S145–S151, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Discussion:

P. 326, lines 23-26: What about evaporation in the water column? You mentioned it before to be the most important factor for increased salinity of creek water. It must not necessarily contradict the statement you make here on the porewater seepage, but rephrasing may help to clarify this.

P. 327: Your literature is very up-to-date, but the discussion in some instances, particularly here, could benefit from mentioning some older studies on the import-export processes mostly carried out in Australia and New Zealand (see Boto & Bunt, 1981, ECSS 13: 247-255; Robertson & Alongi, 1995, Geo-Mar. Lett. 15: 134-139; Woodroffe, 1985, ECSS 20: 447-461 and others from these authors and by Wolanski on the hydrodynamics).

P. 327, lines 5-15: What about the role of leaves in this context? What does "mangrove detritus" mean in this context, with or without leaves? It is conceivable that resuspension of surface sediments is an important factor, but I am curious which role leaves have in this context. You don't mention it, but leaves are generally a major contributor to the "mangrove detritus". A bit of information on that would help here.

P. 327, lines 21-23: What do we learn from the comparison with Brazilian and Thai mangroves?

P. 327, lines 24-25: How can "variations" be "consistent with concentrations"?

P. 327, line 27 - p. 328, line 2: What about ongoing degradation in the creek? Shouldn't it also be a significant factor for oxygen depletion in the creek?

P. 328 and 329: Especially in this part it is difficult to follow the thread. These two pages should be shortened and simplified.

P. 328, line 6-10: When you are talking about "added" DOC do you take into account that leaves may contain up to 40 % watersoluble components which can go into solution rapidly after leaves have fallen into the water? See Benner et al., 1986, ECSS 23: 607-

BGD

4, S145–S151, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P. 328, lines 10-19: What is the use of this part? Can be deleted.

P. 329, lines 9-10: Argueing like this is rather unbalanced. In some instances export occurs primarily in dissolved form, but there are numerous examples of export occurring in the particulate form. A general statement like this is definitely wrong. Again, see the papers mentioned above and by many other authors from other regions.

P. 330: The discussion of the inorganic carbon data seems somewhat aimless and unfocussed. I suggest that those of the authors with particular expertise in inorganic carbon restructure this part of the discussion which obviously is a very interesting aspect of this study.

P. 331 - 332: The whole discussion on methane either needs to be restructured, focussed and linked better to the rest of story or should be deleted.

P. 332 - 333: The summary is much too long. It should be shortened drastically and instead contain some more conclusions, however, within the frame of what can be concluded from a single-tidal-cycle study.

P. 333. The role of crabs is mentioned here, but not adequately discussed. Crabs are a very important factor in the recycling/retention of organic matter and nutrients in mangroves. This deserves more attention in the discussion. See papers by, for example, Camilleri, 1992, Mar. Biol. 102: 453-459; Robertson & Daniel, 1989, Oecologia 78: 191-198; Nordhaus et al., 2006, ECSS 67: 239-250.

P. 333 - 334: I find it rather difficult to have some kind of "balanced" conclusions. I find many of your conclusions/inferences plausible. Nevertheless, it also has to be made clear that all these are derived from analyses of only one tidal cycle. Can you direct the discussion and conclusions in such a more balanced way?

Figures:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Fig. 8: I like the idea of having a sketch with a conceptual model, but at present I find it a little too complicated and it is partly more a summary/list of aspects also given in the text. It will be very helpful if you can simplify it a little. So that it is more conceptual and includes a simple take-home message.

Interactive comment on Biogeosciences Discuss., 4, 317, 2007.

BGD

4, S145–S151, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper