Biogeosciences Discuss., 4, S222–S232, 2007
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Biogeosciences Discussions

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Interactive Comment

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

S. Bouillon et al.

Received and published: 29 March 2007

Response to comments by T. Jennerjahn

We are grateful to the referee for the many constructive comments on our manuscript, and briefly discuss the issues raised below, with the original comments preceeding each response.

REF: 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

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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 (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.

REPLY: We agree that the results from a single diurnal cycle should not be extrapolated too much, and do not include additional drivers such as surface runoff etc. We have added a short section on other potential influences in the Discussion.

REF: 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.

REPLY: The introduction has been shortened in places and the discussion has been re-organized, in line also with the suggestions by other referees.

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REF: 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.

REPLY: We can understand that the discussion on the CH4 fluxes might seem to lengthy in comparison to the rest of the ms - therefore, we have significantly shortened this section. On the other hand, the CH4 data and discussion are an integral part of the work, which we certainly wanted to retain in the ms, since we find them important in the sense that the patterns observed in CH4 (which originates from anoxic sediments) and the similarity with the variations of the other dissolved pools adds important evidence for our hypothesis that exchange of porewater solutes is an important mechanism for export of material from the intertidal zone.

REF: 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 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.

REPLY: We don't agree there -in the 2nd part of the discussion (p329 and onwards), we referred to 7 of our own studies, and to 19 other papers ? We do agree that some important references are worth to include (as mentioned further on). We understand that including 'unpublished' data here and there may seem as slicing the cake, but these data result from simultaneous sampling campaigns by different partners in two mangrove sites along the Tanzanian coast, and this is just a small part of the overall data gathered - it makes little sense to try to include the data gathered in a different context (e.g. sediment metabolism, bathymetry, nutrient fluxes, etc.) into a single paper. The references mentioned as 'in review' at the time of submission of this ms are now fully cited in the revised version; the 'unpublished' ones have been removed.

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REF: 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.

REPLY: We agree that the conclusions from this study should not be generalized, and have tried to stress this explicitly in the revised version.

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

REPLY: The abstract has been shortened somewhat (see also comments by other referees)

REF: 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.

REPLY: "highest water flow" refers to both ebbing and flooding tides - see Figure 3 and discussion thereof.

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

REPLY: We have modified this to : "Dissolved organic carbon (DOC), in contrast, varied in phase with water height and was highest at low tide. Stable isotope data of POC and DOC exhibit large variations in both pools, and similarly followed the variations in water height."

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

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REPLY: We refer here to the low tide and high tide DOC and POC concentrations, which are presented in the ms. See also reply to a similar comment further on.

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

REPLY: This sentence was deleted.

REF: 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?

REPLY: We understand that this might seem speculative, but this is confirmed by data from other mangrove sites (although not discussed in the ms). We do feel that the difference in exchange mechanisms were illustrated earlier in the abstract - since we mention that "Total suspended matter (TSM) and particulate organic carbon (POC) showed distinct maxima at periods of highest water flow, indicating that erosion of surface sediments and/or resuspension of bottom sediments were an important source of particulate material. Dissolved organic carbon (DOC), in contrast, followed the tidal variations and was highest at low tide."

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

REPLY: This has been deleted.

REF: 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.

REPLY: In line with a similar comment from another referee, the introduction has been shortened.

REF: P. 323, lines 1-3: This and the whole preceding paragraph are a bit unfortunate.

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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.

REPLY: We can agree with that, and have shortened this paragraph, focussing on this particular part of the work only.

REF: 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.

REPLY: We have added some more details on the sampling site and tide characteristics in the revised version.

REF: 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.

REPLY: This is a rather standard procedure, and are not convinced that measuring POC and PN on separate filters (treated and untreated) will give much improvement - two filters are rarely identical due to heterogeneity of the water column being sampled, and could then have the disadvantage of having a mismatch between the POC and PN data - in our case, the POC, PN, and δ13C data were all obtained from a single filter. The work by Lorrain et al. (“Decarbonation and preservation method for the analysis of organic C and N contents and stable isotope ratios of low-carbonated suspended particulate material”; Analytica Chimica Acta 491: 125-133) also suggests

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that this is an acceptable procedure, which does not appear to lead to loss of PN. We agree that using a standard that resembles more closely the sample matrix could improve the analyses, but do not consider this an impediment to our results.

REF: 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.

REPLY: We don't quite understand this comment - we don't mention freshwater inputs here, on the contrary the low tide salinity is higher than marine values.

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

REPLY: We agree that this might not have been clear; see earlier reply (abstract section): the pool sizes here refer to the low tide and high tide DOC and POC concentrations. We now mention this specifically in the revised version.

REF: 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.

REPLY: The evaporation effect mentioned earlier in the ms. refers mainly to evaporation in the intertidal areas, which result in highly saline porewater.

REF: 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).

REPLY: We agree that many of these are important references and should be included. We have added some references to Boto & Bunt (1981), Wolanski (1995), and Twilley

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(1985).

REF: 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.

REPLY: Transport of whole leaves is not included in this analysis, since we measured only POC in typically 100-200 mL water samples collected below the water surface. We agree that transport of macrolitter could be an important mechanism for organic matter exchange, and an aspect which has received very little attention so far, but we can do little but speculate on this part with the data at hand here. We now mention transport of macrolitter as one of the missing elements in the final section of the Discussion.

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

REPLY: We have added a reference to Twilley (1985) who observed a similar pattern in Florida, and now mention that this suggests that DOC variations in tidal creeks are determined by similar processes in different tidal mangrove creeks.

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

REPLY: What we meant here is that the DOC variations are consistent with the porewater influence hypothesis, in the sense that they match with DOC concentrations estimated based on the estimated porewater contribution at each sampling time based on the salinity (Table 1). This is now mentioned explicitly in the revised version.

REF: 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?

REPLY: An extensive dataset where water column incubations were performed in different sites in this system and another nearby mangrove site indicates that the water

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column itself is generally net autotrophic, so this cannot explain the observed O2 variations. We do not refer to these data as they are again part of a different study.

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

REPLY: We have tried to re-arrange the entire discussion, and hope that the revised version shows a more logic flow.

REF: 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: 607-619.

REPLY: We are aware of the fact that DOC can be rapidly leached from mangrove leaves in the water column, but this does not alter the approach we used - we simply estimated the d13C signature of the DOC pool being added, this is irrespective of whether it is direct DOC release (e.g. from root exudation), DOC leached from litter, or DOC produced during bacterial degradation of organic matter.

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

REPLY: Fair enough - this section has been deleted.

REF: 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.

REPLY: We do not see why this is definitely wrong - we merely state that “it has been suggested that most of the organic carbon is transported in the dissolved form”. There are indeed many reports of particulate organic C export, and do not claim that this is not the case or insignificant, but (i) DOC is consistently higher than POC (which, indeed, in itself does not prove that export rates must also be higher), and (ii) the available reports where both DOC and POC export were estimated find that DOC export is higher than POC export (e.g. Machiwa 1999, in Mangroves and Salt Marshes 3, 95-104; and Twilley, 1985, cited in the ms.).

REF: 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.

REPLY: the main points we wanted to raise in the discussion of the inorganic C data are that (i) that pCO2 and the resulting CO2 fluxes towards the atmosphere are highly variable, but generally high, (ii) the variations in DIC, TA, and pCO2 are consistent with porewater inputs, and (iii) the isotope data indicate that a significant fraction of mineralization was sustained by material of non-mangrove origin. We have tried to structure this better in the revised version.

REF: 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.

REPLY: We can agree with that, and have tried to shorten it somewhat and integrate it better within the Discussion.

REF: 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.

REPLY: we have re-structured the entire Discussion, and stress now that our results should not be over-extrapolated.

REF: 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

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78: 191-198; Nordhaus et al., 2006, ECSS 67: 239-250.

REPLY: We obviously agree that the role of crabs goes beyond what we had mentioned - we have moved this section to the fist part of the Discussion dealing with porewater seepage, and felt that this was not a good place to discuss these other aspects in detail. We did add a reference to some review papers in this context.

REF: 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?

REPLY: see earlier comments - we have tried to take this into account.

REF: Figures: 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.

REPLY: We agree that the last Figure is perhaps too complicated for a visual conceptual model, and have deleted two of the upper panels.

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