

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.

S. Bouillon et al.

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Response to Referee#2.

We are grateful to the referee for his/her constructive comments, and briefly discuss the issues raised below, with the original comments preceding each response.

REF: This manuscript describes the different pathways of dissolved and particulate matter exchange between the sediment and water column in an intertidal mangrove forest creek system. Material transport showed a strong tidal signal with highest particulate matter levels associated with high current velocities during ebb and flood tides

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and highest solute concentrations coinciding with low tides. Stable carbon isotope data followed tidal variations showing different contribution of marine, seagrass and mangrove end-members during low and high tides. A particularly interesting part of the manuscript is the identification of the important role of porewater seepage for the creek water column biogeochemistry and its implications for future budgeting efforts of intertidal systems. Generally, the presented data are convincing and well presented. However, I feel that some structural changes will improve the overall readability helping to keep the reader's attention. Introduction: The introduction is rather long and not very focused. For example, I feel the sections on litterfall estimates of C fixation (p. 320 ln 27 - p. 321 ln. 5) and the effect of crab burrows on the sediment hydraulic conductivity (p. 322 ln 8 - ln 12) distracts from the main aims of the study. Furthermore, these sections are repeated in the discussion and should be restricted to this part of the manuscript. Overall, I suggest shortening the introduction to improve readability by focusing on the main aims of the study.

REPLY: We have removed the section originally on p320/321; but have kept in the (short) section on crab burrows and hydraulic conductivity - this is in our opinion an important component of the rationale to perform this kind of work. The final section of the introduction was also shortened, in line also with the suggestion by Referee#4.

REF: Materials and methods: The methods are generally well described and understandable. However, I missed the description of various measurements (porewater salinity, TOC/TN in sediments, porewater DOC, Ca measurements) that are introduced in the discussion. As these methods are not described in the methods section and no citations are given, it is not always clear whether these measurements were conducted by the authors during the same study. If the above mentioned measurements described in the discussion were conducted by the authors during the same field study I suggest introducing these results in this section of the manuscript.

REPLY: It is correct that the methods for these measurements were not included, and that the data were not introduced in the "Results" section, since we mainly refer to them

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as background information. These data are part of a much wider set of data (incl. sediment and porewater composition, benthic mineralization and partitioning of metabolic pathways, benthic nutrient fluxes, etc.), and although the data were measured by some of us, we found little sense in detailing the entire set of data on these parameters and their methodology - we mainly wanted to support our conclusions with some of these data, but they will be presented in a different context.

REF: Discussion: While I feel that the results are appropriately discussed and most parts of the discussion are important for the reader's understanding, the discussion suffers from the authors jumping back and forth between different aspects of the study, which made it difficult to keep my attention. I suggest re-structuring the discussion into different subsections such as: 1.Importance of pore water seepage 2.Origin of organic matter during the tidal cycle 3.Organic matter export and CO₂, CH₄ emission 4.Organic matter import 5.Summary of exchange processes Roughly, I could imagine restructuring the following parts of the discussion into the abovementioned subsections: Section 1: (p.326 ln 20-26); (p.327, ln 19-p. 328 ln 2); (p. 330, ln 6-17) Section 2: (p. 328, ln 3-p.329 ln 6) Section 3: (p. 326 ln 27-p.327 ln 18); (p.329 ln 7-p. 330 ln 5); (p.331 ln 15-p.332 ln 20) Section 4: (p. 330 ln17-p.331 ln 14) Section 5: (p. 332 ln 21-p.334 ln 16)

REPLY: We agree that the discussion would benefit from some re-structuring so that there would be a more logic flow of thought, and this was also suggested by the other referees. We have therefore re-structured the discussion, taking into account as much as possible the suggestions from all referees.

REF: The calculation of the end-member signatures of POC, DOC and DIC (p. 328 ln7-10 & ln 20-23; p. 330 ln 23-25) was not very clear to me. I would appreciate if the authors could explain in more detail how these values were calculated.

REPLY: We agree that it might be better to explain this explicitly in the text - this information was briefly provided in the legend of Figure 6, but we have now added

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a short explanation on this in the main text of the manuscript: “These relationships can thus be used to estimate the $d^{13}C$ signature of the “added” DOC and POC from within the mangrove system, either directly from the inverse first order polynomial fit (i.e. $y=y_0+a/x$, see Figure 6), or alternatively as the intercept of the linear regression between $1/DOC$ and $d^{13}C_{DOC}$ or $1/POC$ and $d^{13}C_{POC}$, respectively. The resulting mangrove end-member $d^{13}C$ signatures are very similar: -26.6 ± 0.3 and -26.7 ± 0.3 per mil for DOC and POC, respectively.”

REF: Citation of Ludwig et al 1996 (p. 320 ln 13) and Neubauer and Anderson 2003 (p.322 ln1) is not in reference list. Please check also other citations.

REPLY: These have both been added to the reference list.

REF: The use of the word “constrained” in the manuscript (p.319 ln 21; p.320 ln17 & ln 24; p.334 ln 2) is confusing. I think the authors mean something like “described” or “investigated”?

REPLY: We used “constrain” to indicate placing upper/lower limits on the different components in the systems’ carbon budgets (the word is often used in this sense in modelling work); but where possible we’ve replaced it.

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

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