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Biogeosciences Discussions

Interactive comment on "Small scale variability of geomorphological settings influences mangrove-derived organic matter export in a tropical bay" *by* Geraldina Signa et al.

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Comments The authors of this paper have investigated changes in mangrove outwelling to seagrass beds and coral reefs over seasons in Gazi Bay, Kenya. Mangrove forests are highly productive-ecosystems and have important implications for the exchange of organic material in the tropical coastal seascape. They have combined more traditional analyses (isotopes) with modern techniques (fatty acids) to explain the source contributions and spatial distribution of organic material across the bay. In addition they also investigated how physical factors such as tidal exchanges, river runoff and season may affect the exchange of organic material between different habitats. The authors state they have found significant differences in transport of organic mate-

rial from mangrove forest creeks to adjacent systems. This was primarily due to river runoff and tidal transport. However the river runoff creek was influenced strongly by the rainy season. Interestingly the authors found that macroalgae also had a contribution to organic material, this ecosystem is often ignored in the tropical seascape. The paper is of an interesting subject with new techniques applied. There is a dearth of data regarding the outwelling of mangroves with physical factors taken into account especially with respect to connectivity. It is quite well written and structured. The abstract and introduction are well done and flow nicely. There are minor comments on these sections. I admire the authors for doing such a through job on this study, they have tried to expand our understanding using new techniques in addition they must have worked very hard. It is not easy to complete fieldwork over two seasons and fatty acids are very labor intensive. However, I have some concerns regarding the methodology, results and major concerns (section 4.2) regarding the discussion.

Methodology No statistics were used to compare the isotopes sources. This makes it difficult to understand the patterns in the data the authors state. The changes the authors saw in the organic material sources did not seem statistically significant nor could was it shown in the figures. A statistical test would resolve this.

R: We agree with the reviewer that statistical tests were needed to support our results and discussion. Consequently, we have performed permutational multivariate ANOVA (PERMANOVA) both on d13C and C/N values of organic matter sources and on the outputs of the Bayesian models (lower and upper limit of the credibility intervals, mode and mean). In both cases, the outcomes of the analysis highlighted that the patterns described in the text were significant, hence the main message of the manuscript was unchanged. PERMANOVA on the Bayesian mixing models will be showed in a new (now Table 1).

Results The figures depicting the results are numerous and difficult to understand. Several of the tables/figures could go into the supplementary section (even though there is already data in this section), other figures need to be edited for clarity and

others could be drastically improved by a different method of illustrating them. Several issues in the discussion may be due to the difficulty in understanding the figures. In addition I and I think others would find it easier if at some points the authors specified which creek applied to transects A and B, it was annoying to keep on referring to the methodology to understand which was which.

R: We thank the reviewer for his/her constructive comments. Consequently, we will made several changes in tables and figures (described in more detail below). Moreover, throughout the manuscript, we will refer to the creek name, together with the transect letter.

For example, section 3.1.2, line 7- I am not sure if “often” is the correct word to use. They seemed to be almost always overlapping.

R: We will change "often" with "overall".

Table 1 and 2 could go in the supplementary material. Table 1 especially should be in the supplementary material.

R: Both Table 1 and 2 will be moved in supplementary materials (now Suppl. 1 and 2).

Figure 1 is badly drawn. The labels for the ecosystems do not seem to correspond to where the ecosystems are although I understand that the authors found it difficult to place them on the actual systems. The map looks amateurish.

R: We have changed Figure 1 to improve clarity. The names of the stations have been placed in the right position. “Creek” and “river” have been added to the watercourse names. Moreover, we have drawn both coastline and transects.

Figure 2 is a very difficult figure to understand at first glance. For example if mangrove leaves are a source (symbol is a cross) then why are they not deceasing in size similar to the other sources? In addition the symbols decreasing in size do not help comprehension of the figure. This information may be better shown in a table.

R: We agree with the reviewer 1 that Figure 2 is not clear as it was drawn in the previous draft. Nevertheless, we think it is more appropriated to keep a figure for representing isotopic and elemental data, rather than drawing a table, because the scatter plot draws together both variables and allows to see reciprocal trends at a first glance. Hence, we have changed the figure to make it clearer and more understandable. The sources from the different stations have been drawn in different colors instead of different size. There is only one symbol for mangrove leaves (the black cross) because they were collected only in the mangrove stations, as we will specify in the manuscript.

Figure 4 and 5. Could this be done better as a percentage column graph? From my comments on the discussion, I found it difficult to see the patterns the authors stated were there.

R: Figures 4 and 5 represent the probability distributions that a certain source contribute to the two abiotic compartments studied: SOM and SPOM. Hence, it is important to show the entire credibility interval, indicating both the modes and the upper and lower limits of the 95% credibility interval. Anyway, we agree with the reviewer that the patterns described in the text are not always evident from the figures. To address this issue, we will perform a statistical analysis (PERMANOVA) and change the whole paragraph to make clearer the description of the patterns.

Figure 6, should either be deleted, put into a table or put into the supplementary material.

R: Figure 6 has been deleted.

Discussion In the first section (4.1) of the discussion the authors state (pg 11 Line 2-5) that the depleted 13C in seagrasses and macroalgae could be due to the different physical settings of the two creeks. The depleted 13C in seagrasses and macroalgae is also referred to in the results but I cannot see how this can be inferred by the associated figure. The explanation is plausible but I cannot see the evidence from the data the authors refer to. It would be easier to understand if this figure (fig.2) was represented

in a table.

R: We agree with the reviewer that figure 2 is not clear enough. Now we we have modified the figure to make it clearer, and the trends described in the text will be also changed to be more intelligible. In the right panels (transect B corresponding to Kinondo creek), δ13C values of macroalgae and seagrasses from the mangrove stations (black diamonds and circle respectively) are more negative (macroalgae $\sim$ -25‰ $seagrasses\sim$ -20‰ than those represented in the left panels (macroalgae $\sim$ -20‰ $seagrasses\sim$ -15‰. Moreover, PERMANOVA confirmed that these differences were significant.

Section 4.2 states that mangrove derived material from Kinondo Creek greatly contributed to the sedimentary pool and moving seaward decreased its contribution until dropping steeply in the coral reef. This is true for fatty acids (fig. 7) but I cannot see from the isotopes (fig. 4 and 5) how the authors came to this conclusion. In transect B (Kinondo Creek), mangrove derived material does contribute to the sediment although I would not state it contributed the most nor does it decrease from mangrove forests to seagrass beds, at some points it increased its contribution! However the authors are correct in that its contribution steeply drops off at the coral reef. The authors also state that the transect A, due to the influx of freshwater the export of mangrove derived material is further and indicates a significant contribution to the whole bay. Again this explanation is plausible and the authors place their explanation well within the known literature. But if Figure 4 and 5, which are suppose to represent this pattern, they do not show this. Considering that the authors state that there is a difference between the creeks, I cannot see a statistical significant difference between the two transects from the figures. However their explanation is plausible and I wonder if the wrong data is being shown? The fatty acids do confirm the authors explanation, not the isotopes.

R: Thanks to the reviewer's comment, we have revised the whole isotopic dataset and actually there were a few typos: mean δ13C ($\pm$ ds) of macroalgae from the intertidal area along transect A (-20.08 $\pm$ 0.66 ‰ instead of -20.63 $\pm$ 1.41 ‰ and along transect

B (-21.79 $\pm$ 1.44 ‰ instead of -20.12 $\pm$ 0.89 ‰ and the mean δ13C ($\pm$ ds) of seagrasses from the mangrove area along transect B (-20.23 $\pm$ 1.74 ‰ instead of -19.34 $\pm$ 2.83 ‰. Nevertheless, the patterns of the contribution of organic matter sources to SOM and SPOM did not change significantly. We agree with the reviewer that the patterns described in the text are not striking from figures 4 and 5. Hence, we run PERMANOVA to test for differences among seasons, transects and stations and results highlighted significant differences in all cases. Pair-wise tests among stations were always significant too. Moreover, we will modify this paragraph, softening the discussion about mixing models outcomes and focusing more on fatty acid results. Section 4.2 (pg 12, lines 7-9), the authors should include a line or two regarding seasonal changes in litter fall. Avicennia sp. in Brazil will lose their leaves only directly between seasons whilst Rhizophora sp. have leaf fall continuously over the seasons.

R: We are grateful to the reviewer for this interesting comment. We will add a few lines on the seasonal variability in litterfall in Gazi Bay.

All other comments for the discussion are minor. Minor comments Pg 2, line 20. Hemminga et al. 1994 is a eminent paper but not a recent one.

R: We will remove Hemminga et al. 1994.

Line 26. Wave power is not the correct word to use here.

R: We will change "wave power" with "wave action".

Pg 3, line 6-10. Nitrogen isotopes are also used and should be mentioned here.

R: d15N is a powerful tool to identify diet and trophic levels of consumers in natural ecosystems, rather than to trace organic matter sources. This is because tracing organic nitrogen food sources is complicated by trophic enrichment, and then it is rarely used to this end. Consequently, we think that mentioning nitrogen stable isotopes here would be misleading.

Line 23, A reference should be given for rainfall magnitude.

R: We will add a reference.

Pg 4, line 19. What is the depth of the coral reef?

R: We will add this information.

Line 23. “unless low groundwater discharge” please re-write for clarity.

R: We will rephrase the sentence to improve clarity.

Line 24. What are the high flow rates?

R: We will add this information.

Line 27. “ones” is not a scientific word.

R: We will change "ones" with "flows”.

Pg 5, line 2 “one” not a scientific word.

R: We will rephrase the sentence to remove the word "one".

Line 8. “ones” not a scientific word.

R: We will rephrase the sentence to remove the word "ones".

Line 14. How much volume of sediment was collected in the cores?

R: The cores were approximately half-filled but only the first 5 cm were used for the analysis, as specified in the text.

Line 15. How were the seagrass leaves and macroalgae sampled, plucked?

R: We agree that the sentence was not clear. We did not collect the leaves, but the whole shoots (seagrass) and thalli (macroalgae). Hence, we will change the sentence to specify this.

Line 18. How were the samples kept cool and dark before arrival?

R: We will add "in a cool box".

Line 27. What type of micro mill was it?

R: We will add the model of the micro mill.

Pg 6, line 6. The equation could be presented much more clearly.

R: The equation will be centered in the page and spaces will be inserted to improve clarity and readability.

Line 10. I cannot understand what you did from this sentence.

R: A more detailed description of the lipid extraction method will be provided.

Line 25. Why did you not also look at terrestrial sources, two lines in the introduction should provide some justification for not using terrestrial sources.

R: Mangroves are the dominant terrestrial sources in the area as highlighted by previous research, but agricultural runoff from the sugar plantations surrounding the forest might also flow into the watercourses. We will specify both information in the Introduction section and also we will take into account the potential contribution of agricultural runoff through the fatty acid approach, as suggested by reviewer 2.

Pg 11, line 19. Do you mean the transects when you state “stations”.

R: We agree that the sentence was not clear because we meant stations, not transects. Now we will change the sentence to clarify our point of discussion.

Pg 12, line 21. When was the timing of the other studies?

R: The timing of sampling is not specified in Bouillon et al (2007). In contrast, Hemminga et al (1994) sampled at both ebb and flood tide. The spatial gradient of the SPOM d13C found by Hemminga et al (1994) at ebb tide was similar to that found in this study. Hence, we will mention only Hemminga et al 1994 highlighting the comparable trend.

Line 22-25. I find this line confusing and had to read it several times to understand what the authors were meaning.

R: In this sentence, we meant that, despite the buffering role of seagrass beds in preventing a direct connection between mangrove and oceanic waters, we have inferred that high mangrove export coupled with high rate of water exchange at spring ebb tide has favoured the outwelling of suspended mangrove material up to the coral reef inner area. We will simplify the sentence to improve understandability.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-302/bg-2016-302-AC1-supplement.pdf

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-302, 2016.

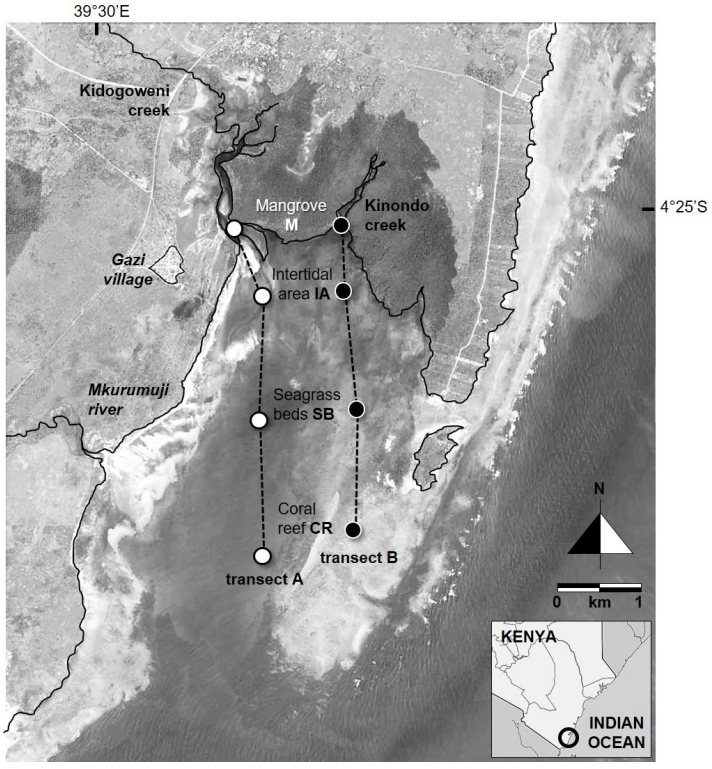

Fig. 1. figure 1

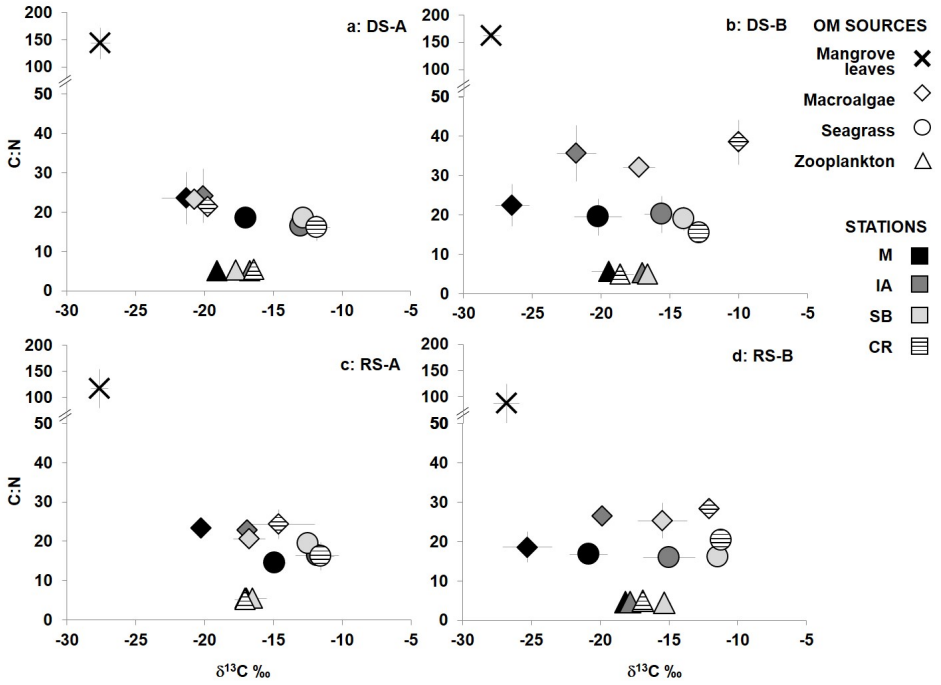

Fig. 2. figure 2

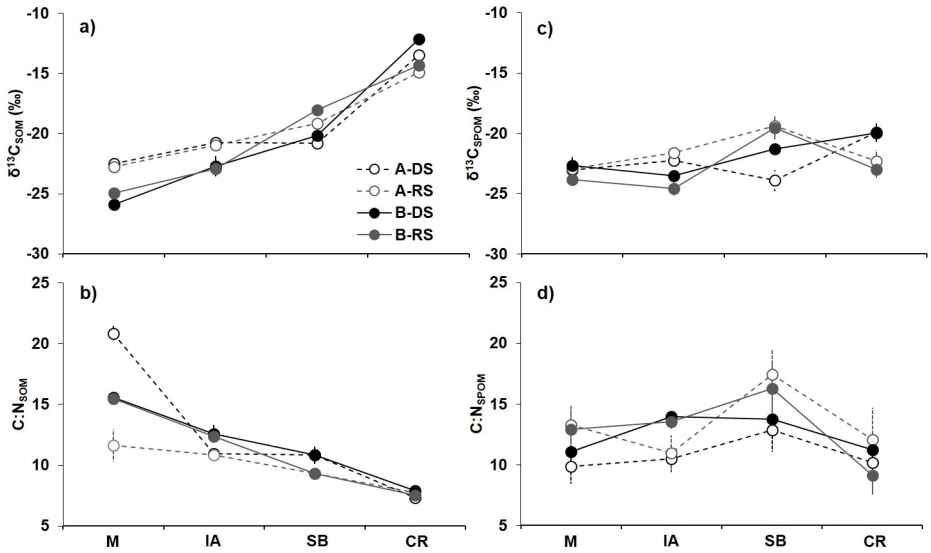

Fig. 3. figure 3

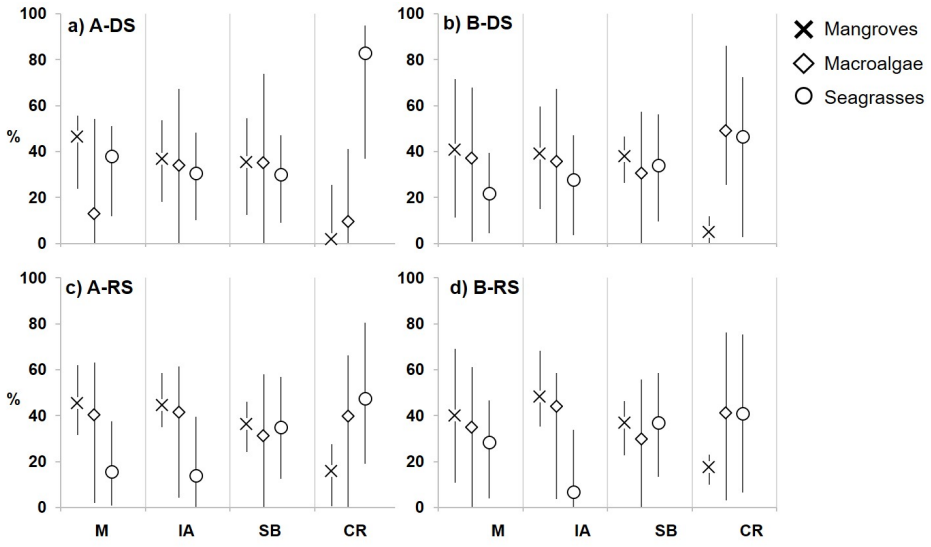

Fig. 4. figure 4

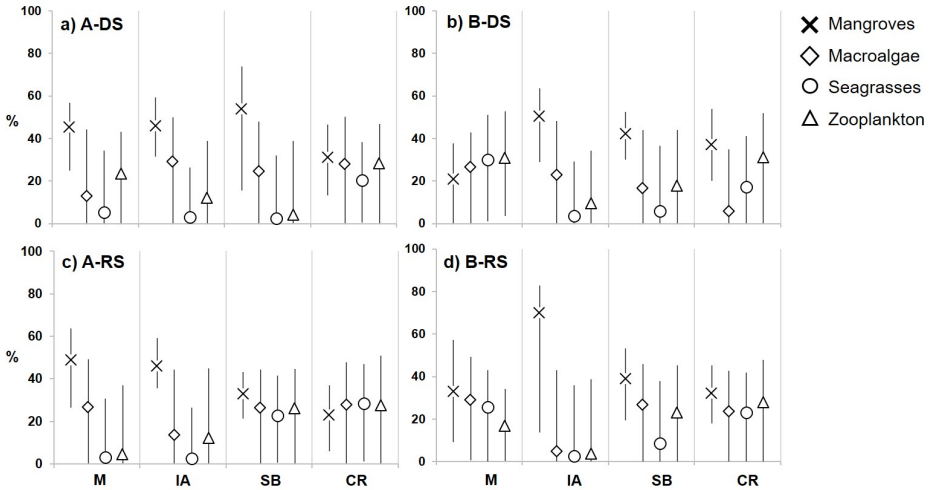

Fig. 5. figure 5