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Biogeosciences Discussions

Interactive comment on "Distribution and origin of suspended sediments and organic carbon pools in the Tana River Basin, Kenya" *by* F. Tamooh et al.

Anonymous Referee #2

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The authors present a large and comprehensive new data set of TSM and organic matter collected along the Tama River Basin, Kenya, for three hydrographically distinct seasons. They applied well established and adequate biogeochemical methods to characterize the origin of suspended matter and changes in its quality along the river course and on seasonal time scales and relate it to the natural setting (e.g. elevation, rain) as well as to human interventions (e.g. dam construction). The whole paper is written in correct English, the descriptions of the methods and calculations are of good quality, and most results are presented adequately. The authors could consider deleting/combining some of the figures. At some sections (see below) the interpretation of data needs improvement. However, the paper certainly addresses scientific questions within the scope of BG and is recommended for publication after major revision.

For revision, the following should be considered:

The whole paper is very long and should be condensed wherever possible; some suggestions will be given in the following.

Concerning the title, I would suggest to replace suspended sediments by suspended matter as this term is used in the text. The amount of supplementary material is quite large but acceptable. I miss the C/N ratios (is it calculated as mass or molar basis?) of individual SPM samples which could help to better differentiate OM sources when combined with d13C. These data should probably be added as figure or table to the MS itself.

The Introduction in general is sound and appropriate for the topic, however, for the global input of sediments (page 2526) I would suggest to additionally refer to "Syvitski, J.P.M., Vorosmarty, C.J., Kettner, A.J., Green, P., 2005. Impact of Humans on the Flux of Terrestrial Sediment to the Global Coastal Ocean. Science 308, 376-380", which considers both the impact of enhanced soil erosion and damming on global river sediment transport. The CO2 outgassing (page 2526) is not really relevant for the paper and could be deleted. Page 2527 first few lines: Is it necessary to list all the different concepts which are not relevant for the presented work? Also, the references to the role of microbes could be deleted because it goes beyond the content of the manuscript (lines 8-13). Line 22 Please remove DIC here because you don't deal with it in this paper, and add POC/PN ratio to d13 C and radio-isotopes

Page 2529 I think the purpose of the paper is more than being the baseline for the ones being published elsewhere, this statement should be changed accordingly.

Materials and methods Line 25: Does the discharge given here apply to the period before or after damming or does it represent a long term average? Any trend described in literature? The location of the two reservoirs sampled should be given in the map

Page 2530 line 10, please differentiate between riverine sediments and SPM here and

elsewhere in the text. Line 20 Were samples for N measurement acidified?

Page 2532 what do you mean by 'clear Sediments'? Were the sediments freeze dried/ dried before being ground?

Page 2532 line 20 remove Total Suspended Matter because abbreviation TSM has already been used before

Page 2534 line 2 are the POC/PON ratios really significantly different, give p-values.

Page 2535 line 2, give altimetry range for the regions named here line 16 OC instead of C Line 19, p value correct, shouldn't it be <0.05?

Page 2536 Line 5 fig 8 not yet mentioned, change order, check for other figures Line 14 River sediments?

Page 2537 Line 7 it is not possible to discern individual sites from the figure. Apart from the one high value of 4.5, I do not agree that the values are really higher during the end of wet season. Line 15 what is the value of atmospheric fallout; were derived from? Stick to TSM or SPM throughout the text

Discusssion

Page 2538 line 27: give distance instead of 'right after the dams'; if the concentrations are still high after dam, sediment trapping is not so effective as stated? How much decrease before and after dam?

Page 2539 Line 18: But how are the high TSM values during raining season caused? Should be explained here

Line 20-25: can you show the coincidence of %POC and higher d13C? I agree that the river bank sediment represents a potential endmember for TSM, but what could be the percentage of contribution? From Fig. 11 it appears that both sample groups are well separated. It would be interesting to add the values of river sediments, which might represent a second endmember, to the figure.

Page 2540 Line 12: which 'conditions' are meant? Please specify

I have some doubts about the significance of the age model and its interpretation. I am not an expert in this, but I wonder whether the differences obtained for the ages of individual samples support the given interpretation. If for example, bank erosion plays a critical role, I would expect much older material as compared to surface erosion. This, however, may not be expressed in the Be/Pb ratio when considering that the soil is Be dead below 2 cm as stated by Matisoff et al 2005. Accordingly, the age of deeper bank sediments will not be mirrored in the Be/Pb ratio. If the bank sediment age is not older than the max of 478 days observed in this study, the authors should clearly state and explain it in this section. In addition, I do not agree that the ages of SPM (not sediments as stated in line 15) are much older during end of wet season. Average age is even higher for the wet season (200 days vs. 180 days according to the data in Suppl Table 6; here please order the end of wet season data according to altitude and not sampling date similar to wet season data). I doubt the usefulness of the ratio for differentiation between bank and soil erosion and accordingly, I cannot follow the conclusion that sediment/bank erosion is much more important during the end of wet season. If bank/soil erosion is the source for the very high SPM loads, than I would expect a relationship between age and SPM load. Have you tested it? For the oldest samples, however, SPM load is only moderate. I find the age data interesting, especially because the age of SPM is much older than the ones given e.g. by Matisoff et al for river SPM which support erosion as major SPM source. However, I feel that the discussions soil vs bank needs better arguments (or the conclusions needs to be changed).

Line 27; why should high POC:Chl a ratios indicate C3 plants? Do you really mean that OC is directly derived from plant detritus or do you suggest that it is from soils with C3 vegetation. The bigeochemical signals should be different for both. What about C/N ratios? I would suggest including C/N ratios for better differentiation of OM sources.

Page 2541 Line 29: why did you take exactly this numbers as end-member values?

Page 2542 Line 6 where are the POC/PN data shown? Low relative to what? Line 9-13 I would suggest removing this section on fatty acids. First because you do not refer to these analysis in the methods and results, secondly, because you can argue accordingly with the data presented in the paper (e.g. POC /Chla, probably C/N) Line 14-29 The whole paragraph is very speculative. Is there any indication of C4 input from the composition of SPM in the reservoirs? Please give a short explanation about the processes which should support preferential C3 plant OM regeneration. Why is there no impact of the in situ production on reservoir sediments? Has this to be considered when comparing the reservoirs and lake burial efficiency? I would suggest removing this whole section on the cores and related figures and tables from the MS.

Page 2543 As far as I know the different fractions of OM (particle-bound and non-bound) can vary in the d13C and C/N (e.g. Hedges et al 1994, Limnology Oceanogr as only one study on OM composition in different size classes) because discrete particle often include vascular plants debris as described here. Have you checked C/N and d13C vs. OC:SA? Line 23-25 Do you think that sorting of particles (e.g. light plant debris) can have an impact of riverine sediments?

2544 Line 4-5 If the material is already very stable how can you explain the distinct stated degradational changes of e.g. d13C in reservoir sediments? Start new paragraph before 'The relationship. . ..'

Line 6-end of page you state two major reasons for POC%-TSM relationships, but I am not sure whether the POC variation observed in the study really supports the distinction between surface soil, litter and deeper soils. I would not exclude the first hypothesis, it is still valid under the impact of soil erosion etc. and negligible in situ production.

The statement in the last four lines appears contradictive: You state the POC% was higher than from soils and then you state that high POC% might be related to soils and detritus? If plant detritus plays a major role here, this conclusion could be supported by POC/PN values, please check.

Page 2545 Line 2 give unit for the literature values line 7 yes there is an increase, but we see very high variability at lower altitudes. Line 16 to the POC pool

Line 25 I cannot see the increase of D13C DOC , I would remove Fig. 5B

Line 28 Which values did you expect?

Page 2546 The last Chapter 4.3 contains a lot of redundancies with the discussion of d13C pattern on page 2541. Please combine the contents of both parts. The short conclusions in the last few lines of the chapter alone do not merit an individual section and should be added somewhere else.

Line 5 refer to figure 3a

Abstract/Conclusions

Both sections should be changed according to changes in the MS where necessary.

Figures

The authors should carefully consider whether all the figures are really necessary. Some of them (e.g., 8A; 6 A&B; 4 A&B; 9) have liitle informative value which could not be described in the text.

Fig. 1 maybe the two geographical units should be depicted.

Fig. 3 I do not see the sense of the insert figure of (A). Especially because the data given seem to be different from the those given in (A). Give some explanation in the figure caption.

Fig 4 what is meant by the samples "reservoirs combined". I guess these are data from Bouillon et al 2009? This should be mentioned.

Fig5 maybe figs 4 and 5 could be combined

Fig 6 . . .. . . along the Tana River Basin . . .. Why are the mainstream data excluded? (. . . not included in panel)

Fig 7 note in figure caption the meaning of the horizontal line in (A)

Fig 9 replace d13Csediment by d13Ctoc in figure caption

Fig 10 I would suggest to apply the same Y axis range in both plots for better comparison, the number of data points would easily allow to combine the data in one figure with different symbols. Max value of 4.5 is missing in figure B

Fig 11 is lower Tana synonym to main lower Tana? I would suggest also giving river sediment data in this figure as they are another potential source of River SPM. Replace riverine POC by river suspended POC%

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