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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.

F. Tamooh et al.

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Thank you very much for you invaluable comments and suggestions. Upon request, i will be willing to upload the revised version of the manuscript to confirm the changes made. Below, please find replies corresponding to each question, comment or suggestion raised.

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

Anonymous Referee #2

Received and published: 16 May 2012

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Interactive Discussion

Discussion Paper

REF: 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.

Ref: Concerning the title, I would suggest to replace suspended sediments by suspended matter as this term is used in the text. Reply: As suggested, "suspended sediments" has been replaced with "suspended matter"

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. Reply: indeed, POC/PN ratios were not included in Table 1, this was an oversight from us and we have now included it. It is correct that the data in the Supplementary file are important but we have the impression the amount of data presented here prevents it from being included as Tables into the manuscript itself. POC/PN ratios are expressed on a mass (atom) basis, this is now specified.

Ref: 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,

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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. Reply: Syvitski et al. (2005) has been referred in the manuscript and added in reference list.

Ref: The CO2 outgassing (page 2526) is not really relevant for the paper and could be deleted. Reply: This section was deleted, as suggested.

Ref: Page 2527 first few lines: Is it necessary to list all the different concepts which are not relevant for the presented work? Reply: This section has been modified, only three of the most relevant concepts (serial discontinuity, flood pulse and river continuum concepts) are retained in the revised version.

Ref: Also, the references to the role of microbes could be deleted because it goes beyond the content of the manuscript (lines 8-13). Reply: correct, this section has been deleted.

Ref: Line 22 Please remove DIC here because you don’t deal with it in this paper, and add POC/PN ratio to d13C and radio-isotopes Reply: This section has been deleted.

Ref: 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. Reply: We have rephrased this section.

Ref: 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? Reply: these data are our own average made from long-term data available on The Global River Discharge Database (RivDIS, http://daac.ornl.gov/RIVDIS/rivdis.shtml). Data in Maingi and Marsh (2002) suggest no differences in total discharge between pre- and post-dam periods, only changes in the seasonality and frequency of flooding.

Ref: The location of the two reservoirs sampled should be given in the map Reply: Figure 1 has been revised and now shows the location of Masinga and Kamburu dams.

Ref: Page 2530 line 10, please differentiate between riverine sediments and SPM here and elsewhere in the text. Reply: Riverine sediment refers to river bed sediment while SPM refers to suspended matter. This has been clarified in revised version of manuscript.

Ref: Line 20 Were samples for N measurement acidified? Reply: Since POC and PN measurements were made on the same samples, these were indeed acidified by exposure to HCl fumes (filters) or in situ HCl into Ag cups (soils, sediments).

Ref: Page 2532 what do you mean by ‘clear Sediments’? Reply: we do not see any reference to “clear sediments” in our manuscript, so unsure what this comment refers to.

Ref: Were the sediments freeze dried/dried before being ground? Reply: yes, these samples were freeze dried before being ground

Ref: Page 2532 line 20 remove Total Suspended Matter because abbreviation TSM has already been used before Reply: This has been corrected in the revised manuscript

Ref: Page 2534 line 2 are the POC/PON ratios really significantly different, give p-values. Reply: The p-value (paired t-test) is now provided.

Ref: Page 2535 line 2, give altimetry range for the regions named here Reply: the elevation range has been inserted.

Ref: line 16 OC instead of C Reply: This has been corrected.

Ref: Line 19, p value correct, shouldn’t it be <0.05? Reply: indeed – we have corrected the p-value

Ref: Page 2536 Line 5 fig 8 not yet mentioned, change order, check for other figures Reply: The order of figures has been harmonized

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Interactive Comment

Ref: Line 14 River sediments? Reply: this sentence was corrected to read "Riverine sediment. . ..."

Ref: 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. Reply: The discussion of these results has been re-arranged, see also further comments and replies. Also, we have modified the corresponding Figures.

Ref: Line 15 what is the value of atmospheric fallout; were derived from? Reply: No direct measurements of atmospheric fallout of 210Pb and 7Be are available for this region. The7Be/210Pbxs AR ratio used (12) was extrapolated based on data available in the literature (Saari et al. 2010, Liu et al. 2001). This estimate has some uncertainty, but this will affect only the absolute age estimates, and not the relative trends. We have clarified this and added the literature sources to the reference list. Liu, H, Jacob, D.J., Bey, I., and Yantosca, R.M.: Constraints from 210Pb and 7Be on wet deposition and transport in a global three-dimensional chemical tracer model driven by assimilated meteorological fields, J. Geophys. Res., 106, D11, 109-128, 2001. Saari, H.K., Schmidt, S., Castain, P., Blanc, G., Sautour, B., Masson, O., and Cochran, J.K.: The particulate 7Be/210Pbxs and 234Th/210Pbxs activity ratios as tracers for tidal-to-seasonal particle dynamics in the Gironde estuary (France): Implications for the budget of particle-associated contaminants, Sci. Total Environ., 408, 4784-4794, 2010.

Ref: Stick to TSM or SPM throughout the text Reply: we now use TSM consistently.

Discussion Ref: 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? Reply: Distance indicated now in the manuscript. The reservoir is quite effective in trapping the sediment but the concentration we referring to is Masinga reservoir's compared to the average of the headwater streams which is relatively low. We observed a decrease in TSM concentrations of 23% and 10%, during dry and end of wet seasons respectively – but these should not be overinterpreted as they are merely concentration measurements and not sediment fluxes. Literature estimates of sediment trapping efficiency are up to 90% for Masinga Reservoir.

Ref: Page 2539 Line 18: But how are the high TSM values during raining season caused? Should be explained here Reply: This is better addressed now in the revised version. Ref: 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 end-member, to the figure. Reply: Data from riverine sediments have been added in Fig 11.

Ref: 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).

Reply: (i) “conditions” was removed. (ii) inputs from deep bank erosion would result in low 7Be/210Pbxs values, since, as the reviewer correctly mentions, these deeper layers are expected to be 7Be-dead. Hence, the absolute ages are indeed not as old as might be expected if the suspended sediment would result only from bank erosion. It should be kept in mind, however, that the suspended sediment is a mixture of different sediment sources with different ages, and hence the age on the bulk suspended matter is a relative measure of the average age. Another way to represent the data is (as mentioned in the ms), to express them as an estimate of the % contribution of “new” (recent) sediment. The suggestion to look into relationships between the radionuclide data and TSM is valuable, and we have modified Figure 10 to include the observed relationships between 7Be/210Pbxs and TSM, as well as with %POC. We have now also included the data from sites more upstream into the new Figure (these were included in the Supplement Table, but not in the original Figure 10). The discussion of these data has been thoroughly revised, with a less strong interpretation of seasonal difference and now focusing on the following key patterns: (i) Along a longitudinal profile, seasonal differences in the average age of riverine suspended matter are not pronounced (ii) During the wet season, there is a clear downstream decrease in sediment age (or, increase in the relative contribution of recent sediment) – this reflects recent surface erosion. (iii) During dry conditions, increasing TSM concentrations correlate with sediment age, i.e. inputs of sediment can be considered to be relatively old (=resuspended material or bank collapse). In particular, one site shows almost 7Be-dead material. (iv) Overall, %POC is higher in more recent suspended matter – consistent with a surface erosion end-member. We also agree that the interpretation of the radionuclide data is not straightforward but that there are some interesting patterns emerging. In the near future, we have foreseen much more extensive sampling to further explore this tracer of sediment provencance and dynamics.

Ref: Line 27; why should high POC:Chl a ratios indicate C3 plants? Reply: This is a misunderstanding- we use two different arguments (i) high POC:Chl a ratios point towards a terrestrial (not algal) source, and (ii) the low d13C-POC values point towards C3 vegetation (rather than C4 vegetation). This is now clarified in the manuscript.

Ref: 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 biogeochemical signals should be different for both. What about C/N ratios? I would suggest including C/N ratios for better differentiation of OM sources. Reply: Actually, we suggest OC is derived from direct plant detritus based on the fact that measurements in headwaters correspond with high %POC typical of plant detritus matter coupled with highly depleted δ13CPOC values and relatively high POC:PN ratios hence strongly suggestiong C3 vegetation as possible origin.

Ref: Page 2541 Line 29: why did you take exactly this numbers as end-member values? Reply: Based on our own samples measurement from the site, this is now clarified.

Ref: Page 2542 Line 6 where are the POC/PN data shown? Low relative to what? Reply: As mentioned earlier, POC:PN ratios are now included in supplementary Table 1. POC:PN values are low relative to those of headwater streams. This has been clarified in the manuscript.

Ref: 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 /Chl-a, probably C/N) Reply: This section was deleted and modified, with high POC:Chl a ratios reflecting absence of algal contribution

Ref: 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. Reply: We understand these concerns, but feel it important to keep these data and their discussion in the manuscript, as they provide an unexpected and intriguing result –even if we cannot provide a conclusive explanation for this strong C4 signature. However, we have significantly rewritten this section taking these comments into account : (i) regarding the impact of in situ production : this is a valid point and we can interpret part of the depth trends in this context: in situ production is expected to have quite 13C-depleted signatures (e.g measurements during the dry season when most of the POC was of algal origin), and the depth trends concur with its preferential degradation: loss of OC in deeper layers is associated with an increase in d13C, i.e. consistent with the more rapid loss of the 13C-depleted algal biomass, to the extent that its contribution to the long-term C storage is not detectable. This suggestion is now discussed. (ii) As for indications of C4 inputs from the composition of TSM in the reservoirs, this question is not entirely clear to us. Data on d13CPOC for the reservoirs are presented in the manuscript (Figure 4 and 7), and they do not show the same pronounced C4 signature as already discussed.

Ref: 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? Reply: Indeed, there are some patterns described in characteristics (%OC, POC/PN, d13C) between different particle size classes for certain systems, and several mechanisms have been invoked to explain these. In our case however, we do not have discrete samples of different size classes – only SA measurements which should be related to the overall size class distribution or relative contribution of clay minerals. However, we do not find any consistent patterns between OC/N ratios or d13C signatures and SA data.

Ref: Line 23-25 Do you think that sorting of particles (e.g. light plant debris) can have an impact of riverine sediments? Reply: Yes – but we do not see how are data can be adequately interpreted in this context, as plant material can be both C3 and C4 derived given that our samples come from contrasting sites.

Ref: 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? Reply: See reply to one of the previous comments: we do see a down core decrease in %OC and OC:SA ratios which coincide with an increase in d13C – hence, this would be consistent with a contribution of more labile, phytoplankton component in the top layers which is preferentially degraded. This suggestion is now discussed in the revised version.

Ref: Start new paragraph before 'The relationship:. . ....' Reply: This has been done.

Ref: 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. Reply: We do not entirely agree on this point – we see no evidence or indications in our dataset which would support the first hypothesis (either from POC:Chl a ratios, POC:PN ratios or δ13CPOC values).

Ref: 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. Reply: the POC:PN ratios of suspended matter in headwaters are generally high confirming the origin of organic matter is possibly from plant detritus.

Ref: Page 2545 Line 2 give unit for the literature values Reply: This has been corrected now and units given as mg L-1 Ref: line 7 yes there is an increase, but we see very high variability at lower altitudes. Reply: The statement has been edited indicating variation downstream

Ref: Line 16 to the POC pool Reply: this was corrected

Ref: Line 25 I cannot see the increase of d13C DOC , I would remove Fig. 5B Reply: Thi section has been modified to indicating δ13C signatures of DOC were generally stable. However we choose to retain fig.5b for comparison with δ13CPOC pattern

Ref: Line 28 Which values did you expect? Reply: we expected low signatures since headwaters sub-catchment is dominated by C3 vegetation.

Ref: 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. Reply: This section has been edited to carefully reduce repetition with previous parts

Ref: The short conclusions in the last few lines of the chapter alone do not merit an individual section and should be added somewhere else. Reply: We agree that it's rather short but when removing this section the manuscripts ends rather abruptly. We chose to keep a "Conclusions" section but have expanded this somewhat and added a number of other highlights.

Ref: Line 5 refer to figure 3a Reply: Reference is now made to figure 3b and not 3a

Ref: Abstract/Conclusions: Both sections should be changed according to changes in the MS where necessary Reply: Both sections have been updated accordingly.

Figures Ref: 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 little informative value which could not be described in the text. Reply: We have carefully re-evaluated the relevance of all Figures – see other replies and comments from Referee #1.

Ref: Fig. 1 maybe the two geographical units should be depicted. Reply: Latitude and longitude have been added in figure 1.

Ref: 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. Reply: The data are the same, but the insert shows only data for the wet season campaign, this is now mentioned in the Figure legend. We prefer to keep the insert of Figure 3a, as the data can otherwise be misleadingly interpreted as showing a consistent increase in TSM during all seasons. The insert shows that the pattern during the wet season is much more erratic and does not show a smooth, gradual downstream increase.

Ref: Fig 4 what is meant by the samples "reservoirs combined". I guess these are data from Bouillon et al 2009? This should be mentioned. Reply: This refers to all suspended matter data from reservoirs and not from Bouillon et al. (2009) only. This is now mentioned specifically in the Figure legend..

Ref: Fig 6....... along the Tana River Basin........ Why are the mainstream data excluded? (..... not included in panel). Reply: We are comparing data from soils to those of riverine C species – which in our opinion only makes sense for the tributaries, and not for the mainstem river where soil samples reflect very local conditions but riverine samples integrate inputs from a large catchment.

Ref: Fig 7 note in figure caption the meaning of the horizontal line in (A)- Reply: The horizontal line represents a DOC:POC ratio of 1, this is now mentioned in the revised version.

Ref: Fig 9 replace d13Csediment by d13Ctoc in figure caption Reply: This has been corrected.

Ref: 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

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figure with different symbols. Max value of 4.5 is missing in figure B Reply: This Figure has been replaced with an alternative Figure.

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%- Reply: Indeed, lower Tana is the same as the main lower Tana. Riverine sediment data has been included in the figure now. Riverine POC has been replace with river suspended POC%.

Interactive comment on Biogeosciences Discuss., 9, 2523, 2012.