Biogeosciences Discuss., 9, 7943–7981, 2012
www.biogeosciences-discuss.net/9/7943/2012/
doi:10.5194/bgd-9-7943-2012

Organic carbon and nitrogen export from a tropical dam-impacted floodplain system

R. Zurbrügg, S. Suter, M. F. Lehmann, B. Wehrli, and D. B. Senn

Reviewer #1

General comments

This paper addresses the role of tropical floodplains in the transport, storage and transformation of the organic matter along the aquatic continuum from land to the ocean. The manuscript analyzes fluxes and quality of organic matter flowing through the Kafue Flats, a tropical dam-impacted floodplain system in Zambia. This study presents relevant and important data of significant current interest, and is likely to attract a wide group of readers from the growing field of inland water carbon cycling.

The study is well designed, the sampling and analytical techniques are state-of-the art and are clearly documented. The authors show convincingly that during the flooding season, more than 80% of the river water that passes through the floodplains remobilize and export to the river downstream large quantities of organic carbon and nitrogen (mainly in the dissolved form). They provide interesting correlations between elemental C:N ratio, isotopic signature, and spectroscopic proprieties, which help them to assess distinct source of dissolved and particulate matter and which will be a useful reference for further studies on this topic. Overall, dissolved organic matter has the same terrestrial origin in the upstream reservoir, river, and floodplain while particulate organic matter in all systems is dominantly of aquatic (phytoplankton) origin.

The paper is generally well-written although language can be further improved (see my suggestions below: Technical corrections/suggestions). The relevant scientific debate is cited and meets the standards of Biogeosciences. There are however a number of issues listed below that the authors should addressing prior to publication.

Thank you for comments, they are very helpful as we revise and improve the manuscript.

Specific comments

The abstract. This is just a suggestion but I think the reader expects to see a final conclusion of the abstract that relates more to the effect of the Kafue Flats on the OM dynamics (more related to the title and the main goal of the study) rather than a conclusion on the effect of the upstream reservoir which is mainly derived from the results of others (Kunz et al., 2011, Wamulume et al., 2011, etc). The authors may consider re-writing the last paragraph of the abstract or add another final conclusion to accommodate this remark.

Good suggestion, we will do this.

Page 7945, line 25: States that particle trapping in dams together with hydrological alterations resulted from dam construction "may affect riverine OM loads and quality. . .". While this is perfectly true, it is however not limited to "tropical systems" as suggested latter. Consider removing "in tropical systems".

We agree and will remove "in tropical systems" from the revised manuscript.

Chapter 3. Results Both subchapters 3.1 (describing the DOC, DON, DOC:DON and POC, PON and POC:POC) and 3.2. (describing 13C-DOC, 13C-POC, 15N-TDN and 15N-POC) are a bit difficult to follow since there is a downpour of mix information on temporal (2 flooding season + one dry season) and spatial variability of several parameters along the river stretch plus floodplain samples. I would suggest a bit of structure there.

We address this by including an introductory sentence that provides a roadmap, explaining how the results are presented, namely as "flooding season" versus "dry season" results. This is also indicated in the revised Figures 3, 4, 7, and 8.

Moreover, why those results are not discussed and presented (Figure 3 and Figure 4) in chronological order as someone would expected from 2008, 2009 and 2010?

The focus of the Results chapter is on the difference between flooding season and dry season rather than on the temporal sequence. We realize this may cause some confusion, so for clarity, the labels "flooding season" and "dry season" will be added to the Figures 3, 4, 7, and 8. The May 2010 data are presented first because of the completeness of the data set.

Also, the variability of all parameters together with patterns along the river stretch during the three seasons that are illustrated in Figure 3 and Figure 4 and described in both subchapters (3.1 and 3.2) should be better documented (see points below).

See answer below.

Page 7953, line 24: The authors stated that during the dry season (October 2008), d13C-POC along the river was relatively constant at around -26‰. While this constancy is true for the first 250 km below the dam, a sharp and constant decreased pattern from about -24‰ to -30‰ (see Figure 4) emerge after 250 km. This is worth documenting as such decrease suggests an important shift in the origin of POC (from more terrestrial to aquatic sources) as water come out of the floodplain.

Yes, we should have pointed this out originally. The changes in δ^{13}C-POC in October will now be mentioned in the Results part as suggested by the reviewer and possible explanations are provided in the Discussion section. As an answer to reviewer #2 we also present POM profiles from Itezhi-Tehzi reservoir as supplementary information. The ranges of δ^{13}C and C:N of reservoir POM will, in addition, be indicated in Figure 8.

Moreover, while relatively constant along the upstream river stretch, dry season 13C-POC seems to me significantly higher (around -26‰ than during both flooding seasons (average around -29‰. Those are important differences suggesting seasonal variability on the origin of OM and need to be pointed out and properly documented. Same apply to 15N-TDN and 15N-PON.

The variations in δ^{13}C-POC between the sampling campaigns will now be addressed in the Discussion section, along with N stable isotopes. POM profiles from Itezhi-Tezhi help to interpret the differences between the sampling campaigns.

Page 7954, line 20: Related to the hydrological exchange between the river and the floodplain, it is stated that at around 200 km downstream of the dam, a large fraction of the river is forced into the flats while at around 300 km, the water returns from the floodplain. I was wandering if anyone has estimated the residence time of the water into the floodplain. This would be an important parameter explaining potential transformations of the OM in the floodplain.

Based on $\delta^{18}O\text{-}H_2O$ the residence time was estimated at ca. 2 months (Zurbrügg et al., 2012). However, this is a rough estimate, and a manuscript with more detailed water residence times in the floodplain based on the inundation model from Meier et al. (2010) is currently in preparation. We will indicate the residence time from Zurbrügg et al. (2012) in the revised manuscript.

Page 7955, line 16: Patterns of POC and PON loads needs more discussions. The mentioned increase in May 2010 and decrease in May 2009 and in October 2008 is relative to what? What factors are responsible for those increased/decreased trends? Increased discharge, increased concentration or both? I would also like to see a discussion of the relative contribution of DOC and POC as well as DON/PON/DIN to those increased/decreased load patterns. Based on Figure 7a and b, there seems to be a major change in the relative contribution of POC and DOC to the OC load (same for N load) suggesting a dilution effect due to increased discharge below 300 km but also a large retention of the particulate form in the floodplains.

For clarity, the sequence of C and N loads (t d^{-1}) along river will be changed in Figure 7a and 7b, respectively. The load patterns are due to large changes in discharge during the flooding period (see Figure 2 for May 2010) while DOC and DON concentrations (Figure 3a) and POC and PN concentrations (Figure 3b) vary by less than a factor of 2. Because of the decreasing POC and PN concentrations along the river (Figure 3b), settling of particles in the floodplain may be the main process influencing the particle loads.

We will address the point about relative contributions of the dissolved and particulate fractions in the revised version. During the flooding period, the contribution of POC decreases from ~15% at the dam site to ~7% at the end of the floodplain. The same holds for PN, whose contribution decreases from ~40% to ~20%. During the dry season, the particulate fraction is overall higher but, no there is no obvious trend.

Page 7956, line 6: Has anyone estimated the magnitude of OM burial in the Kafue Flats or other similar floodplain systems? It would be good to have an idea on the scale of this process.

We found that the sedimentation rate in open water areas was ~1 mm yr^{-1} on 137-Cs dating of a sediment core (R. Zurbrügg, unpublished data). This is equivalent to 25-42 g C m^{-2} yr^{-1} and 1.9-3.6 g N m^{-2} yr^{-1}, and this is rather low for tropical wetlands which were found 56-94 g C m^{-2} yr^{-1} (Mitsch and Gosselink, 2007) and approximately one forth of temperate riverine wetlands of ~140 g C m^{-2} yr^{-1} (Bernal and Mitsch, 2012). Burial rates in temperate wetlands can exceed 300 g C m^{-2} yr^{-1} (Reddy and DeLaune, 2008). We provide a comparison of burial rates in the revised manuscript.

Page 7956, line 11: How comparable are these estimated yields (representing either 1-3 or 30% of the annual primary production) relative to other similar studies? Citations are needed there if exists to validate and size the findings of this study.

We found no study that related export yields to the net primary production (NPP) of an ecosystem. Yield data are available mostly for large catchments where an estimate of a NPP is not trivial. OC yields for the Amazon were found 2.6 - 5.0 g DOC m^{-2} yr^{-1} and 2-4 g POC m^{-2} yr^{-1}, for the Zambezi River basin 1-2 g DOC m^{-2} yr^{-1} and 0.3-0.5 g POC m^{-2} yr^{-1} (Harrison et al., 2005;Mayorga et al., 2010). Estimates from over 500 catchments range from 1.2-57 g DOC m^{-2} yr^{-1} and 0.4-74 g POC m^{-2}

yr^{-1} (Alvarez-Cobelas et al., 2012). Johnson et al. (2006) found exports of 15 g DOC m^{-2} yr^{-1} which seems somewhat typical for different Amazon catchments (8-19 g DOC m^{-2} yr^{-1}). Waterloo et al. (2006) measured an average of export 19 g DOC m^{-2} yr^{-1} from an rainforest catchment in Amazonia. We will address this in the revised manuscript by better comparing of our data with studies from other systems.

Page 7958, line 21: States that ". . .terrestrial POM is efficiently retained in the ITT reservoir. . .as indicated by intense sediment accumulation. . .". While there is no doubt that the reservoirs can retain large fractions of the particulate inflowing load, sedimentation of 16,000 t C yr-1 as shown in Figure 9a (large fraction of which must be in-situ produced – otherwise the balance would not hold) represent only 16% of the inflowing load of 100,000 t C yr-1. This does not necessarily qualify for intense sedimentation, nor for efficient trapping.

The "sedimentation" flux in Figure 9a was falsely labeled and should say "sediment accumulation", that is, burial. Nevertheless, the lack of a terrestrial signature in the POM leaving the reservoir ($\delta^{13}C$, C;N) compared to sediment trap and sediment data from Kunz et al. (2011) suggests that a large fraction of the riverine POM is trapped. We will change this statement in the revised manuscript.

Unlike for the floodplain, we did not intent to present a complete budget for the reservoir in Figure 9a. It should rather illustrate the relevant reservoir processes and fluxes published in Kunz et al. (2011) and serve as a comparison to Figure 9b. Sedimentation and primary production are not shown in Figure 9a since we do not intend to discuss previous work from our colleagues in detail. A comprehensive discussion on the reservoir processes is beyond the scope of this paper, but we will modify Figure 9 in the revised manuscript for more clarity and make a clear separation between data from Kunz et al. (2011) and own data.

How much of the inflowing OC load of Figure 9a is in particulate form? If I would be to reconstruct the OC mass balance shown in Figure 9a, as it stands there, a surplus of +8 t C yr-1 (100,000 - 16,000 – 76,000) would be suggested. Is this due the not shown C produced by reservoir primary production? If yes, this seems awfully small giving an estimated annual average primary production rate of 280 g C m-2 yr-1 (according to Kunz et al., 2011) and a reservoir surface area of 364 km2. These should be shortly discussed together with potential uncertainties.

The model from Kunz et al. (2011) does not give accurate estimates on the particulate fraction. For comments concerning Figure 9, see answer above.

Page 7960, line 24: What is the difference between the two increased distances: a) from the river to the floodplain; and b) from the shore to floodplain? Give a better explanation.

The axes in Figure S6 and in the text were replaced by a simpler unit.

Page 7961, line 12: This sentence need to be revised as it is not clear wheatear it refer to inflowing loads into the reservoir or out of the reservoir into the flats. Also, why plural ('loads") when latter on (line 14) there is only one value (7900t N yr-1) presented?

We now simplified this paragraph for clarity. The sentence has been changed and we now use the singular term "load".

Page 7962, line 22: States that the exchange between the river and floodplain causes a net export of 35-75 kg C km-2 d-1 in the form of DOC? Is this true? I thought that only 80% of it is DOC.

The wrong numbers for DOC were presented here and have now been changed.

Technical corrections/suggestions

Thank you for spotting them. We included all small corrections indicated by the reviewer and only refer to the questions that require an answer:

Page 7946, line 7: I believe that evidences of pronounced hypoxia were encountered over the "last" 150 km long stretch, after water comes out of the floodplains.

Changed to "…last 150 km long river reach that were dominated by floodplain inflows."

Page 7947, line 23: ". . .water leaving the system. . .". What system? River system, reservoir system? Please specify.

Changed to "leaving the floodplain system".

Page 7948, line 12: describe with full name here and everywhere else (especially throughout 2.2. Sampling campaign,and 2.3. Laboratory analyses) all abbreviations and initials used for the first time, for instance GF. EDTA, NH4, NO2, K2SO4, UV, NaOH, USGS, etc.

We now write the full name of all abbreviations when used the first time. The expressions USGS-41, IAEA-NO3, IAEA-N2, IAEA-CH6 are the official names of isotope standards and we refrained from writing the full names of the two organizations, but we now indicate the compound.

Page 7953, line 21: "During the flooding season, d13C-POC increased by. . .". Which flooding season? Which year? Figure 4 suggest an increased trend during both flooding seasons. If that's the case, replace than accordingly with "During both flooding seasons,. . .".

Changed to "During both flooding seasons… ~2.5‰ …".

Page 7954, line 7: "3.5 ± 0.3 l mg^{-1} m-1" - keep the same unit - see Figure 5a.

"L mg^{-1} m^{-1}" in Figure 5a was changed to "l mg^{-1} m^{-1}".

Page 7954, line 20: replace "was" with "is". Otherwise mention the exact period of your observation when the ". . .stream flow was forced into. . .".

Was changed to "During high flows, a large fraction of the stream flow was diverted into the floodplain between 180 and 225 km, while downstream of the 300 km mark…"

Same at page 7955, line 3 "every year" was added to clarify

Page 7956, line 8: there is a repeatedly use of "flooded area" in the same line. Consider using synonym words, for instance "flooded surface".

The wording was changed as suggested by the reviewer.

Page 7956, line 11: consider re-writing this entire line after the comma. My suggestion: "suggesting that other processes (mentioned above) are dominantly responsible for the fate of the OM in the floodplain".

Was changed accordingly.

Page 7957, line 6: first “and” must be “of”???

The “and” is correct. For clarity, “floodplain soil” was replaced by “soil OM”.

Page 7957, line 22: Even spectroscopic results support chemical data, the fact that DOM is primarily terrestrial is a conclusion. The phrase should read something like: Spectroscopic results not only support the overall conclusion drawn from the chemical data that DOM was primarily of terrestrial origin, but. . . “

Changed to: “Spectroscopic results supported the overall conclusion drawn from the chemical data that DOM was primarily of terrestrial origin, however, they also revealed a microbially-derived contribution.”

Page 7959, line 23: replace “Any” with “Even” and add comma after “more”.

The sentence was rephrased.

Page 7962, line 9: replace “N-fixation rates that high” with “high N-fixation rates”

The sentence was rephrased.

Page 7963, line 9: I suggest starting a new paragraph with “Our study. . .”. Generally, this last paragraph needs a bit of “brush up” for better reading.

This paragraph was edited in the revised version.

Figure 1b, lower graph,: What does “Stage” stands for on the Y axis? Why not “water level”?

Was changed to “water level” for simplicity.

Figure 3 and Figure 4: Why not displaying data in chronological order as one may expect from 2008 to 2010?

The focus of the Results chapter is on the difference between flooding season and dry season rather than on the temporal sequence. 2010 data are presented first because of more complete data set (see comment above).

Caption Figure 9. Be consistent putting a dot (or not) between (a) and (b) (see other figures). Same constancy for using capital letters (or not) when describing superscripts, i.e. “1)”

This was changed accordingly, we now have consistent captions.

Reviewer #2

This manuscript details a study in which the impact of the exchange of riverine water with the bordering floodplain on the riverine POM and DOM is studied. What makes this study fascinating is that the exchange of water has been quantified using O-isotopes and it seems clear that the floodplain should have a major influence on the fluvial OC load.

Thank you for the review of our manuscript. The comments will help us present a clearer and more convincing manuscript.

Though I don't necessarily disagree with the conclusions concerning the sources of DOM and POM, I don't find the arguments as laid out in the paper compelling. The discussion can be so succinct at times that adequate foundation is not provided for a conclusion.

For instance, on pg 7957 it is argued that the delta 13C of the POM, along with its C/N ratio indicates a phytoplankton source with little explanation. In this type of environment the sources could be quite complex and these parameters alone might not allow that conclusion. I could not find measurements of an algal end member to support that claim or even microscopic analyses of the samples, which in my opinion is essential yet trivial to do.

We agree that the data base for the source allocation could be improved, and that POM after the dam may not be exclusively composed of algal biomass. We thus use "dominated by phytoplankton".

In the revised version we will include profiles from Itezhi-Tezhi reservoir (δ^{13}C-POC, δ^{15}N-PN, C:N), behind the dam wall, located 25 km from the two major inflows to the reservoir (Kafue and Musa River) as supplementary information. We will develop the argument that the sample location and the samples elemental and isotopic composition are consistent with OM being primarily phytoplankton-derived. We will also mark the reservoir POM ranges in Figure 8. The surface water samples for June 2008, June 2009 and October 2008 can be used as algal end member since a terrestrial contribution to POM is very unlikely at this site of the reservoir. For the flooding season (May/June), δ^{13}C-POC and a C:N corresponds to the values at the dam outflow (analogous for October 2008). We unfortunately have no microscopic data available; however, we feel the other arguments are sufficiently compelling.

Another example is the observation on pg 7954 concerning the water exchange with the floodplain. The reader is directed to Fig. 2 which provides little detail. There is another paper that is referred to on this topic that presumably provides details. It would add to this paper if a little more explanation were added. A paragraph within the main text summarizing the isotopic approach would be valuable and could serve as a springboard to better integrate the observations.

We will add a paragraph that briefly explains the δ^{18}O-H_2O approach and the results from Zurbrügg et al. (2012).

One striking feature of the isotopic data is their relatively invariant nature as one moves through the system. This point is noted by the authors. Given the presumed changes in processes and sources as transits downstream this is surprising. The explanations offered are unconvincing and ad hoc. I am left wondering if we are missing something important in terms of processes.

We agree that the limited variation in our OM parameter was an unexpected finding given the large changes in the origin of the river water. Our results suggest that (with the parameters measured) DOM is more similar than it does dissimilar. While the finding was indeed unexpected, our explanation is not ad hoc. Rather, suggesting that the DOM from different locations had similar biogeochemical history is the most straightforward explanation. We will keep the reviewers comment in mind as we

continue revising the manuscript, and will consider other processes that could contribute to the small variation in DOM parameters.

In short, more thought needs to go into presentation. The study would be of interest to those involved in river research and could be a valuable contribution to the field if the manuscript could be improved.

We will work on improving this in the revised manuscript.

More specific concerns follow:

Pg 7948: Many types of peristaltic tubing plastics have DOM backgrounds. Were blanks/controls done with the tubing here?

We did not do specific blank tests for the tubing. However, given the reasonably high DOM concentrations we do not feel this could have been a substantial source of contamination. The tubing in the peristaltic pump that experiences mechanical forces was out of Teflon and the extension going to the water out of PE. The tubing was prepared as indicated in the manuscript and flushed entirely 7 times with sample water before takings samples. During sampling the sample contact time with the tube was ~1 min. We will address this further in the revised manuscript.

Sample storage at 4°C for 3-4 weeks seems unwise. Were any controls done to document that the samples did not change?

Because of the remote location, and logistical limitations (transport within Zambia and Zambia-Switzerland), earlier analysis of the samples was not possible. To check for storage, two samples (one river, one floodplain) were analyzed at the arrival at the lab as well as 11 days later. We found no systematic changes in the fluorescence properties of the samples. This is consistent with Yamashita et al. (2010) who found no differences between EEM sample measurements 2 and 4 weeks after sampling.

Filtration using 0.7 um filters will not sterilize samples. Could this have influenced the DOC/DON and spectroscopic analyses?

Using 0.7 µm filtration is very common and allows for comparison with other studies. Moreover, samples for DON and DOC were acidified, which would minimize biological transformations. For spectroscopic analyses, 0.7 µm filtration for EEM is common practice (e.g. Fellman et al., 2011;Stedmon et al., 2003).

Could the sample treatment methods have contributed to the difficult to explain invariance in the isotopic data (see above)?

Samples for DOM parameters (concentrations, $\delta^{13}C$, $\delta^{15}N$, C:N) were filtered acidified and cooled to 0°C on the boat and immediately frozen on land. For analysis methods, samples were treated according to established methods and validated as described in the supplement, e.g. by using DON standards of known $\delta^{15}N$ for yield and isotopic calibration of the $\delta^{15}N$-TDN digestion step, and subsequent $\delta^{15}N$ nitrate standards for the denitrifier method.

Pg 7950, line 20-22: In most cases, the authors are probably safe in assuming that the delta 15N of the TN is representative of the TON but they should be careful. The nitrate can be greatly enriched in 15N in denitrifying systems, thus even a 5-10% contribution to the total can measurably influence results.

Unfortunately, measurements of $\delta^{15}N$ of nitrate using the denitrifier method (Sigman et al., 2001) gave unsatisfactory results because of the low nitrate concentrations. A simple mixing calculation shows that a 5% (=mean contribution of nitrate to TDN during flooding, ~2% in October) contribution of nitrate with $\delta^{15}N$=+22‰ would increase $\delta^{15}N$-TDN by only 1‰. A contribution of nitrate to $\delta^{15}N$-TDN cannot be excluded and we now address this more clearly in the Discussion.

Pg 7952: Provide %C of the suspended load in this section as well (it is in the supplemental section but one might not know to look there initially). %C is diagnostic of source and thus places the other datasets in a better context.

The data on %C and %N of TSS is now presented in Figure 3 and discussed along with the isotopic data of the particulate fraction.

Pg 7956: Do the km-2 units refer to floodplain or watershed area? Watershed would be most typical when referring to a river however the discussion implies floodplain.

The area of 6,500 km^2 is the floodplain delineation which is most commonly used in the literature e.g. (Mumba and Thompson, 2005) and originates from a wetland conservation study (Schelle and Pittock, 2005). The flooded areas have been replaced by the results from the inundation model (calibrated with remote sensed data) of Meier et al. (2010). We use these flooded areas for the yield calculations and will explain that in the revised manuscript.

Pg 7957: 'Higher' or 'lower' delta 13C seems a little ambiguous (and is not traditional (. More positive or negative, or 13C-enriched/depleted are clearer.

"Higher" or "lower" has been removed and replaced as suggested by the reviewer.

There seems to be some confusion in how to interpret the F1 parameter. A 'terrestrial' source is complex in that it may have plant, soil (including microbes) and rock OC. It also seems unlikely that a sharp cut-off in the F1 parameter at 1.4 can be used to distinguish between sources. What exactly is being detected in this case. This is another example where the sparseness of the explanation leaves one wondering if the authors have adequately researched the topic.

In the submitted manuscript, the discussion on the FI was deliberately kept short because of the limited informative value of the parameter for our data set. The source definitions "terrestrial" and "microbial" originate from the parameter definition (McKnight et al., 2001) and two end members that have fulvic acids dominated by terrestrial (FI~1.21) or and microbial (FI~1.55) sources (Cory et al., 2010). The FI has been widely applied as a simple and fast screening method for whole water samples.

We agree that there is no sharp cut-off at 1.4 but intermediate values rather imply a mixture between terrestrial and microbial sources. We now clarify this in Methods and Discussion.

Pg 7959, lines 5-10: Does it seem likely that floodplain and reservoir POM would have the same isotopic signatures? This is not something that anyone would predict. Is another explanation possible?

Along the uppermost stretch of the river after the dam, the POM pool likely consists of the primary production of the reservoir, as indicated by the C:N and stable isotope data that we now present as supplementary information. There is considerable annual variation in the reservoir and spatial variation in the floodplain. However, the ^{13}C-depletion of the floodplain POM suggests that a large fraction of it consists of phytoplankton. We address this point in more detail in the revised manuscript.

lines 10-12: It seems unlikely that periphyton would be a dominant OM source (over the vascular plant sources for example) as implied. What role would in-channel production play in this system?

We have single chlorophyll A measurement from May 2009 that show that primary production is rather low, as expected by the low-nutrient conditions. Chlorophyll A concentrations range from 3.6-4.7 µg L^{-1} along the first 60 km after the dam, decrease to 2.3 µg L^{-1} at 210 km (R. Zurbrügg, unpublished data). For comparison, Chl A ranges 5-20 µg L^{-1} in the Okavango delta (Mladenov et al., 2005) and 1-7 µg L^{-1} in the Tana River (Bouillon et al., 2007).

Periphyton is ubiquitous on the inundated parts of the floodplain grasses and could thus be an important source of river POM. In the course of revising the manuscript, we will improve the discussion on POM sources as suggested by reviewer #1.

Lines 20-28: This section might make more sense if 'terrestrial' and 'microbial' were clarified.

We now clarify 'terrestrial' and 'microbial', see answer above.

Pg 7961, lines 5-17: The use of denitrification rates from remote and extremely large field site (the Amazon) is questionable when estimating a N-budget especially when an N-deficit is calculated and compared to an estimated input. The authors then go on to speculate that the difference may have been impacted by dam construction. The uncertainty in this type of calculation is so large that we don't know what the difference is much less can we argue about what has changed it. This paragraph could be eliminated.

We now consider a denitrification rates from different studies and systems (see comment to the editor below). In the revised discussion we will eliminate the section on dam impacts on the N budget, as suggested by the reviewer.

Lines 18-27: The source of PON varies with the river – in eutrophic settings it is derived from in-channel production. The authors seem to be arguing for intense (rapid) cycling of DON yet this could result in significant variability in isotopic compositions. This is not what they see. Note that they may be correct about the rapid cycling but they don't make a strong case for it. This is another example where the argument could be built better.

We agree that rapid cycling of can have strong effects on the $\delta^{15}N$ of the residual DON as shown e.g. by Schlarbaum et al. (2010). The statement will be changed and the argument revised.

Pg 7962: This N-section borders on speculation as written. Reorganizing the paper around what is known about the system and then bringing in the isotope data to answer questions would make for a stronger presentation. As the manuscript currently reads, data are presented and untested hypotheses concerning what they mean are proposed (and offered as conclusions). This leaves the reader with a feeling of dissatisfaction.

The comments from the reviewers and the editor helped to produce a better revised manuscript. In addition, we now ask more specific questions in the Introduction and clearly distinguish between conclusions and speculations. We also aim to better present the system and emphasize the exchange between river and floodplain.

Comments from the Editor

Dear Roland,
As you know, your recent submission to Biogeosciences has now been reviewed by two referees. Both referees found the data and manuscript to be of considerable interest but listed a number of issues which should be addressed. Based on their comments and my own reading of your paper, I would strongly encourage you to submit author replies, and in a 2nd stage, a revised version of your manuscript.

Thank you for the positive assessment of our manuscript. We hereby provide author replies and will upload a revised version of the manuscript within the requested time frame.

Apart from taking into account the referee reports, I have listed below a number of issues / comments / suggestions based on my own reading of the manuscript.

-d15N data for DON: it might be worth referring to these as d15N of TDN which would technically be more correct (see also comment by Ref#2). The same holds for the term "PON" which is actually PN.

We now write TDN and PN for consistency.

-Introduction and/or discussion: one interesting point here not stressed so far is that many floodplains are typically consider to function as sinks for sediments, organic C and nutrients. It might be worth referring to some of the literature which demonstrates their role as sinks, rather than as sources (as in your study and the Amazon River literature cited). Noe & Hupp (2009), Ecosystems (2009) 12: 728–746 is a good start.

We address this point in the Introduction and the Discussion of the revised version of the manuscript along with the suggestions of reviewer #2 concerning the flow of arguments.

-Methods section: confirm if filters for d13C analyses were acidified ?

Filters for $\delta^{13}C$ were not acidified for analytical reasons. However, carbonates were not expected to be important, as the reservoir was strongly undersaturated with respect to $CaCO_3$. A contribution of IC to POC of 2% or more with a mean $\delta^{13}C$-DIC of -6.4‰ (R. Zurbrügg, unpublished data) would exceed the precision of the Elemental analyzer-IRMS of 0.1‰. The differences that are discussed in the manuscript are in the range of a few ‰. We now clarify this in the Methods section of the revised version.

-Both referees mention a number of issues related to the C and N budgets which are summarized in Figure 9. I have to agree that there are a number of inconsistencies and that uncertainties on some of these numbers should also be mentioned in the text and/or on Figure 9. Some examples:

We realize that Figure 9 has caused some confusion and first clarify its purpose: Figure 9a shows schematically the DOM and POM fluxes through the reservoir and gives some of the C and N fluxes from the model of Kunz et al. (2011) as a comparison to the floodplain fluxes. As mentioned above, we do not intend to discuss these (published) numbers in detail and will change Figure 9 accordingly.

* The C in- and outputs from the reservoir as given in Kunz et al. (2011) are 100,000 ± 86,000 t C y-1, and 110,000 ± 63,000 t C y-1, respectively. These are very high uncertainties which should be mentioned somewhere in order not to give a false impression of accuracy. Also, why is the number mentioned in Kunz et al. (2011) for the outflow of C from the reservoir different from the one

presented here? If new calculations have been made with the same dataset, please mention this (and how they differ).

We now make a clear separation between data from Kunz et al. (Figure 9a) and our data and data from Wamulume et al. (2011; Figure 9b). See also answers to comments of reviewer #1.

* For the N budget, as Ref#2 mentions, please think on the adequacy of using the Amazon data to constrain denitrification in the floodplains.

We now use denitrification rates of different studies to estimate floodplain denitrification in the Kafue Flats. Data from tropical floodplains are sparse, particularly in Africa. For the Okavango Delta, a relatively well-studied floodplain system, no data on denitrification could be found.

* N budget: in the manuscript, you mention "The annual measured N inflows reservoir from Wamulume et al. (2011) which are indicated in Fig. 9a underestimated the actual loads because of the insufficient sampling resolution during the "first flush" of N at the onset of the rainy season. The revised loads are 7900 tNyr 1 based on the model fluxes of Kunz et al. (2011)." If you have more confidence in the latter value, why not mention this on Figure 9? Does the same hold true for the C inflow values mentioned on Figure 9?

See answer below.

Figure 8 in Kunz et al. (2011) mentions 4000 t N y-1 as outflow, your Figure 9 mentions 3100 t N y-1. Please double-check and resolve these apparent inconsistencies and explain where values are derived from in the text.

We now clearly separate the outcomes of the two previous studies (Kunz et al./Wamulume et al.). The numbers from Kunz et al. (2011) are model requirements whereas the data from Wamulume et al. (2011; Figure 9b) are field data. However, the indicated loads are in the range requested by Kunz et al. (3,000-5,000 t N vs. 3,100 t N).

-As a final suggestion (which I try to make for all papers I handle where applicable), I would strongly encourage you to provide the full dataset as an electronic supplement. This is a relatively small effort, but makes the actual data available to the scientific community which is an important long-term benefit.

We support this effort and present concentration and isotopic data as supplementary information.

References

Alvarez-Cobelas, M., Angeler, D. G., Sanchez-Carrillo, S., and Almendros, G.: A worldwide view of organic carbon export from catchments, Biogeochemistry, 107, 275-293, 10.1007/s10533-010-9553-z, 2012.

Bernal, B., and Mitsch, W. J.: Comparing carbon sequestration in temperate freshwater wetland communities, Global Change Biology, 18, 1636-1647, 10.1111/j.1365-2486.2011.02619.x, 2012.

Bouillon, S., Dehairs, F., Schiettecatte, L. S., and Borges, A. V.: Biogeochemistry of the Tana estuary and delta (northern Kenya), Limnology and Oceanography, 52, 46-59, 2007.

Cory, R. M., Miller, M. P., McKnight, D. M., Guerard, J. J., and Miller, P. L.: Effect of instrument-specific response on the analysis of fulvic acid fluorescence spectra, Limnology and Oceanography-Methods, 8, 67-78, 2010.

Fellman, J. B., Dogramaci, S., Skrzypek, G., Dodson, W., and Grierson, P. F.: Hydrologic control of dissolved organic matter biogeochemistry in pools of a subtropical dryland river, Water Resour. Res., 47, 10.1029/2010wr010275, 2011.

Harrison, J. A., Caraco, N., and Seitzinger, S. P.: Global patterns and sources of dissolved organic matter export to the coastal zone: Results from a spatially explicit, global model, Global Biogeochemical Cycles, 19, Gb4s04 10.1029/2005gb002480, 2005.

Johnson, M. S., Lehmann, J., Selva, E. C., Abdo, M., Riha, S., and Couto, E. G.: Organic carbon fluxes within and streamwater exports from headwater catchments in the southern Amazon, Hydrological Processes, 20, 2599-2614, 10.1002/hyp.6218, 2006.

Kunz, M. J., Wüest, A., Wehrli, B., Landert, J., and Senn, D. B.: Impact of a large tropical reservoir on riverine transport of sediment, carbon, and nutrients to downstream wetlands, Water Resour. Res., 47, 16, 10.1029/2011WR010996, 2011.

Mayorga, E., Seitzinger, S. P., Harrison, J. A., Dumont, E., Beusen, A. H. W., Bouwman, A. F., Fekete, B. M., Kroeze, C., and Van Drecht, G.: Global Nutrient Export from WaterSheds 2 (NEWS 2): Model development and implementation, Environmental Modelling & Software, 25, 837-853, 10.1016/j.envsoft.2010.01.007, 2010.

McKnight, D. M., Boyer, E. W., Westerhoff, P. K., Doran, P. T., Kulbe, T., and Andersen, D. T.: Spectrofluorometric characterization of dissolved organic matter for indication of precursor organic material and aromaticity, Limnology and Oceanography, 46, 38-48, 2001.

Meier, P., Wang, H., Milzow, C., and Kinzelbach, W.: Remote Sensing for Hydrological Modeling of Seasonal Wetlands – Concepts and Applications, ESA Living Planet Symposium, 28 June - 2 July 2010, Bergen, Norway, 2010,

Mitsch, W. J., and Gosselink, J. G.: Wetlands, 4th ed., John Wiley & Sons Inc., New Jersey, USA, 2007.

Mladenov, N., McKnight, D. M., Wolski, P., and Ramberg, L.: Effects of annual flooding on dissolved organic carbon dynamics within a pristine wetland, the Okavango Delta, Botswana, Wetlands, 25, 622-638, 2005.

Mumba, M., and Thompson, J. R.: Hydrological and ecological impacts of dams on the Kafue Flats floodplain system, southern Zambia, Physics and Chemistry of the Earth, 30, 442-447, 2005.

Reddy, K. R., and DeLaune, R. D.: Biogeochemistry of wetlands: science and applications, CRC Press, Taylor & Francis Group, Boca Raton, FL, USA, 2008.

Schelle, P., and Pittock, J.: Restoring the Kafue Flats - A partnership approach to environmental flows in Zambia, Proceedings of the 8th International River Symposium, Brisbane, Australia, 2005.

Schlarbaum, T., Daehnke, K., and Emeis, K.: Turnover of combined dissolved organic nitrogen and ammonium in the Elbe estuary/NW Europe: Results of nitrogen isotope investigations, Marine Chemistry, 119, 91-107, 10.1016/j.marchem.2009.12.007, 2010.

Sigman, D. M., Casciotti, K. L., Andreani, M., Barford, C., Galanter, M., and Böhlke, J. K.: A bacterial method for the nitrogen isotopic analysis of nitrate in seawater and freshwater, Analytical Chemistry, 73, 4145-4153, 2001.

Stedmon, C. A., Markager, S., and Bro, R.: Tracing dissolved organic matter in aquatic environments using a new approach to fluorescence spectroscopy, Marine Chemistry, 82, 239-254, 10.1016/s0304-4203(03)00072-0, 2003.

Wamulume, J., Landert, J., Zurbrügg, R., Nyambe, I., Wehrli, B., and Senn, D. B.: Exploring the hydrology and biogeochemistry of the dam-impacted Kafue River and Kafue Flats (Zambia), Physics and Chemistry of the Earth, 36, 775-788, 10.1016/j.pce.2011.07.049, 2011.

Waterloo, M. J., Oliveira, S. M., Drucker, D. P., Nobre, A. D., Cuartas, L. A., Hodnett, M. G., Langedijk, I., Jans, W. W. P., Tomasella, J., de Araujo, A. C., Pimentel, T. P., and Estrada, J. C. M.: Export of organic carbon in run-off from an Amazonian rainforest blackwater catchment, Hydrological Processes, 20, 2581-2597, 10.1002/hyp.6217, 2006.

Yamashita, Y., Maie, N., Briceno, H., and Jaffé, R.: Optical characterization of dissolved organic matter in tropical rivers of the Guayana Shield, Venezuela, Journal of Geophysical Research-Biogeosciences, 115, G00f10 10.1029/2009jg000987, 2010.

Zurbrügg, R., Wamulume, J., Kamanga, R., Wehrli, B., and Senn, D. B.: River-floodplain exchange and its effects on the fluvial oxygen regime in a large tropical river system (Kafue Flats, Zambia), Journal of Geophysical Research-Biogeosciences, 117, 1-12, 10.1029/2011JG001853, 2012.