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Interactive comment on "Seasonal dynamics of carbon and nutrients from two contrasting tropical floodplain systems in the Zambezi River Basin" *by* A. Zuijdgeest et al.

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General comments:

The authors present an interesting study on river biogeochemistry from two Zambezi tributaries. Data and knowledge from African rivers are strongly underrepresented in the global data base and make this study a valuable addition. The study conducted in subbasins of the Zambezi River aims at assessing the effects of floodplains and dams on river biogeochemistry. Understanding of these processes in river catchments is important with regard to what ultimately reaches and affects the coastal ocean.

The paper is generally well structured and written. However, while the set of samples

and analytical data appears to be robust and absolutely worthwhile publishing, the design of the study raises major concerns regarding the two major issues discussed: the effect of floodplains and dams. First, the authors collected data and samples from two stretches of the river passing through floodplains during dry and wet seasons. However, although the importance of the biogeochemical exchange between river and floodplain was stressed by the authors, samples were collected only in the middle of the river. While I agree with the authors that this exchange is important, I would also expect biogeochemical gradients from the river until the landward margin of the floodplain. The study design does not allow accounting for this. Having said this, the least the authors could do is try to find examples from the literature and discuss these. Second, the authors collected data from a floodplain stretch between two dams, but have no data/samples on the respective river portions upstream of the first (ITT) and downstream of the second dam (KG) which makes it difficult to assess the dam effect, in particular with regard to quantification.

Moreover, the discussion of the important issues often remains at the surface and is full of flaws, in particular when it comes to the use of C/N ratios and stable carbon isotopes. You will find detailed comments below. While the authors have a very good set of data and the issues discussed are highly relevant, the story is simply not yet there. Therefore, this paper cannot be recommended for publication in Biogeosciences in its present form. It will require major revisions to make it a robust study and valuable addition to the literature. However, I am convinced that the authors have the means to do so and would greatly appreciate to see this study published!

Detail comments:

Abstract:

The abstract is rather long, in some places a bit confusing and ends with a little uninspired conclusions. I suggest to shorten the detail description of results and to put more effort on the inferences of the study. A final, clear take home message would be

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fine.

P. 10546, l. 5: Why just "hydropower" dams? There are many multipurpose dams which affect river hydrology and biogeochemistry etc.

P. 10546, l. 11 ff.: These paragraphs are rather long and not really clear. In the first sentence you state that the "Barotse plains retain particles", but have "higher annual yields of POC and N. . .than previously reported for the Zambezi. . .". This sounds a bit strange. It must not necessarily be a contradiction, but rephrasing could help make clear what is meant.

P. 10546, l. 23-27: There is a contradiction regarding organic matter (OM) sources. While you first stress the importance of aquatically produced OM during the wet season, you state in the following dominances of C3 vs. C4 vegetation. This does not match and raises a general issue with the use of C isotope composition. The relevance of autochthonous OM production is often neglected. For example, depending on the carbon source freshwater plankton can have the same d13Corg as C3 vegetation. Moreover, rivers do not only transport plant material, but rather larger portions of soil material. That, in turn, often has a d13Corg in between that of C3 and C4 plants.

P. 10547, l. 7-11: So, what are the clear effects? Name them. The following two sentences are rather commonplaces. Be more specific regarding your results. It will make it more interesting for the reader.

Introduction:

The introduction generally looks good.

P. 10548, l. 22 ff.: The first sentence is simply wrong. The type of OM never depends on discharge alone, but mainly on the sources and processes in the catchment. The following examples are quite arbitrary and not representative. Here again, we have the problem with the use of C isotopes; reducing it to C3 and C4 plants is too simple and definitely wrong!

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P. 10549, l. 3-13: I suppose this paragraph should state the research problem, but remains a bit undetermined. It would benefit from clearly pointing out the gap in knowledge that this study wants to close.

P. 10549, l. 14-24: And following up on that, this paragraph can also be a bit shortened and focused on what was done for what purpose.

Study sites:

P. 10550, l. 15-19: What is much more important here and later for budget calculations: was there a change in transported water volume before and after dam construction? Is the water only used for power generation or also for irrigation and/or drinking water?

Methods:

There is no information at all about data quality. Add information on accuracy/precision of analyses. This is important in order to assess the relevance of differences in numbers discussed later.

Results:

In general, all results sections can be shortened. Quite often they contain repetitions, first describing general trends and then repeating them with numbers. I also don't like the style how the isotope data are described. Sometimes they are reported as "water O-isotopic signal", then a depletion of one isotope is mentioned in the same sentence with a simple delta notation. For reasons of clarity and to make it easier for the reader I suggest to use exclusively low or high delta values.

P. 10552, l. 13-14: The distances displayed here do not match with the scale in the inset maps in figure 1. Correct this.

P. 10552, l. 11-16: Why should river constrictions lead to a discharge minimum? One should expect higher velocities/discharge in such a case. The following sentences provide an explanation, but this phrasing is a bit unfortunate.

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P. 10552, l. 19-20: What do you mean by "upstream Zambezi", the Barotse plains? Then say so.

P. 10553, l. 16: What is meant by "organic nutrient species"? Carbon as such is not a nutrient.

P. 10553, l. 23: Interesting to read about loads. But the reader does not get any information how and for which points/locations these loads were calculated. This information must be included.

P. 10554, l. 15-19: The C/N ratio of 166 looks rather strange and results from only two measurements. Although you may find a statistically significant difference, I don't think that it has a real diagnostic value.

Discussion:

P. 10555: The whole discussion on the relevance of hydrology and inundation dynamics is absolutely not convincing. It is based on a mass balance approach with isotope data from another study for one of the flats and almost no data for the other flat. What is necessary for such a discussion is the volume of water moved and the period of inundation. From the discharge data and area of the floodplains the authors have, they should at least be able to calculate roughly the volume of water transported.

P. 10555, l. 20-21: What is meant here "with a floodplain contribution of 16%"? 16% of what coming from where? And following, what is meant by "there was still exchange between the river channel and some permanently inundated areas"? Could it be that lower discharge during the dry season is only related to evaporation?

The paragraph then ends with a few examples from other areas/studies. But what is the inference with respect to own findings?

The whole chapter "5.2 Export and retention behavior" is quite disappointing, because it simply repeats overall results and then speculates about the reasons for the observed behavior. It should be reduced drastically or needs a real discussion of the factors

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mentioned.

P. 10556, l. 14-17: What is the use of discussing concentrations in a chapter that deals with fluxes? This can be deleted.

P. 10556, l. 19: "...Kafue flats were a minor source."? If I can believe table 1 the Kafue flats should be a minor sink for DIN (I read table 1 as follows: wet season 0.1 t N d-1, dry season -0.2 t N d-1; taken together a negative flux should mean retention; right?). It would be helpful to make clear in the legend of table 1 what positive and negative signs mean, i.e. export vs. retention.

P. 10556, l. 19-23: This is hard to understand, the mixing of fluxes and loads and differences between numbers in table 1 and figure 5. I suppose the numbers are correct, but the way it is written up is confusing.

P. 10556, l. 24-25: This is an interesting point, but as is nothing but speculation. It belongs to the issues mentioned above. If the reader should believe in the relevance of Aeolian input, there must be some facts and discussion underpinning it, not just a "most likely" without discussion.

P. 10556, l. 27 ff.: Same issue: "DOC and DIC were retained by both systems, potentially converted..."; just speculation. Provide arguments or delete this. The same holds true for the following statement on p. 10557, l. 3-6.

P. 10557, l. 7-10: This is an interesting point and relates to one of the major issues of the whole paper, the floodplain as a biogeochemical reactor. However, what comes is just a comparison to a few other systems and absolutely no discussion of the "biogeochemical reactor". So, what is happening there? What are the inferences for your own findings?

P. 10557, l. 19-21: If I can believe table 1 the Barotse Plains are a net sink for particulate matter. How shall we understand the statement that the "particulate matter mobilized in the floodplain will end up in the sediments of Lake Kariba"? Again, looking

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at table 1 you may have some mobilization during the dry season, but what would be important to discuss why you find large seasonal differences. You have very interesting findings, but do not discuss them.

P. 10557, l. 25-27: This is rather a commonplace. What I am missing are the inferences for the studied floodplains and what that could mean for the whole river catchment in terms of export vs. retention. What are the controls? How do they vary by season? Are the floodplains a sink or a source and is that different from other rivers?

The style of discussion in the following chapters is rather annoying, every paragraph starts with a conclusion and then tries to bring some arguments for it. In a scientific paper own findings should be presented in context with other studies/areas/findings and discussed and then a conclusion can be drawn on own findings. This is something the authors should correct.

P. 10558, chapter 5.3.1: This whole chapter does not discuss sources of dissolved organic matter as announced in the chapter heading "5.3 Sources of organic matter". It only once mentions runoff from inundated soils, the rest is seasonal variations without further source discussion.

P. 10558, l. 11: Do not use the term "can explain" (here and throughout the whole manuscript!). A person can explain something, but the "source of DON" cannot explain something.

Chapters 5.3.2 and 5.3.3 should be merged. As is they are parameter discussions, but not source discussions. The C/N ratio alone is not suitable for such a source discussion. It is affected by numerous factors, the initial difference between plankton and plants/soils is just one of them. Selective decomposition of OM increases the C/N ratio over time. Adsorption of inorganic nitrogen to fine-grained particulate matter (clays) leads to a low C/N ratio. The C/N range of terrestrial plants and soils is huge. Similarly, using the stable carbon isotope composition of OM alone to distinguish between sources is also of limited value. Also, just using it in terms of contributions of C3 and

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C4 plant material means ignoring all the other sources/factors that contribute to the d13Corg of a specific sample one considers. What you mainly find in river suspensions is not plant debris, but eroded soil (including plant debris and processed plant debris). The d13Corg of soil can be very different from the plant growing on it. Moreover, in the water you find micro- and macrophytes the d13Corg of which varies over a wide range. This can be very important in a river and in particular in floodplains. Of course, you can have a lot of microalgae (or not) depending on nutrients and turbidity etc. Moreover, I have myself observed macroalgae and water hyacinths in massive amounts in tropical rivers and reservoirs, all of them aquatic plants with a d13Corg that falls in the range between terrestrial C3 and C4 plants. All this is not mentioned in the discussion in this manuscript. And what is also mandatory for such a discussion is to have stable isotope data from the various OM sources you find in the area, not just literature data. Are there no stable isotope data available for the soils and terrestrial and aquatic plants from the area? Of course, if these data are not available, you have to use literature data, but these are often of limited value. Some interesting information from the area (resulting from other publications of the same group) is given (p. 10560, l. 11-13 and l. 19-26), but the own data are hardly discussed in the context of those observations. Take this up and develop the scenario which explains the observations made.

Chapter "6 Conclusions" is quite long and rather a summary with only a few conclusions. If it should stay like that, it should be called "Summary and conclusions". Moreover, in some places it is not clear and/or simply wrong.

P. 10561, l. 14-15: What is meant by "yields"? Do you want to say that the Barotse Plains have a higher relative export per unit area and time? Then say so.

P. 10561, l. 18-19: What is this? If I take a look at table 1 the Kafue Flats are doing exactly the opposite, with a small retention during the dry season and a high export during the wet season they seem to be a net sink for dissolved and particulate matter on an annual basis.

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P. 10561, l. 19-23: see my previous comment. This is one possibility or part of the explanation, but not the whole truth.

P. 10561, l. 24 ff.: As mentioned before, this is just part of the story and therefore cannot remain as a conclusion.

P. 10562, l. 3-7: This is definitely wrong. The seasonal difference in C/N ratios is fairly small and it is absolutely in the range of aquatic plants. Of course, variations in soil OM contributions may play a role, but there may be other factors (see above).

P. 10562, l. 9-14: This is an interesting point, however, the reservoir effects were hardly discussed before and this seems to be rather a conclusion of the cited paper.

P. 10562, l. 15-16: No, it has not been shown. It has just been mentioned and was not really discussed. As such it is just an unjustified assertion. Nevertheless, I agree with the authors that this could be an important factor.

P. 10562, l. 17-19: No. The only difference one can see is in the seasonal discharge pattern and the change in the Kafue Flats before and after dam construction. However, the data presented in this study do not give any hint on specific transport or export pulses of dissolved and particulate matter besides the general dry vs. wet season differences.

Tables:

Table 1: Clearly designating positive and negative signs to the terms "export" and "retention" (or similar) would help the reader to understand easily what is meant when the text refers to the numbers in the table.

Table 2: What is the use of this table? The numbers are to some extent mentioned in the text, but not really discussed. Why were (only) these rivers chosen and not others, for example, non-tropical rivers? Of course, it contains interesting information, but it is hardly used in the discussion. If it will remain as marginal as is now, it is sufficient to mention the numbers in the text and delete the table.

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Figures:

In general, I find the choice of colors and symbols a bit unfortunate, the differences are fairly small (e.g., black vs. dark blue) and they do not allow to distinguish easily.

Figure 1: The distance scales in the floodplain insets do not match with distances mentioned in the text. What do the red symbols represent?

Figure 4: I think, what you have there is not PON, but PN.

Figure 5: Very hard to figure out what is what. Do not use grey scales, but color. In the case of nitrogen (figures e-h), does it mean there is no dissolved inorganic nitrogen?

Figure 6: Symbols and color are too similar to distinguish easily. Be careful with the legend. What you show is the ratio of particulate organic carbon to particulate total nitrogen, but the ratio of dissolved organic carbon to dissolved organic nitrogen, right?

Figure 7: I like the idea of having some kind of visual summary! However, as a reader I would prefer not to see the numbers again, but instead I would like to see the differences in the various sources/processes, e.g. export/retention, terrestrial/aquatic OM etc.

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