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Interactive comment on "Along-stream transport and transformation of dissolved organic matter in a large tropical river" *by* Thibault Lambert et al.

Anonymous Referee #2

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Comments to Author

Summary: In this manuscript the authors present new DOC and DOM composition data from one of the World's largest tropical rivers: the Zambezi River. Samples were collected during both dry and wet seasons and along the river and one of its tributaries. The results indicated clear seasonal differences in sources and processing of DOM as well as down-river shifts in concentrations and composition. "Humic"-like DOM dominated in headwaters close to forests and at wet conditions when wetlands were dominating sources of DOM. In contrast, at dry conditions the DOM composition shifted towards more aquatically produced, or influenced, material. The authors claim that these differences are primarily driven by shifts in discharge, which influences connectivity with e.g. wetlands, and water residence times. As has been noted before, the effect of reservoirs or lakes have a particularly significant role in increasing water

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residence times and thereby DOM composition and concentrations.

Contributions: Although the patterns presented and conclusions drawn are not revolutionary, they are indeed important since this type of data from tropical rivers is rare. In addition, the results largely confirm previous interpretations of DOM dynamics in boreal and temperate areas. This is interesting since it suggests that, although the details may differ (e.g. microbial community composition), the large-scale governing processes and functioning are similar across biomes.

The manuscript was a pleasure to read. After having reviewed several poorly written manuscripts recently, it was a joy to see a well written and logically organized text. Still, I do have some minor remarks detailed in a number of general and technical comments below.

General comments:

-The description of some of the methodology requires additions and clarifications.

-The use of some terminology is confusing (not uncommon when it comes to this type of terminology) and I suggest clarification. One clear example is the apparent dichotomy between terrestrial and microbial, which is clearly misleading since substantial portions of DOM may be of terrestrial microbial origin.

-The relationships between DOM properties and landscape characteristics is interesting, but presented in the Discussion section. I suggest the authors add a paragraph or two about these results in the Results section.

Altogether, this manuscript is a valuable addition to the scientific field and I support its publication in Biogeosciences. The science is as far as I can tell sound and well communicated. I recommend minor revisions of the manuscript before the editor considers publication of the manuscript.

Technical comments:

Abstract (why no line numbers in the abstract?)

Line 13-14: You write “terrestrial DOM dynamics shifted from transport-dominated during the wet seasons towards degradation”. I don’t think this terminology matches; what do you mean “towards degradation”? Do you mean that it shifted to a state dominated by in-stream processing?

Introduction

Line 41: This is only partly true. Sure, DOM composition controls reactivity but there are other factors that may be equally important. You identify one: water residence times. However, there are others as well, see e.g. Marín-Spiotta, E., K. E. Gruley, J. Crawford, E. E. Atkinson, J. R. Miesel, S. Greene, C. Cardona-Correa, and R. G. M. Spencer (2014), Paradigm shifts in soil organic matter research affect interpretations of aquatic carbon cycling: transcending disciplinary and ecosystem boundaries, Biogeochemistry, 117(2-3), 279-297, doi: 10.1007/s10533-013-9949-7.

Line 71: ultraviolet

Line 74-75: I know this terminology is common, but it is rather misleading, which I often point out. Terrestrial vs. microbial is not a dichotomy. On the contrary, much DOM from the terrestrial environment is of microbial origin. I think it is better to call them terrestrial and aquatic inputs.

Line 86-88: I don’t know if the use of prepositions is correct here. I suggest changing to “. . .drivers of downstream patterns in DOM at the scale of a large tropical river, with a specific attention to the. . .” Materials and methods

Line 91: northwestern Zambia

Line 100: If it is a single peak it is not bimodal. A bimodal distribution has two peaks.

Line 103: I suggest changing the comma to a semi-colon: “. . .whole catchment; forests (20%). . .”

Line 128-130: I suggest you move the year before the parentheses. Now you interrupt "the flow". So e.g. "...wet season 2013 (6 January to 21 March, n = 41) and dry season 2012 (..."

Line 140: what do you mean by "conditioned"?

Line 141: Did you use any blanks? I am always suspicious when filters made by organic compounds are used for DOM analyses.

Line 148: Were the DOM samples kept cold during sampling and transport? Due to logistical reasons I guess not (and you added phosphoric acid) but could be worth noting. Any potential effects of this sample handling? In addition, where were the analyses (concentrations, isotopes, FDOM, CDOM) performed? In Belgium?

Line 151: Do these uncertainty bounds include both accuracy and precision? Relative which standard are carbon isotope values reported?

Line 171-173: Again a somewhat confusing terminology. Is there a dichotomy between aromatic and hydrophobic? Is it aromatic vs. aliphatic?

Line 172: "...indicative of the presence..."

Line 193: Should this be "Raman units"?

Line 196: "The PARAFAC model was using..."

Line 197: This is repetitious so I suggest adding "Furthermore, the PARAFAC..."

Line 200: "...a two-year monitoring..."

Line 210-211: Here is terrestrial vs. microbial again. I suggest changing this terminology.

Line 214: Define PCA

Results

Line 223-224: "…one dry season; the two wet seasons' data…"?

Line 226: "…during the dry period…"

Line 262-263: Remove "a" before "maximum" and "minimum"

Line 266: "globally" seems strange here

Line 267: I guess this should read "except"

Line 277-278: Here is terrestrial vs. microbial again. "aquatic microbial" would be fine

Line 283: I found "corresponding river sections" unclear. Could you clarify?

Line 288: "as" seems out of place here. Perhaps "…downstream concurrent with DOC concentrations…"

Line 318: Do you mean "all samples during the dry season"? I found this unclear.

Line 319: what other variables?

Discussion

Line 328: Do you mean "conversely" instead of "inversely"?

Line 340-343: Perhaps, but from the figure it looks like C1, C2 and C3 are more related to PC2.

Line 348: "in" instead of "of"?

Line 356-357: "…in the northern part of the basin at the headwaters of the Zambezi to grasslands…"

Line 370-371: Aren't these results and should therefore be presented in the Results section?

Line 393: This only applies to water residence times, not necessarily solute residence times since they are dependent on vertical fluxes and in-stream recycling as well.

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Line 397: Why only photodegradation? This should also include microbial degradation.

Line 403-404: "...(1) increasing water levels mobilizes a greater proportion of terrestrial DOM and (2) higher water velocities..."

Line 409: What does "in which" refer to? I found this sentence unclear.

Line 420-422: Is it more likely that this is due to macrophytes than to algae? What about CO2 evasion?

Line 434-436: This agrees with work in temperate/boreal systems, see e.g. Winterdahl, M., M. Erlandsson, M. N. Futter, G. A. Weyhenmeyer, and K. Bishop (2014), Intra-annual variability of organic carbon concentrations in running waters: Drivers along a climatic gradient, Global Biogeochemical Cycles, 28(4), 451-464, doi: 10.1002/2013GB004770.

Line 437: According to Table 2 this is a 1.5 year long monitoring.

Line 446-448: This is interesting! Could you then estimate the loss/production of C in the reservoir by using CO2 and CH4 data?

Line 450: "...sources to sinks..."

Line 461-462: See also Fiebig et al. (1990), Dosskey & Bertsch (1994) or Hinton et al. (1998).

Fiebig, D. M., M. A. Lock, and C. Neal (1990), Soil water in the riparian zone as a source of carbon for a headwater stream, Journal of Hydrology, 116(1-4), 217-237

Dosskey, M. G., and P. M. Bertsch (1994), Forest sources and pathways of organic matter transport to a blackwater stream: a hydrologic approach, Biogeochemistry, 24(1), 1-19

Hinton, M. J., S. L. Schiff, and M. C. English (1998), Sources and flowpaths of dissolved organic carbon during storms in two forested watersheds of the Precambrian Shield, Biogeochemistry, 41(2), 175-197

Line 465-466: There are several references for this; the Winterdahl et al. (2014) paper referred to above is another.

Figure captions

Line 728: "…upstream of their…"

Line 746: Remove "wet"

Line 754: This is really exports. Fluxes are technically export per unit area.

Line 755-756: "…exports at the same location between wet and dry seasons."

Table 1: Very interesting!

Table 2 Line 763: "…during a one and a half year monthly…"

Figure 7: Are these all sites? The number of sites in the Zambezi River seems few compared to other figures. Is this a selection of sites? If so, based on what?

Figure 8: This is rather DOC export. Flux is export per unit area.

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