

Interactive comment on “Organic carbon and nitrogen export from a tropical dam-impacted floodplain system” by R. Zurbrügg et al.

Anonymous Referee #2

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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.

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

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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.

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.

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.

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.

More specific concerns follow:

Pg 7948: Many types of peristaltic tubing plastics have DOM backgrounds. Were blanks/controls done with the tubing here? Sample storage at 4oC for 3-4 weeks seems unwise. Were any controls done to document that the samples did not change? Filtration using 0.7 um filters will not sterilize samples. Could this have influenced the DOC/DON and spectroscopic analyses? Could the sample treatment methods have contributed to the difficult to explain invariance in the isotopic data (see above)?

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

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can be greatly enriched in ^{15}N in denitrifying systems, thus even a 5-10% contribution to the total can measurably influence results.

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.

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.

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

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.

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?

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?

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

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

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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.

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.

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.

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