

## ***Interactive comment on “Shift in the chemical composition of dissolved organic matter in the Congo River network” by Thibault Lambert et al.***

### **Anonymous Referee #1**

Received and published: 20 July 2016

This manuscript examines the chemical composition of DOM in the Congo River basin. The data presented is predominantly optical (CDOM/FDOM) alongside <sup>13</sup>C-DOC and DOC concentration data and I have major concerns with the optical data quality from the study that is likely unfortunately beyond the authors control. This leads to issues with data interpretation as well as clear overinterpretation of the data with respect to processing for which there is no evidence presented. The manuscript is clearly organized but there are a lot of mistakes with English language throughout that need a careful read through before it would be suitable for publication. Please find below a list of major / minor comments:

Line 17: You can't have a DOM concentration – just DOC.

Line 19: “stretch in” is poor English. I'm not editing English throughout but there is a

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lot of minor editorial English correction required.

Line 20: “second river in the world” for what? Discharge, basin size, number of people?

Lines 23-28: This is all speculation for which no evidence is presented. I’ll come back to this below.

Lines 101-105: So what is the difference in WRT between the two seasons? If you are going to postulate WRT as a major driver it would be good to know at least how much it varies.

Lines 132-135: Samples were collected during the declining limb of a secondary discharge maxima which has its source in the southern part of the Basin and so this makes sense for comparison for samples near the mouth to the major discharge maxima. As you move upstream towards Kisangani on the map how is seasonality changing there for example – does this hydrograph have any bearing on what is happening there? It is unclear to me if this hydrograph (i.e. timing of high and low waters) is the same for all the sites – it seems not from what is written so what is really been compared in terms of seasonality / WRT?

Lines 141-149: Samples for DOC and  $^{13}\text{C}$ -DOC were filtered and acidified so were stable. Samples for optical analysis were just filtered – were they then refrigerated or frozen – it is unclear? A major concern here is that these samples were just filtered (even at 0.2 $\mu\text{m}$  we see 3% of the microbial community get through the filter and it rebounds in a few days) and then basically were degrading until they were analyzed. Please provide details of how samples were stored, I appreciate this is probably a logistically very difficult part of the world to work in but more details on sample storage and analysis are required to assess the validity of the optical data.

Lines 204 -207: SUVA is a linear sliding scale, there is no hard and fast cut off between aromatic and aliphatic . . . higher values = greater percent aromaticity.

Lines 378-383: A general increase in DOC is reported along the axial transect related

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to inputs from flooded forest (Fig 4). The authors are stating that flooded forest extent controls DOC export, which makes sense, and so how then does it not control DOM composition? If the composition is varying independently of DOC concentration you can not invoke processes like photochemistry as that would also impact DOC concentration . . . This gives me concern about the DOM composition data as its storage history is not clear.

Lines 397-436: This is all highly speculative and ok so WRT is changing but by how much? TSS and phytoplankton growth are leading to less light penetration but in the Congo your light penetration is likely controlled by CDOM as its low TSS year round apparently. So if you have high CDOM year round (i.e. your river is colored) you have lots of substrate for photochemical reactions but its self regulating as your absorption is so high in the water column that only the very surface will degrade . . . when you look at any reference except the Cory Science paper they see very little evidence for photochem in freshwaters . . . they are typically either mechanistic (i.e. we put this in a bottle and degraded it) or they are in coastal systems where your colored waters are diluted out in optically clear marine waters and so see more light (i.e. self-shading goes away). I'm very skeptical that in a colored river like the Congo photochem actually can meaningfully degrade DOM so it shows up compositionally between seasons – maybe though if the WRT increases by weeks / months it could but this is not described.

Lines 467-551: Again this whole section is really speculative with just the optical data to suggest things – there are no incubations to support any of the trends. I appreciate the authors are just trying to explain their data but in reality its always fairly aromatic dominated. When I look at all the data I see a highly terrestrially dominated system and I'm not sure I'd dare get too much into seasonality with just two time periods. This needs scaling back to reflect this as a minimum.

Lines 552-583: Again highly speculative – isn't the Cuvette constantly connected and so its not like it flushes like the varzea on the Amazon? If its always connected shouldn't the DOM coming out of it always look compositionally similar and so there should be

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no flood pulse?

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