**Biogeosciences
Discussions**

Interactive comment on "Linkage between the temporal and spatial variability of dissolved organic matter and whole stream metabolism" *by* S. Halbedel et al.

Anonymous Referee #3

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General comments: Overall, I think that this manuscript is potentially interesting for fluvial ecology and biogeochemistry. However, I agree with the main concerns of the other referees, namely the poor language of the manuscript and the highly speculative nature of the discussion. With regard to the first concern, I think that the manuscript should be thoroughly revised by a professional native English speaker. The poor language and style make the manuscript difficult to understand in some parts. Note that I have not added language corrections in this review. With regard to the second concern, I think that the study design and results obtained are not enough to test the proposed hypothesis. I think that showing a significant correlation (without all data) between some variables of DOM composition and metabolism is not enough. In addition, in case we

would admit some causality in this correlation, it is unclear if DOM composition controls stream metabolism or vice versa. Nonetheless, I think that the data shown are interesting and publishable if the objective is toned down and the hypothesis is reformulated. Finally, I think that the manuscript is excessively long in some parts and would greatly benefit from focusing more on the main objective.

Technical corrections: P18255, L20-21: I suggest "biochemical composition" or just "composition". P18255, L22: This sentence and overall paragraph are disconnected to the previous paragraph. P18256, L3-4: This sentence contradicts at least in part the last sentence of the following paragraph. If there are studies reporting results on this issue, they should be shown here with references. P18256, L5: In this context the term "autotrophic" rather than "phototrophic" is used. Check throughout the manuscript. P18256, L29: It is unnecessary to evaluate the method here. I suggest deleting this sentence. P18257, L11-22: For a good overview of the effects of agriculture on DOM amount and composition, I suggest referring to Graeber et al. (2012) Science of The Total Environment 438: 435–446 and references therein. The Graeber et al. study was done in catchments located near your catchments. P18258, Study area: It would be nice to see the % land use covers of each catchment. P18258, L15-16: I am not convinced about the classification of the streams in "non-forestry" and "forestry" streams. Also, these terms are not kept consistently throughout the manuscript. For instance, in L20 the term "open-land streams" is used. I suggest using the same nomenclature throughout the text, tables and figures. In addition, it seems to me that these streams may be rather classified into "open canopy" and "closed canopy" streams. Would it make sense? Related to this, it would be nice to see data on the % canopy cover in each stream, or at least add the data on PAR for example in table 1. P18260, L13-15: Unclear why samples were taken twice a day and taking into consideration the travel times. Please clarify. Also please clarify which samples were used for further data analysis. Where all this samples pooled? How many replicates were used? This information should be provided in figures (SD, etc.). P18260, L13-15: What does "if necessary" mean? Please clarify. P18260, L18: Why did you use GF/F filters of 0.7um

pore size? DOM and DOC are usually considered after filtering with 0.45 um pore size. Please justify. P18260, L2: Were the reach lengths chosen based on water travel times? P18260, L6-7: How were the DO sensors calibrated? P18260, L16-17: I am concerned about the way reaeration was measured. Apparently the conservative tracer (NaCl) was injected as a slug, while the propane gas was injected at a constant rate. For this type of measurements usually both tracers are injected concomitantly at a constant rate. With the slug it seems relatively easy to correct for dilution between the top and down stations but it seems not so easy to correct for dispersion. Please clarify. Can you provide a reference? P18261, L21: Was the area estimated as reach length X mean reach width? Please specify. Also, how many transects were used for width and depth measurements? P18262, L10-13: This paragraph is quite disconnected from the previous. P18263, L2: The correction for inner-filter effect is usually done based on the absorption. Please clarify. Also, did you minimize potential effects of pH and temperature on fluorescence measurements? P18264, L11-12: If you already measured DOC concentration, this measure of DOM concentration seems unnecessary. Also, why did you not measure absorbance spectra in order to estimate absorbance indexes (e.g. SUVA, spectral slope, etc.)? These could have added in the characterization of DOM composition. P18264, L16: Why both parametric and non-parametric analyses? It seems that you later only use parametric correlations. P18266, L25: If the correlation is also good for TP why is only the correlation with light shown (Fig. 5)? P18270, L14: I suggest adding "and carbon" after "nutrient". P18271, L3-4: This sentence sounds strange. Eutrophication supports primary production? Please rephrase. P18271, L19-20: Many studies have shown this connection between light availability and periphyton productivity. P18271, L21-22: I do not agree. I do not think that this correlation is indicative of P limitation. P18272, L3: Speculative. Tone down. Further down you admit that "these complex relationships make the direct proof of the linkage between the DOM composition and whole stream metabolism difficult". P18272, L26: What do the authors mean by "stream ecosystem function as a whole"? It seems rather exaggerated in this context. Fig. 3: I suggest including these data in table 1 and erasing this

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figure. These data are just for characterizing the study sites. If kept, it would be nice if breaks in the y-axis could be added for NH4, TP and NO2. The values of NO2 ad NO3 could be added and merged into one column if these data are put in a table. Fig. 5: It would be nice to see the streams separated with different symbols or colors. Fig.7: All 3 correlations seem statistically significant. Another thing is that some of the data are not normally distributed and fail to meet the assumptions of parametric statistics. Please clarify. Fig. 8: The significant correlations between parafac component ratios and fluorescence indexes seem quite logical since those indexes are often estimated from the ratio between fluorescence-emission peaks. So, how do these correlations advance our knowledge of DOM dynamics in this system?