

## ***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 #2**

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General Comments:

The focus of ‘Linkage between the temporal and spatial variability of dissolved organic matter and whole stream metabolism’ by Halbedel et al. was to discern how stream metabolism influences DOM composition across stream types and seasons. Stream metabolism was measured across several seasons in two forested and two non-forested streams. CDOM and resulting PARAFAC analysis was used to quantify DOM composition that was compared to land use and production and respiration ratios (P/R) from stream metabolism measurements. Linking stream metabolism and DOM composition is an interesting and valid avenue of research. Unfortunately, I don’t

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think the work presented in this manuscript executed the intended goal of the paper. The authors found some interesting correlations between DOM composition and P/R, however correlation does not necessarily mean causation. More experimental work or data would be required before justifying identification of any mechanisms driving DOM composition. My major concern is the difficulty I had reading this manuscript. While the English grammar was not all together bad (English is my first language), the writing itself could have greatly benefited from major editing prior to submission. Overall, the writing needs to be more concise and clear. Sentence structure needs work, more clearly thought out paragraphs (e.g. topic sentences), and overall structure to the paper needs streamlining. The lack of editing of this manuscript is a shame given that the poor writing has greatly over-shadowed the science.

Specific Comments:

1. The authors group the streams into ‘forestry’ and ‘non-forestry’ streams. If the ‘non-forestry’ streams are not forested, what are they? Give the stream types the appropriate name or use some sort of land-type coverage (e.g. % agriculture or % forested). 2. Furthermore, was classifying the % land use considered as part of the analyses given the goal of the research (e.g. land use and metabolism on DOM composition)? 3. In regards to the metabolism measurements:  $k$  should be included in the metabolism equations, but is missing. Also given the possible errors associated with measuring metabolism in streams, did the authors consider another method of calculating GPP and ER (e.g. Holtgrieve et al. 2010) to incorporate measurement error? How were the DO loggers calibrated? 4. I am not an expert of EEM’s and PARAFAC analysis, but I have not come across the use of component ratios (essentially ratios of ratios, right?) in the literature before. I think this needs further explanation (and should be explained in the methods).

Technical Corrections:

The paper needs major re-writing, so I have not addressed the issues with grammar,

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sentence structure, and overall clarity, but here are a few of the technical corrections I found:

1. 18254 – line 9: ‘The whole stream metabolism...’ This is awkward. Stream metabolism or whole stream metabolism is better, remove ‘the’ throughout.
2. 18256 – line 6: ‘raising agricultural...’? I think you mean increasing instead of ‘raising’?
3. 18265 – line 10: Nutrients might be lower than Hassel, but regarding the other streams as ‘nutrient poor’ is not justified.
4. 18269 – line 7: The justification of excluding summer data was because of high nutrients and precipitation? This justification is unclear.
5. 18273 – line 16: I follow your discussion here – but why would the top layer of the soil affect stream metabolism? Unless the top layer of the soil is in the water?
6. Figure 4 – I think it would be helpful to include stream type here (similar to Figure 3)...

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Interactive comment on Biogeosciences Discuss., 9, 18253, 2012.