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Interactive comment on "DOM quality in a peatland and forest headwater stream: seasonal and event characteristics" *by* Tanja Broder et al.

Anonymous Referee #2

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The study of Tanja Broder, Klaus-Holger Knorr and Harald Biester used spectrofluorometric indices to relate the quality of DOM to potential processes and hydrological sources of DOM generation. I recommend addressing the following issues in a revised manuscript:

The study is intended to elucidate the DOM quality and dynamics in two contrasting catchments. The authors expect "water level, temperature and precipitation to be the main controls on stream DOM . . .". I would acknowledge if the authors provide a more specifically defined working question/hypothesis along which the results and discussion can be organized. How can the DOM of both catchments differ?

Samples were taken by an automated sampler in a six day interval and the filtered samples were stored at 4°C in the dark. How long were the samples stored until analysis?

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Although filtered, the samples were probably not sterile. Bacteria can degrade organic carbon even at low temperatures. Have the authors tested if the DOC composition remained unchanged?

UV absorption characteristics were used as indicators of DOM composition (absorbance at 254 nm, spectral slope ratio). Besides DOM, however, dissolved iron exhibits significant UV absorption. The dynamics of iron is also related to hydrology, i.e. flood events can be associated with high iron concentrations. Changes in UV absorption may reveal changes in DOM quality but at the same time they can reflect different contributions of dissolved iron. How the authors think about that?

The fluorescence index (FI) differentiates between vascular plant derived (FI 1.3 – 1.4) and microbially derived (1.7-2.0) DOM. FI values >1.7 were interpreted as microbial DOM (p. 5 lines 15-20). Can the authors exclude that non-vascular plants (mosses) contributed to DOM generation? Later in the manuscript a contribution of Sphagnum is specifically discussed with respect to SUVA but not to FI (p. 9 lines 15-31).

In the results and also in the discussion sections, fluorescence indices and absorption values were often reported to be higher or lower when sites or situations were compared. However, I missed statistics providing a little more confidence if these differences are significant. I recommend including a table/figure, summarizing the main results as well as the levels of significance. This table/figure along with a hypothesis can be used to guide the reader through the results and discussion. I found it less convenient to work through the detailed description of the results. The figures 2, 3, 5, 6, 7 and 8 look more or less similar, which makes it not easy to keep the most important results in mind.

Further comments The title: Please refer more directly to the outcome of the study p. 1 lines 22-24: The concluding sentence of the abstract “Our study demonstrated that DOM export dynamics are not only a passive mixing of different hydrological sources, but. . .” is unclear. Now the reader expects a statement why it is not a passive mix-

ing or how the process can be characterized instead. However, it is only concluded that "...assessing DOM quality can greatly improve our understanding...". Please include here the most relevant result that improved our understanding. The sources and the quality of DOM appeared to be highly variable within events depending on runoff generation. What are the consequences for sampling/monitoring programs?

p. 2 line 28: "aromatic or humic", is there a difference? p. 2 line 33: groundwater DOM is of smaller size and mostly of microbial origin, please include a reference p. 4 line 29: The near UV includes light from 300 – 400 nm, 254 nm is in the middle UV range p. 7 line 9: Figure 5 instead of Figure 6? p. 8 lines 10 and 16-17: I had problems to relate these statements to the figures p. 9 lines 6-9: A confirmation of the suitability of fluorometric indices appears difficult without independent methods (e.g. isotopes, mass spectrometry). It is problematic to conclude that an increase in aromaticity is caused by an increase in apparent molecular size if the latter is not measured. p. 14 line 1: "The export of labile, protein-like DOM was specific..." I suggest being more cautious with characteristics of DOC that have not measured directly (e.g. "labile"). See also P. 9 line 5 "...specific strong microbial DOM signature...".

In Figures 1, 3, 5 and 6, the individual symbols in highlighted boxes (events) were difficult to distinguish.

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