Review of Gandois et al. *From Canals to the Coast: Dissolved Organic Matter and Trace Metal Composition in Rivers Draining Degraded Tropical Peatlands in Indonesia*

The manuscript by Gandois et al. examines dissolved organic matter characteristics together with trace metal composition for a peatland-draining river system in Indonesian Borneo. These results add further to our understanding of tropical peatland river biogeochemistry, and in particular the combination with trace metals makes this an important contribution that also fits the topic of the special issue very well. The manuscript is written well, the data are presented clearly, and they are discussed appropriately. I only have a series of relatively minor comments.

1) My main comment concerns the fluorescence index, which shows surprisingly high values, but also spans a large range. The canonical interpretation is that FI < 1.4 indicates terrestrial fulvic acids, while FI > 1.4 indicates microbially-derived fulvic acids (Cory et al. 2010), at least if instrument-specific spectral corrections are made. Here, most data even for the blackwater river are above 1.5. If instrument-specific correction factors were not applied, then it might be better to calculate the FI with lower emission wavelengths (e.g. Kida et al. 2018). Regardless of the correction factors, I think the high and variable FI data should be discussed in a bit more detail – maybe FI is not the most useful measurement to identify terrestrial *vs*. microbial DOM? We also found a large range in FI in our river data in Sarawak, with some rivers having FI > 1.4 despite predominantly conservative transport and no indications of strong microbial DOM processing in the rivers (Zhou et al. 2019 in this special issue). I feel therefore that the conclusion in Lines 242 ff. of extensive microbial processing in some locations is perhaps somewhat questionable. What I would recommend is that the authors use their absorbance data to calculate CDOM spectral slopes for 275–295 nm and 350–400 nm. The slope at 350–400 nm is usually found to increase with microbial processing, while the slope at 275–295 increases upon both photodegradation and microbial degradation (e.g. Helms et al. 2014; Hansen et al. 2016; Lu et al. 2016). This would provide an additional indication of whether microbial processing really is taking place, or whether perhaps the FI is a problematic measurement.

2) Line 24: “characterised the characteristics” could perhaps be phrased more elegantly

3) Line 32: significantly higher than what? Is this compared to the whitewater river, or is this a statement in general about blackwater rivers compared to other rivers?

4) Line 75: what is meant by “re-emission” as opposed to just emission? Isn’t this simply referring to emission of CO_2 after DOC degradation?

5) Line 82: what is meant by “with contrasted effects on optical properties”?

6) Line 118: please give the duration and temperature at which the filters were baked

7) Line 120: nutrient analysis is mentioned here, but the data are not shown. This is a shame, these results would be useful if they can be made available, especially since the authors mention the hypothesis that biodegradation might be nutrient-limited.

8) Throughout the manuscript and figures, Cl should be changed to Cl^-, since it's referring to the chloride ion, not to chlorine.

9) Lines 142–149: were the EEM data also spectrally corrected using instrument-specific correction factors?

10) Line 163: "anoxic" is the wrong word, the rivers clearly do contain measurable amounts of oxygen. Hypoxic would be a better choice.

11) Since the authors have dissolved Fe measurements at all their stations, I would recommend that they estimate how much of the absorbance at 254 nm might be due to dissolved Fe(III), using the relationship in Fig. 1 of Poulin et al. (2014). I did a quick estimate based on the mean SUVA, DOC, and Fe reported in Table 1, which suggests that the errors in SUVA are on the order of 5% or less, but it is important to have more data available about these potential interferences.

12) Lines 230 ff.: the increase in C1/C2 ratio is interesting, and in fact our photodegradation experiments with peatland river water in Sarawak provide direct experimental support for this interpretation (Zhou et al. 2019). The components 1 and 2 in the present study are quite similar to C1 and C2 in Zhou et al., and in all of the experiments the ratio of C1/C2 increases upon photodegradation, especially in the blackwater river.

13) Line 250: The reference to Wickland et al. has the wrong year, it should be 2012.

14) Line 250: since nutrients were measured, it would be really interesting to have these data discussed in this context. I'm actually doubtful that microbial DOM degradation is nutrient-limited, since blackwater rivers in SE Asia do tend to have a few micromolar DIN and DIP (e.g. data table in Alkhatib et al. 2007; Bange et al. 2019).

15) Section 6.4: the authors argue that the large change in DOC/Cl^- ratio after the confluence of the rivers cannot be due to dilution alone, and must involve degradation of DOC. This doesn't necessarily follow: the two river systems both have similar, and low, concentrations of Cl^-, but a large difference in DOC concentration. Cl^- is therefore not a good tracer of the mixing behaviour. It seems more likely that the blackwater river makes a quantitatively relatively small contribution to the whitewater river, so that after mixing there is not much of an impact on the river chemistry. If the change in the ratio really was partly due to degradation, I think this would imply that the DOC from the blackwater does not degrade much in the blackwater river, then suddenly undergoes significant degradation between the confluence and the first whitewater sampling station less than 5 km downstream, but thereafter doesn't show much further degradation (since the DOC/Cl^- is quite similar in all the whitewater stations). So, I think that the relevant parts of Section 6.4 need to be re-written.

16) The data tables have a mixture of points and commas as the decimal marker

17) In the figure legends, “WRu” is sometimes given as “WRt”

18) The fonts in Figure 5 might need to be increased somewhat, it’s very hard to read in a print-out.

19) In Figure 6b, there are a few light-coloured triangles at the top left of the graph, but this symbol is not explained in the legend.

20) The authors switch between the terms “black river” and “blackwater river”. Would it be better to pick one and stick with it?

References

Bange H.W. et al. 2019. Nitrous oxide (N2O) and methane (CH4) in rivers and estuaries of northwestern Borneo. Accepted for Biogeosciences, doi: 10.5194/bg-2019-222 (this special issue)

Cory R.M. et al. 2010. Effect of instrument-specific response on the analysis of fulvic acid fluorescence spectra. Limnology & Oceanography: Methods 8: 67-78

Helms J.R. et al. 2014. Loss of optical and molecular indicators of terrigenous dissolved organic matter during long-term photobleaching. Aquatic Sciences 76: 353-373

Hansen A.M. et al. 2016. Optical properties of dissolved organic matter (DOM): Effects of biological and photolytic degradation. Limnology & Oceanography 61: 1015-1032

Kida M. et al. 2018. Contribution of humic substances to dissolved organic matter optical properties and iron mobilization. Aquatic Sciences 80: 26-

Lu C.-J. et al. 2016. Sources and Transformations of Dissolved Lignin Phenols and Chromophoric Dissolved Organic Matter in Otsuchi Bay, Japan. Frontiers in Marine Science, doi: 10.3389/fmars.2016.00085

Poulin B.A. et al. 2014. Effects of Iron on Optical Properties of Dissolved Organic Matter. Environmental Science & Technology 48: 10098-10106

Zhou Y. et al. 2019. Composition and cycling of dissolved organic matter from tropical peatlands of coastal Sarawak, Borneo, revealed by fluorescence spectroscopy and parallel factor analysis. Biogeosciences 16: 2733-2749 (this special issue)