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Interactive comment on "Sources and fate of terrestrial dissolved organic carbon in lakes of a Boreal Plains region recently affected by wildfire" *by* D. Olefeldt et al.

Anonymous Referee #2

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General comments: The purpose of the study was to evaluate the potential for wildfire to alter the linkages between terrestrial and aquatic C cycling in boreal regions. The expectation is that fire frequency and severity in boreal regions will continue to increase in the future, and few studies have investigated how fires in watersheds may alter aquatic C cycling. The intended contribution of this study was an examination of DOC quantity and quality collected from watersheds influenced by a recent wildfire in the boreal region. Based on a modeling exercise using measures of DOC quantity and quality, the authors concluded that wildfire altered the composition of near surface peatland DOC, but DOC exported to lakes in this region is mainly derived from the deeper peat layers not influenced by wildfires. Given some critical issues with the approaches to

characterize DOC and its removal, it is not clear that the data support the conclusions made. Specific comments There is no way to relate photochemical loss of DOC from experiments to photochemical loss in a real lake, due to differences in light available and light absorption by DOC over depth in a tube during an experiment compared to depth in a lake (refer to Hu et al. 2002, Vähätalo et al. 2000, for example). Photochemical loss of DOC in water is estimated from the product of apparent quantum yield of photochemical loss and the rate of light absorption by DOC. None of these data were included here. Thus, the photochemical data in its current form can't be used in the model to make conclusions on photomineralization in lakes. Secondly, it is not clear how the comparison between photochemical processing (DOC UV) and microbial processing (DOC DARK) was conducted given that for the majority of the dark incubations there was either an insignificant change in DOC or a net gain in DOC (Fig. 9a). For example, how can 25% of within-lake terrestrial DOC removal be attributed to microbial processing if the mean microbial processing of DOC from peatland wells and all lakes was not statistically different from zero (DOC DARK loss was -0.5 +/- 2.5% and 0.1 +/- 2.7% for the peatland well samples and the lake samples, respectively)?

With respect to the PARAFAC analysis, normalizing Raman units to DOC concentration likely yield artifacts in the analysis. The goal was to calculate the mean contribution of each fluorophore or component to the overall fluorescent signal of the DOC sample, and then to compare the mean contribution of each component across different sample sites (Figure 3). The same approach was also taken for determining changes to fluorophore contributions after light and dark incubations. This could have been done by comparing the relative changes and ratios of the different components at Fmax. By dividing each component concentration by the DOC concentration of the individual samples, the assumption is that each DOC sample has a similar ratio of fluorescent DOC to total DOC, which has repeatedly shown to be incorrect given the wide range of fluorescence per DOC across freshwaters in space and time. Specific comments:

Pg. 6096, lines 21-22: Several other studies have studied the effect of wildfire on

aquatic C cycling - see Stubbins et al. (2010), (2012), Hockaday et al. (2006), (2007), and Dittmar et al. (2012). These studies have used chemical markers and high resolution mass spectrometry to determine how fire derived condensed aromatics influence the chemical composition and fate of DOC exported from fire impacted watersheds. Also, see Czimczik and Masiello (2007) where models are developed to predict the export of condensed aromatics from fire impacted watersheds. Pg. 6097: I tried looking up Fire ID SWF057 and found references to SWF060 which was a fire in the same year, in the same area, and of the same size. Is this the right ID? Pg. 6097: The author's provide a thorough site description but do not talk about the fire history of this region. Is this region historically prone to wildfires? If so, given that fire-derived chars reside in the soil column for hundreds to thousands of year, is it accurate to use lakes and wells outside of the most recent fire boundary line as an "unburned" control? Pg. 6100, line 4: Say "henceforth all referred to as peatland well samples," but on page 6104, line 8 well samples and surface samples are distinguished. Pg. 6100, line 20: Is it possible to have a DOC standard of 0 mg-C L-1? Also, be consistent with units (reported here as mg L-1 but everywhere else as mg C L-1). Pg. 6102, line 19: UV-Vis absorbance not absorption was measured on a Varian Cary 100 – see Hu et al. (2002). Pg. 6102, line 21-21: Add units for absorbance at 254 (m-1) and SUVA (L mg-C-1 m-1) according to Weishaar et al. (2003), also it seems that the SUVA254 values here may be too high and influenced by iron (Weishaar et al. 2003). Pg. 6103, line 4: Here absorbance at 254 nm is in units (cm-1) but this is not how Weishaar et al. 2003 defines A254. Please use the conventional units (m-1). Pg. 6105: Instead of introducing a new term called R, which is essentially 1/SUVA, why not just use SUVA and modify your equation so that you end up with DOC in the numerator. This would help clarify the mixing model. Pg. 6107, line 11: Is there a p-value associated with the PARAFAC component comparisons? Pg. 6108 and Figure 9: Report UV losses, dark losses, and UV – dark losses in that order, it is very difficult to read as is. Figure 9a: Data from Olefeldt et al. 2013 is very confusing, not easy to understand how this fits in. Corrections: Pg. 6096, line 27: Add an "n" to western. Pg. 6104, line 19: Add "respectively." Pg. 6108, line 3:

Fig. 8 should be Fig. 9a. Pg. 6115: delete "in"

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