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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 #1

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The discussion paper by Olefeldt et al. (Biogeosciences Discuss. 10, 6093-6141, 2013) uses hydrochemical mixing models to assess the groundwater source of lake dissolved organic carbon (DOC) and to assess the fate of the DOC in the lake: photochemical vs microbial degradation. By using electrical conductivity as a conservative tracer in one of the models, the authors were able to conclude that the DOC had its origin in organic soils (peat) rather than in mineral soils. The model also allowed for calculation of the change in DOC, from source water to lake water, relative to the corresponding change in absorbance at 254 nm. As this ratio was argued to be different for photochemical degradation and microbial degradation, the authors could apply another mixing model, suggesting that photochemical DOC degradation was the dominant fate

of DOC in the studied lakes. I enjoyed reading this manuscript is it takes a novel hydrochemical mixing model approach to the fate of terrestrial DOC in lakes. The ms is generally well written. My main critique is that the assumptions behind the mixing models are not well justified. Further, the very large sources of uncertainty involved are not given much attention in the discussion, although some of them are mentioned and dealt with in the appendix.

General concerns

1. It is argued that the relationship between DOC loss and A254 loss, respectively, during 11 day incubations was strongly different for microbial and photochemical degradation, respectively (Fig. A1). However, the photochemical degradation was not directly measured; instead it was assumed to be represented by the difference in the rate of DOC loss between irradiated and dark samples. The problem with this assumption is that the UV light can have caused a significant stress and mortality of the microorganisms in those small incubations flasks that were irradiated. It could even be discussed whether or not the microbes at all contributed to the DOC degradation in the irradiation treatment. If they did not contribute, the photochemical degradation should be considered as the DOC loss in the light per se, and not as the difference between DOC loss in irradiated and dark samples. This alternative way of considering the photochemical degradation would drastically change the slope of the relationship in Fig A1 and it might completely reverse the mixing model results. My suggestion to the BG Editors is to give the authors some time to perform control tests to confirm the accuracy of assuming equal microbial activities in the light and in the dark. For example, they could perform cell counts during the incubations and even determine the change in biomass over time. They could also measure bacterial production using the 3H-leucine incorporation method to test for differences between the irradiated and dark samples. An additional option would be to measure the photochemical degradation directly, i.e. by running tests with sterile-filtered samples.

2. Based on an unreferenced line in the appendix, stating that 'precipitation inputs

are generally balanced by evaporation losses in the region' (p. 6118 l.24-25), it is assumed that evaporation did not affect the mixing model results. However, even if the overall precipitation-evaporation of the region is in perfect balance, it is still possible that evaporation exceeded precipitation on the surface waters of the region, and that precipitation slightly exceeded evaporation in the terrestrial environment. Considering the long water residence times in many of the lakes, a substantial amount of evaporation may have occurred, possibly reflecting the gradient of δ18O of water that was observed. Lake evaporation can be a key cause of δ18O variability and 18O enrichment in catchments with long water residence times in lakes. If evaporation actually exceeded precipitation on the lake surfaces, electrical conductivity would be significantly affected and the mixing model would lead to false conclusions about the source of the water and the DOC.

Specific comments

1. p.6096 l.1 It seems too simplified to state that the higher export of DOC from peat soils, compared to mineral soils, is due to the lack of minerals that impede and reduce export. There are huge differences in contents of organic matter between these soils. In peat, the organic matter accumulates because the system creates an environment where degradation is inefficient.

2. p.6097 l.19 The figure of the study area is well prepared, but I miss a reference to a table that describes the limnological characteristics of the lakes more in detail.

3. p.6098 l.4 The $\sim$1 m deep lakes of region makes me wonder about the definition of lakes. For example, is a 0.5 m deep aggregation of water a lake or a wetland? I again recommend a table that describes the limnological characteristics of the study lakes, to sort out the possible confusion about the nature of these systems.

4. p.6099 l.22 Perhaps the authors could clarify whether or not the choice of collecting lake samples from land, rather than from the lake center, might have affected the results.

5. p.6099 l.26-27 I suggest clarifying to which extent these wells received water also from superficial organic soils

6. p.6101 l.20 The fact that the incubation chambers were set to 17.5°C does not mean that the actual temperature inside the flasks was 17.5°C. My experience from these types of experiments is that it is difficult to keep the temperature below 25°C and that the actual incubation temperature of the water easily can rise to both 30 and 40 degrees, even inside of climate chambers that are 'set' to certain temperatures.

7. p.6102 l.9-13 The choice of assuming fixed pH could/should be further justified, as the pH effect of produced CO2 during laboratory incubations can be substantial (depending on the design of the incubations). Further, I suggest clarifying whether or not the 'mineralization rates' mentioned here refers to the same thing as the 'CO2 production' that is referred to in the results and the discussion sections. Using the same term everywhere seems preferable, as 'CO2 production' could also be interpreted as the change in CO2 only and not the sum of the produced CO2 and the changes in bicarbonate and carbonate.

8. p.6104 l.11-12 The uncertainty of the assumed ECM values are missing here

9. p.6105 l.6-10 I found it difficult to understand this part without having to read large parts of the appendix. Although lengthy uncertainty analyses might fit an appendix, I suggest bringing the key points regarding the handling of uncertainty into the manuscript (here and, especially, in the discussion). A specific reference to Fig A1 should be given, so that the RUV and RDARK parameters become understandable. Another suggestion could be to bring Fig A1 into the paper, since it is possibly the one figure that is most central to the results of the study.

10. p.6108 l.9-10 This result actually shows that the UV light was stressing the microbes and decreasing the rate of metabolism. Otherwise, the values could not have been negative. See general concern #1.

11. p.6111 l.4-6 Unclear if sentence refers to this study or the previous Olefeldt study

12. p.6113 l.11-14 This is an important point. If the study lakes are extremely shallow, an extremely large importance of photo-oxidation could be expected.

13. p.6114 l.17-19 Also the present study does largely ignore the effects of the autochthonous DOC, e.g., on the ratios between DOC loss and A254 loss.

14. p.6118 l.25 – p.6119 l.2 I do not find this convincing enough. See general concern #2.

15. It could be pointed out that the variability in R represents the uncertainty of the mean for the whole region. If RDARK and RUV would be assessed for individual lakes, the values would be varying by hundreds of per cent.

16. p.6119 l.21 Are the lakes in the previous study from the same area? If not, including them may not be adequate.

17. p.6120 Yes, the RDARK could be much lower on a longer time-scale since the non-pigmented DOC fractions might be used first. This is an important consideration that should be brought into the discussion (in the main paper). The lakes in the study have long residence times so the RDARK measured during 11d incubations (biased towards high values) may not be relevant for the actual DOC degradation during the time frame of the lake water residence time.

18. Fig. A1. A suggestion could be to use different symbols for the data that come from the different studies and from the different sources (lakes, wells etc). That would make it easier to judge the relevance of the relationships.

Typographic comments

1. p.6094 l.13 The phrase 'mineral DOC' should be changed: DOC is per definition organic

2. p.6106 l.23 I am not sure about the BGD author guidelines, but small sigma is

typically used for populations, not samples

3. p.6108 l.1 'found' needs to be replaced with, e.g., 'showed'

4. p.6108 l.2 Here and elsewhere: it seems like the coefficient of variation is being used. I have not seen small sigma (see above comment #2 and the p.6106 l.23) being used for the coefficient of variation before.

5. p.6115 l.22 'whether' should be replaced with 'whether or not'

6. p.6116 l.13 'find' should be, e.g., 'show'

Interactive comment on Biogeosciences Discuss., 10, 6093, 2013.