

Response to reviewers:

We would like to thank the two anonymous reviewers and Dr. Waldron for their constructive and helpful comments that have significantly improved the manuscript. Below is a point by point response to each comment, but first we will discuss the implementation and presentation of the mixing model in the manuscript, since it was commented on by all reviewers.

In short, the mixing model had three steps, where results from each step were dependent on assumptions and uncertainties of the preceding step. In the manuscript, we included a Monte Carlo simulation in the appendix to assess the sensitivity of the mixing model results to its assumptions. Reviewer 1 and Dr. Waldron suggested that the uncertainty analysis should be moved to the main text, and this has been done. We are now explicit in stating both central values, and their confidence bounds, of both the constants that are used in the mixing model and for the results of the mixing model.

In the first step of the mixing we assessed the relative contribution of different water sources to the lakes, assuming that lake water is a mix of near-surface groundwater from peatlands and mineral soil groundwater. In this step we had assumed that there was a balance between lake evaporation and direct precipitation inputs were equal and that there thus was no concentration or dilution of ion concentrations (as measured by electrical conductivity). Reviewer 1 and Dr. Waldron comments that even in a landscape where there is an overall balance between evapotranspiration and precipitation; it is still likely that lake evaporation will exceed direct precipitation inputs. We now have included a factor in the mixing model in order to take into account the uncertainty of the lake water balance. Direct precipitation has been measured within the study region, and was 520 mm over the preceding year. We also estimated potential evaporation using the Hargreaves model, which uses air temperature and incoming solar radiation. We used data from a nearby climate station and estimated that lake evaporation had been 630 mm over the preceding year. Both precipitation and evaporation estimates are acknowledged to be imprecise, and in the Monte Carlo analysis we used 95% confidence bounds of 150 and 250 mm, respectively. The difference between lake evaporation and precipitation is then expressed as a fraction of the lake volume to yield a concentration (or dilution) factor. Lake depths had been measured during previous winter surveys, and ranged between 0.5 and 5 m and are reported for each lake in a table.

In the second step of the mixing model, we compared hypothetical lake DOC concentrations and A_{254} based on an assumption of conservative mixing with observed lake DOC concentrations and A_{254} , and attributed the difference between the two to within-lake losses of terrestrial DOC and A_{254} . Reviewer 1 points out that we did not sufficiently take into account any influence of algal DOC in this step. We now have included an uncertainty term in the mixing model which attributes a fraction of the lake DOC pool to

be of algal origin (and should therefore be discounted in before assessing losses of terrestrial DOC). We use a relationship for estimating the algal contribution to lake DOC as outlined by Bade et al. (2007, Biogeochemistry), which uses lake absorbance and lake chlorophyll A concentrations. While we did not directly measure Chl a in this study, it has been done previously in the studied lakes (Sass et al. 2008) and we used the reported range to bracket our estimate of the algal contribution. We now estimate that lake algal contribution to the lake DOC pool is $15 \pm 15\%$ (95 % confidence bounds). Algal contribution to lake A_{254} is assumed negligible as it is strongly linked to terrestrial aromatic DOC.

In the third step of the mixing model, we used the estimated lake losses of A_{254} and DOC from the second step to infer whether the losses had been caused by microbial processes alone or in combination with UV-mediated processes. This step used results of A_{254} and DOC losses from incubations reported in this study and in a companion study now published in *Ecosystems* (Olefeldt et al. In press – available for early view on *Ecosystems* webpage). Reviewer 1 requested further empirical evidence to support our assumptions of this step, and a better presentation of the uncertainties in the mixing model results, Reviewer 2 and Dr Waldron found the use of data from Olefeldt et al. (In press) to support the model as unclear and largely unsuitable. Reviewer 2 also argues that the presented approach cannot be used to infer rates of photomineralization. We agree that the approach cannot be used to infer rates of photomineralization, but that was never the aim of the model which rather was designed to estimate the relative importance of microbial and UV-mediated processes for DOC losses over an integrated time-period defined by the lake water residence time (which is unknown). We have, however, decided to omit this last part of the mixing model. Given the comments by reviewer 1 (particularly on the time-dependence of the incubation ratios of DOC and A_{254} losses, and the difficulties in separating the microbial and UV-associated processes), we agree that further theoretical considerations and empirical studies are needed before short-term dark and UV incubation results can be included in the mixing model as presented, without leading to uncertainty bounds of the results that render central estimates with little meaning. We have made changes to the abstract, method, result and discussion sections in accordance to the omission of the third part of the mixing model.

We still argue that overall our results are consistent with the interpretation that UV-mediated processes likely contributed significantly to within-lake losses of terrestrial DOC, but we now do not state any % fractional attribution of within-lake losses to either dark or UV-mediated processes.

In the abstract we state: “Incubation results, patterns in DOC composition among lakes and mixing model results are consistent with significant within-lake removal of terrestrial DOC through UV-mediated processes.”

And the relevant paragraph in the discussion now states: "Taken together, this study presents several independent results that are consistent with substantial contribution of UV-mediated degradation of aromatic DOC for within-lake losses of terrestrial DOC. Peatland DOC was found to dominate lake DOC inputs, and it had low biodegradability but high losses during UV incubations. The mixing model suggested that A_{254} had been lost to a greater degree than DOC in lakes, which is consistent with observations of decreasing SUVA during UV incubations but contrasts with observations of increasing SUVA during both short and long-term dark DOC incubations (Kothawala et al. 2012b; Olefeldt et al. In press). Observed changes in DOC composition through PARAFAC analysis during UV incubations were also consistent with the observed patterns in DOC composition in lakes with longer water residence times. Estimates of the contribution of UV mediated processes to total lake CO_2 emissions in humic Swedish lakes has been estimated to ~10%, but these estimates include the CO_2 emissions due to the rapid turnover of autochthonous DOC and may therefore underestimate the importance of UV-mediated degradation of terrestrial DOC (Graneli et al., 1996; Jonsson et al., 2001). Lakes in our study region have characteristics that are likely to enhance UV-mediated losses over microbial processes alone compared to other boreal regions. This includes shallow water depths (0.5 – 5 m) that promote mixing and exposure of DOC to UV light, along with a sub-humid climate that in combination with regional hydrogeology result in lakes primarily receiving highly aromatic peatland DOC which is poorly biodegradable but susceptible to photodegradation. Hence, our study region may represent a boreal region where UV-mediated processes are relatively more important for within-lake losses of terrestrial DOC than in other regions. Given that shallow lakes, extensive peatlands and a sub-humid climate characterizes much of the boreal and taiga plains of western Canada, our study suggests that UV-mediated processes need to be considered as important drivers of aquatic C cycling in large boreal regions."

Below are our responses to all comments by the reviewers. Comments are in bold and our answers are in roman.

Anonymous Referee #1

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 as 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.

Our response: We have omitted the last part of the mixing model, see above. Many assumptions of the mixing model were previously discussed in an appendix, and this section has now been incorporated into the main text. Uncertainties of the mixing model results are now explicitly stated in tables and in the text to show the sensitivity of the model to the assumptions of the model.

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.

Our response: We agree that it is highly likely that UV light slows microbial activity, particularly in samples with low protective UV absorbance (i.e. the samples from deep mineral soil groundwater wells). The greater losses during dark than UV incubations for DOC from deep mineral wells support this interpretation. It is however also likely that phototransformations of aromatic DOC into bioavailable compounds stimulates microbial activity. The idea for using the difference between dark and UV incubations was to capture the net effect of these two processes (inhibited microbial activity due to damaging UV light and increased microbial activity due to increased access to bioavailable compounds formed through phototransformations). The last part of the mixing model, where the ratios between DOC and A_{254} losses were used, have been omitted – see top of document where this is discussed. Hence, while studying microbial activity through cell counts and 3H -leucine incorporation and studying the role of direct photomineralization are both interesting experiments for assessing the relative importance of microbial mineralization and photomineralization, we believe they fall outside the scope of this study.

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 $\delta^{18}O$ of water that was observed. Lake evaporation can be a key cause of $\delta^{18}O$ variability and ^{18}O 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.

Our response: We have now included an uncertainty factor of the lake water balance (evaporation and precipitation) in the mixing model, see answer at the top of this document. This inclusion shows that there had been ~100 mm of excess evaporation visavi precipitation over the last year; however this small concentration factor (1-10%, depending on lake depth) did not significantly alter the results or the conclusions of this step of the mixing model.

Specific comments

1. p.6096 1.1 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.

Our response: We have changed the sentence to “Peatlands are generally important catchment sources of DOC both due to the strong source capacity associated with thick organic soils and due to the lack of mineral soils that impede and reduce export”

2. p.6097 1.19 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.

Our response: We now include a full table of lake characteristics and water chemistry data from both lakes and wells in the supplementary data. We have also added a table to the main body, which describes the average and variation of lake characteristics and lake water chemistry for lakes in fine and coarse-textured settings, respectively. The table is referred to in two sentences in the methods section: “Median lake size was 9 ha, and lake size ranged from 0.5 to 300 ha in both the fine- and coarse-textured settings (Table 1). Lake depths were greater in the coarse-textured settings (2.6 ± 1.3 m) than in fine-textured (1.6 ± 0.6) (t-test, $p = 0.04$) (Unpublished data on lake depth, K. Devito)”.

We now include data on lake depths; I was unaware at the time of initial submission that the lake depths had actually been measured on each of the studied lakes in the winter of 2002, through holes in the ice.

3. p.6098 1.4 The 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.

Our response: See answer just above. We now include improved characterization of the lakes, explicitly stating the size and depth ranges. We now also refer to the lakes as “shallow lakes” already in the abstract, to better convey the nature of these systems to the reader right away. Shallow lakes are very

characteristic of the boreal and taiga plains of western Canada, even very large lakes (e.g. nearby Utikuma lake 288 km²) has a maximum depth of 5 m.

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.

Our response: While lake samples from the centre of the lakes would be preferable, it was not practically feasible for this study due to time constraints. Also, the lake edge effect has been tested previously in some of the sampled lakes (unpublished data), where lake samples were taken along a transect from the center of the lakes to shore by canoe, including samples from within the edge vegetation. It was concluded that using a 3 m rod from the edge of the vegetation was sufficient to avoid an edge-effect on water chemistry.

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

Our response: The deep wells only have openings at specific depths (piezometers), all located below the interface with the organic soils. Hence there should be no contribution of superficial soils to the mineral soil wells. We have added a sentence “Seven wells that extend 4 to 16 m in depth and only receive mineral soil groundwater were sampled, of which 4 were located in burned areas (Fig. 1). Seven wells in peatlands (~1 m deep, with no mineral soil contact) were also sampled, 4 of which were located in burned peatlands.”

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.

Our response: Water temperatures in the flasks were measured at the end of the incubations, and were < 20 deg C in both samples under the screen and samples exposed to UV light. We used a large growth chamber for this experiment and the flasks were placed > 0.5 m away from the UV lights which were the only potential heat sources. The floor of the growth chamber (~1 m²) was slatted and provided continuous ventilation and climate control to the chamber.

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.

Our response: We did measure pH at the start and the end of the incubations, but since the jars were sealed we were unable to get pH measurements for the samplings on incubation day 1, 3 and 6. Rather than assuming constant pH during the incubations, we have now altered the methodology for estimating change in DIC in order to take into account the small changes in pH. The following section has been included in the methods section: “Initial pH of the incubation samples were all between 7.01 and 8.64, following the dilution using NaHCO₃, see above, which is in the range of observed pH in the lakes. Sample pH could only be measured at the start and at the end of the incubation as the jars were sealed; the difference between the start and end of the incubation was $+ 0.1 \pm 0.15$ pH units and we assumed linear change in pH during the incubations for samplings at day 1, 3 and 6.” This change in methodology had only a minor effect on the total CO₂ production.

The terminology has now been made consistent through the manuscript, with CO₂ production defined as the sum of the change in headspace CO₂ concentrations and the change in DIC concentrations.

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

Our response: All the confidence intervals of the used constants in the mixing model, including ECM, are now explicitly stated in the last paragraph on the mixing model in the methods section.

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.

Our response: The third step of the mixing model, which used data from Fig. A1 has been omitted; see discussion at the top of this document. As for the uncertainties of results of the mixing model, we now include the Monte Carlo analysis in the main document in order to assess the uncertainty bounds of the mixing model results for each lake. The Monte Carlo analysis is described in the methods section and we report the uncertainty bounds in the results section, and in the new Table 1.

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.

Our response: We agree with this interpretation. We state in the discussion: “The difference between DOC losses during UV and dark incubations was strongly related to initial sample SUVA, causing substantially increased losses during UV incubations for peatland well DOC while losses decreased slightly for mineral well DOC. Reduced DOC loss during UV incubations for samples of low aromaticity has previously been observed (Tranvik and Bertilsson, 2001), possibly linked to the damaging effects of UV light on microbes in the absence of UV attenuation.”

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

Our response: This section has been restructured to read: “Biodegradability and SUVA of DOC samples from organic soils, fresh litter and vegetation leachates have a strong negative relationship (cf. Kalbitz et al., 2003). In this study we found only a weak negative relationship between dark incubation DOC losses and SUVA, and biodegradability remained relatively low even for samples with very low SUVA. In a similar incubation experiment, leachates from organic soils with comparable SUVA to that of the mineral soil DOC in this study exhibited losses that were 5-10 times greater (Olefeldt et al. In press).”

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.

Our response: We now refer to the lakes as shallow already in the abstract, and we discuss the lake depths in the methods section, and lake depths are shown in a new Table 1. The shallow nature of the sampled lakes is highly characteristic for the boreal and tundra plains of western Canada, hence our results are of relevance for a large boreal region.

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.

Our response: We have now included the influence of algal DOC in lakes in the mixing model, see discussion at the top of this document.

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

Our response: We have now included in the mixing model an uncertainty factory which takes into account lake evaporation and direct precipitation inputs, and their uncertainties. See discussion at the top of this document.

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.

Our response: The third step of the mixing model, which used Rdark and RUV, have been omitted. See discussion at the top of this document.

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

Our response: The third step of the mixing model, which used data from Olefeldt et al. In press, Ecosystems, has been omitted. See discussion at the top of this document.

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.

Our response: The potential for time-dependency of the ratios of DOC loss and A254 loss during incubations was found to introduce large uncertainties in the third step of the mixing-analysis, contributing to our decision to omit it from the manuscript. See discussion on top of this document.

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.

Our response: This figure has been omitted; see discussion at the top of this document.

Typographic comments

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

Our response: We now refer to the two terrestrial sources as peatland groundwater DOC and mineral soil groundwater DOC throughout the manuscript for clarity.

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

Our response: We now define the error term as standard deviation the first time we report an error term, and omit the small sigma throughout the manuscript.

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

Our response: We have made the suggested change.

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.

Our response: The error terms represent standard deviation throughout the study. This is now defined the first time we report an error term, and the small sigma has been omitted throughout the manuscript.

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

Our response: We have made the suggested change.

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

Our response: We have made the suggested change.

Anonymous Referee #2

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.

Our response: We agree that rates of photomineralization would need data on apparent quantum yield and light absorption. However, our approach did not estimate the rates of photochemical loss, but used differences in ratios of DOC and absorbance losses between dark and UV incubations to assess which process that fit the DOC and absorbance losses in lakes as estimated from the first two steps of the mixing model. This approach is thus independent of the actual rates of photo- and bio- degradation of DOC, and only solved for an estimate of the time-integrated ratio between microbial and photochemical processes. As discussed at the top of this document, the last part of the mixing model as has been omitted in the revised manuscript.

However, we have revised and expanded a part of the discussion where we argue that our study has results from several independent approaches that all are consistent with a strong influence of UV-mediated processes on within-lake losses of terrestrial DOC: “Taken together, this study presents several independent results that are consistent with substantial contribution of UV-mediated degradation of aromatic DOC for within-lake losses of terrestrial DOC. Peatland DOC was found to dominate lake DOC inputs, and it had low biodegradability but high losses during UV incubations. The mixing model suggested that A_{254} had been lost to a greater degree than DOC in lakes, which is consistent with observations of decreasing SUVA during UV incubations but contrasts with observations of increasing SUVA during both short and long-term dark DOC incubations (Kothawala et al. 2012b; Olefeldt et al. In press). Observed changes in DOC composition through PARAFAC analysis during UV incubations were also consistent with the observed patterns in DOC composition in lakes with longer water residence times.”

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 $-0.5 \pm 2.5\%$ and $0.1 \pm 2.7\%$ for the peatland well samples and the lake samples, respectively)?

Our response: We used a combined dataset of incubation results from this study and from Olefeldt et al (In press), which was carried out with under very similar conditions, to get estimates of R_{dark} and R_{UV} . The larger DOC losses were primarily from the Olefeldt et al. In press study. Low losses of DOC during

the first few weeks of incubation experiment does however not preclude that larger microbial (dark incubation) losses could occur at the time-frames of lake water residence times, as has been shown by Koehler et al (2012 JGR). However, this step of the mixing model has been omitted; *see* discussion at the top of this document.

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 F_{max}. 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.

Our response: Yes, component fluorescence from PARAFAC analysis is often expressed as a per cent of the summed F_{max} of all components. However, expressing fluorescence intensities normalized to DOC concentrations is also common (e.g. Wickland et al 2007 Ecosystems). By normalizing component fluorescence to DOC concentration (fluorescence intensity per unit DOC concentration), fluorescence of each component is reported as DOC composition indices that are analogous to SUVA (absorbance per unit of DOC concentration). We do not assume that the ratio of fluorescing DOC per total DOC concentration unit is similar across samples; on the contrary, the fact that total fluorescence (sum of all component F_{max}) varies greatly across samples and changes during the incubations is one of the main points of figure 3 (peat DOC > lake DOC > mineral soil DOC and end of dark incubation > start of incubation > end of UV incubation).

We have clarified the unit of the fluorescence measurements in the methods section, where we also introduce the use of expressing component fluorescence as a % of total fluorescence. In figure 3B we have now included component fluorescence % contribution to total fluorescence, so that our results can be compared with other studies that have chosen this way to present fluorescence PARAFAC data.

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.

Our response: We now explicitly acknowledge that it is well known that soil char layers release highly aromatic DOC to downstream ecosystems, citing some of the suggested references, and that our study mainly addresses the question whether there is a pulse perturbation in the years immediately following wildfire to the delivery of terrestrial DOC into lakes and its potential effects on aquatic C cycling.

We have added/alterd these sentences in the introduction: “Highly aromatic DOC derived from soil char layers has been shown to be available for downstream transport, as it has been detected in rivers, lakes and in the ocean (Hockaday et al. 2007). Char-derived DOC has very low biodegradability but is rapidly mineralized or transformed into biodegradable compounds through UV processes (Stubbins et al. 2012; Olefeldt et al. In press), and therefor has the potential to alter aquatic C cycling if its export is significantly altered following wildfire. Few studies have however assessed implications for aquatic C cycling during the first years following wildfire (c.f. Marchand et al., 2009), and studies of catchment DOC exports and lake DOC concentrations have yielded variable short-term responses to fire in different regions (Schindler et al., 1996; Carignan et al., 2000; McEachern et al., 2000; Allen et al., 2003; Betts and Jones, 2009; Marchand et al., 2009). Given the likelihood of increased fire frequencies and severities in boreal regions, particularly continental regions such as western Canada (Flannigan et al., 2009), understanding downstream implications of wildfire on lake C cycling and the degradation of terrestrial DOC is important.”.

In the discussion we have added to and altered the following paragraph: “... It is likely that char layers present in the peat strata, associated with historical wildfires from the course of the peatland development, contribute highly aromatic DOC to the soil DOC pool in the study region (Hockaday et al. 2007), but low levels of DOC export derived from such deeper char sources should be regarded as a long-term influence of wildfire and not as a pulse perturbation to terrestrial-aquatic linkage arising from recent wildfires that this study was designed to detect. We observed a large variation among the sampled lakes with regards to DOC concentration, composition and biodegradability, but none of this variation could be attributed to lakes being located within or outside the recent fire perimeter. Hence, although wildfire created a potential for altered downstream export by altering near-surface peatland DOC sources, the lack of a

signal in lake DOC composition suggests that peatland export is dominated by DOC from deeper peat layers below the water table that were not altered by the recent wildfire.”

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?

Our response: We used the ID given in the Historical Wildfire Database, downloaded from the Alberta Environment and Sustainable Resource Development web-page

(<http://srd.alberta.ca/Wildfire/WildfireStatus/HistoricalWildfireInformation/Default.aspx>).

The fire the study area is known as the Utikuma Complex fire, and it is designated SWF057, while SWF060 is known as the Deer Mountain Complex, and occurred during the same period but south of the study area. Due to the potential confusion, we have omitted the fire ID and now refer to the fire as the Utikuma Complex. Given the name of the fire, the location, date and the size of the fire, we believe that anyone who is interested could find the correct fire in the relevant databases. We have also included a lat/long description of the study area in the figure legend of Fig. 1.

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?

Our response: We have added a description of the fire-regime of the region to the introduction. In addition to the general description of the fire-regime, we know that the last time the study area was partially affected by fire were by smaller fires in 1952 and 1956. The text now states: “The study area was partially affected by the Utikuma Complex wildfire that broke out on May 15th 2011 and eventually burned an area of 880 km², including the surroundings of many lakes within URSA (Fig. 1). The mean fire return interval of the Boreal Plains in Canada is ~150 years (Flannigan et al., 2009), and the last time the study area was partially affected by wildfire were in 1952 (96 km²) and 1956 (57 km²) (Alberta Sustainable Resource Development, 2013)”. Given the time since these much smaller fires, we still consider the 2011 fire perimeter to be a useful delineation for assessing abrupt changes in water quality among lakes following wildfire.

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.

Our response: This has been changed to read “peatland well water samples” only.

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).

Our response: We have changed the unit to mg C L⁻¹. The calibration curve included a pure MilliQ sample, along with mixes of MilliQ water and a C standard to concentrations of 1, 10, 50 and 100 mg C L⁻¹. Including or excluding the pure MilliQ sample from the calibration curve has minimal influence on the resulting linear calibration curve. We have changed the sentence to “Four point calibration curves over the range 1-100 mg L⁻¹ were established for DOC using standards and MilliQ-water...”.

Pg. 6102, line 19: UV-Vis absorbance not absorption was measured on a Varian Cary 100 – see Hu et al. (2002).

Our response: “Absorption” has been changed to “absorbance”.

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).

Our response: Absorbance at 254 nm on its own is often reported in the unit cm⁻¹, even in the article by Weishaar et al (2003). We have added the units of absorbance at 254 nm and SUVA to the location in the methods section where they are introduced the first time “Absorbance at 254 nm (A_{254} , unit: cm⁻¹) divided by the sample DOC concentration and multiplied by 100 yields specific UV absorbance (SUVA, unit: L mg C⁻¹ m⁻¹), which has a robust positive relationship with DOC aromaticity (Weishaar et al., 2003).“

We do not think that the SUVA values in this study are higher than expected based on results from other studies. Peatland water samples often have SUVA between 3.0-5.0 L mg C⁻¹ m⁻¹, leachates from live vegetation or collected from mineral soils have generally lower values, e.g. < 2.0 L mg C⁻¹ m⁻¹, and

SUVA in streams and lakes vary between these extremes depending on terrestrial sources and internal processing (Wickland et al 2007 Ecosystems, Ågren et al 2008 JGR, Olefeldt and Roulet 2012 JGR, Kothawala et al 2012 Geoderma, Balcarczyk et al 2009 Biogeochem).

High iron concentrations and associated absorbance could elevate SUVA of a sample, but we assess its impact in this study to be minor. Iron concentrations of a large number of lakes in the Utikuma Research Study area has been measured previously, including some but not all of the lakes in this study (unpublished data). Of 86 lakes, the average Fe concentration was $0.16 \pm \text{mg Fe L}^{-1}$, and only 3 lakes had concentrations $> 0.50 \text{ mg Fe L}^{-1}$. Using the relationship in Weishaar et al (2003), a concentration of $0.16 \text{ mg Fe L}^{-1}$ would increase A_{254} by 0.011 cm^{-1} . This in turn would mean that lake SUVA in this study is overestimated by $0.04 \pm 0.02 \text{ L mg C}^{-1} \text{ m}^{-1}$ on average, or by $0.14 \pm 0.02 \text{ L mg C}^{-1} \text{ m}^{-1}$ if using the highest observed, $0.50 \text{ mg Fe L}^{-1}$ lake concentrations. These are small potential sources of error, given the range of SUVA among lakes was between 0.97 and $2.88 \text{ L mg C}^{-1} \text{ m}^{-1}$.

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).

Our response: Absorbance at 254 nm on its own is often reported in the unit cm^{-1} , even in the article by Weishaar et al (2003), e.g. their figure 2 and 4. We have kept the unit as cm^{-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.

Our response: The terms RDark and RUV have been omitted in the revised manuscript, along with the third step of the mixing model, see discussion at the top of this document.

Pg. 6107, line 11: Is there a p-value associated with the PARAFAC component comparisons?

Our response: Yes. T-tests for each component was run to test for differences between lakes in coarse and fine-textures setting and all $p < 0.02$. This has been added to the sentence: “Lakes in fine-textured settings also had higher C_C , C_A , C_X and C_M than lakes in coarse-textured settings (all components associated with terrestrially or microbially derived humic and fulvic acids), while C_T (associated with amino acids) was lower (t-tests done for each component, all $p < 0.02$) (Fig. 3A).”

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.

Our response: We have changed figure 9 to have 3 plates with dark losses, UV losses and UV-dark losses. We have also re-written the paragraph in the results section as recommended. The revised results section reads: "There was a weak but significant negative relationship between sample SUVA and DOC loss during dark incubations across all incubations ($R^2 = 0.07$, $n = 51$, $p = 0.04$). Losses of DOC during UV incubations were greater for peatland well samples than mineral well samples, and initial SUVA across all samples had a strong positive relationship with UV incubation DOC losses (Fig. 9B). However, initial sample SUVA had its strongest relationship with the difference between UV and dark incubation DOC losses (Fig. 9C). Losses of DOC during UV incubations were lower than during dark incubations for mineral soil well samples, -2.0 ± 2.7 % (i.e. UV – dark incubation losses), while UV incubation significantly enhanced losses for peatland well DOC, $+13.9 \pm 3.5$ %."

Figure 9a: Data from Olefeldt et al. 2013 is very confusing, not easy to understand how this fits in.

Our response: The data from Olefeldt et al In press has been removed from this figure. Instead we compare the relationships between dark incubation biodegradability and SUVA reported among studies in the main text, where we have added this section in the discussion: "Biodegradability and SUVA of DOC samples from organic soils, fresh litter and vegetation leachates have a strong negative relationship (cf. Kalbitz et al., 2003). In this study we found only a weak negative relationship between dark incubation DOC losses and SUVA, and biodegradability remained relatively low even for samples with very low SUVA. In a similar incubation experiment, leachates from organic soils with comparable SUVA to that of the mineral soil DOC in this study exhibited losses that were 5-10 times greater (Olefeldt et al. In press)."

Corrections:

Pg. 6096, line 27: Add an "n" to western.

Our response: This change has been made.

Pg. 6104, line 19: Add “respectively.”

Our response: This change has been made.

Pg. 6108, line 3: Fig. 8 should be Fig. 9a.

Our response: This change has been made.

Pg. 6115: delete “in”

Our response: This change has been made.

Reviewer 3

Overview: a very well – written paper with an interesting data set that furthers understanding of attenuation of DOC in lakes. That there has been fire is secondary influence that may have been sought to assess through the sampling campaign. Fire does not prove to be significant, so this m/s is not furthering our understanding significantly of the impact of fire (as the title may imply) but moves on understanding of DOC attenuation in lakes. I have only a few comments questioning scientific interpretation, but improvements can be made to the focus and clarity of the m/s.

I have also produced this review without reading first the other reviews so I am not influenced by them.

Key issues that need to be addressed are:

1. A hypothetical framework is required: there is no explanation of why the variable measured may be affected by fire and the reader would be better informed if this was set-up in a hypothetical context.

Our response: We have made changes to the introduction, which now explicitly states that wildfire may alter both the quality of DOM from different sources and the hydrological connections between terrestrial sources and downstream aquatic ecosystems – and that these changes may alter aquatic C cycling. We also state that the purpose of this study was both to assess whether we could identify any influence of

wildfire on the quality of terrestrial DOC sources, and whether we could find any evidence for wildfire to cause a pulse perturbation to the sources and cycling of terrestrial DOC in the lakes. The last paragraph of the introduction, where the purpose of the study is outlined, has been changed to: ” The purpose of this study was to evaluate the potential for recent wildfire to alter the linkages between terrestrial and aquatic C cycling in a boreal setting. We assessed whether wildfire altered the DOC composition of terrestrial sources, and we sampled lakes located both within and outside the recent fire-perimeter in order to detect an influence of wildfire on DOC composition and reactivity. Several different approaches were used to assess within-lake degradation of terrestrial DOC, including incubations under both dark and UV conditions, and the implementation of a hydrological mixing model. We used absorbance and fluorescence properties of DOC to yield information on aromaticity (Weishaar et al., 2003) and relative measures of humic/fulvic acids and amino acid abundances (Fellman et al., 2010), information that we used to assess the mixing of different terrestrial DOC sources in lakes as well as to infer within-lake changes to DOC compositions as a result of selective biodegradation or UV-mediated degradation. This study stresses the importance of regional and local characteristics, including climate and surface geology, for understanding the relative contribution of various terrestrial DOC sources to aquatic ecosystems, dominant processes for aquatic DOC degradation and impact of wildfire.”

2. Further related to the framework: the authors suggest ‘a framework for understanding the downstream implications of the wildfire on lake C cycling is needed’ (L25, p6096) and this is sensible. But I do not feel that a framework is developed as a point in this paper explicitly. Rather is it implicit in the results, perhaps because wildfire proves not to be a significant influence. Thus does a framework need to be more explicitly developed? If not, then the authors should reconsider rewording this section to reflect the wildfire is a minor influence and the focus of this study is on lake attenuation of DOC.

Our response: We have reworded the section. At the end of the discussion we have added a few sentences to put our results into a larger framework for understanding short-term impacts of wildfire on aquatic C cycling: “Our results therefor suggest that it is necessary to consider both the impact of fire on terrestrial sources and their hydrological connectivity to downstream aquatic ecosystems in order to anticipate changes to aquatic C loading and cycling. Differences in terrestrial C sources and dominant flow-paths in different regions may explain much of the variability in short-term response to catchment DOC export and aquatic C cycling, and suggests that other regions may be

more susceptible to a pulse perturbation following fire than observed in the boreal plains setting of this study.”

3. The application of all the different techniques does not become clear until the data interpretation; more front-end explanation is needed e.g. ‘lakes isotopic signature are presented to delineate source of influx, the Raman peak is measured to : : :’. Authors should aim to have their papers as widely understood as possible and given the breadth of analytical approaches here there is insufficient supporting information for a nonexpert to be able to understand significance and rationale from the paper alone.

Our response: We have made several additions to the methods section in particular I order to give the reader a better understanding of the rational for the chosen study design and methods. Here are some of the additions to the methods section:

“Peatlands were assumed to be the primary sources of DOC to the lakes, and in order to assess whether wildfire influenced peatland pore-water DOC concentrations or composition we established depth-profiles for soil pore water sampling using rhizon pore-water samplers (Rhizosphere research products) in one peatland that burned in the previous year and in one nearby unburned peatland.”

“Electrical conductivity (Ec) of water samples can be assumed to act conservatively when mixed, and was used to assess relative importance of different water sources to lakes, *see below*. Isotopes of oxygen and hydrogen ($\delta^{18}\text{O}$ and δD , respectively) can also be used to assess water sources, but is further sensitive to fractionation through evaporation.”

“We conducted DOC incubations under dark and UV conditions of lake and well samples in order to test for differences in microbial and UV-mediated DOC lability between different terrestrial DOC sources, between lakes in different settings and to assess any influence of wildfire.”

“Since this study is interested in the lability of terrestrial DOC within lakes, we used the NaHCO_3 to buffer and homogenize pH and among samples, in particularly to increase the pH of peatland well samples to approach pH in the lakes (average pH before and after dilution for

peatland well samples: 4.32 ± 0.8 and 7.04 ± 0.21 , respectively). Standardized DOC concentrations at 20 mg C L^{-1} is in the range observed among lakes and was also chosen to avoid excess microbial growth and associated anoxia during the incubations.”

“The incubation light levels were comparable (UV-A) or lower (UV-B) than measured in the field under sunny mid-day conditions during our sampling period, with measured UV-A and UV-B light intensities of $\sim 20 \text{ W m}^{-2}$ and $\sim 1 \text{ W m}^{-2}$, respectively.”

“Absorbance and fluorescence of DOC samples yield information on DOC composition, which was later used to characterize terrestrial DOC sources to lakes, to observe changes in DOC composition during dark and incubations and to infer dominant processes responsible for within-lake losses of terrestrial DOC. “

“We used a two-step hydrological mixing model to assess (step one) the fractional contribution of water and DOC to lakes from either mineral soil or peatland groundwater sources and (step two) to assess losses of terrestrial DOC and A_{254} within lakes.”

4. I am concerned there is a high dependence on citing the author’s own work which may currently be in review. Olefeldt et al, 2013 is cited 7 times and this reference is not the same as the reference list where there is Olefeldt et al 2012 in press or Olefeldt et al in review. The context of the references make me think it is the latter (e.g., l12, p6116 refers to DOC fro soils than catchments If it is the latter such referencing and the material needs to be removed as this research: a) has not yet been verified by peer review and ii) is not available to consider the significance of the findings in the context in which they have been cited. If the manuscript under review is highly dependent on data that is in another manuscript under review then the authors should wait until the first manuscript has been accepted prior to submitting the second, or should leave the references to the first manuscript out.

Our response: The manuscript Olefeldt et al which was in review, is now in press and can be found online in its final version (Ecosystems). The other article, Olefeldt et al which was in press at Hydrol Proc, has now been assigned an issue which we now refer to. Hence, all cited studies can now be accessed and evaluated with regards to their relationships to this study.

In the revised manuscript, we have removed the use of data from the Olefeldt et al (In press, Ecosystems) in figure 9a, and figure A1 (which also had data from that study) has been removed as part of the third step of the mixing model – see discussion at the top of this document concerning the mixing model.

The Olefeldt et al (In press, Ecosystems) study is still cited several times, but we find it warranted given their connections (both were carried out in the same region, with similar incubation methodologies, and were both assessing the influence of wildfire on DOC).

5. Related to this point: why is it data in review included in A? We cannot tell the data points in this study from those in the review study. I also have a slight concern in not being able to see both papers how much duplication in results between papers there is. Why do we need appendixes in an on-line open access journal?

Our response: The data from Olefeldt et al In press in figure 9A has been removed. We instead compare the results between the studies in the main text: “Biodegradability and SUVA of DOC samples from organic soils, fresh litter and vegetation leachates have a strong negative relationship (cf. Kalbitz et al., 2003). In this study we found only a weak negative relationship between dark incubation DOC losses and SUVA, and biodegradability remained relatively low even for samples with very low SUVA. In a similar incubation experiment, leachates from organic soils with comparable SUVA to that of the mineral soil DOC in this study exhibited losses that were 5-10 times greater (Olefeldt et al. In press).”

The two studies have no duplication, other than in the fact that we used similar incubation methodologies. The Olefeldt et al (In press) describes an incubation experiment that was done on DOC leached from surface soils collected from the URSA study region.

The uncertainty analysis of the appendix has now been incorporated into the main text, or omitted if concerned with the third step of the mixing model; see the discussion at the top of this document.

Comments specific to the m/s:

1. Over how long were the lakes sampled and did water level change much during this period. Was the sampling random? Were the wells sampled at the same time or a long period apart?

Our response: All samples from lakes and wells were collected over a 1 week period during late July in 2012. This sampling period did not include any significant rainfall, and the weather was stable – so all samples were collected under similar conditions. The study area has been a research area for hydrological studies during the last 15 years, hence all wells had been previously developed and the lakes have been studied either as part of detailed studies of individual lakes (e.g. Ferone and Devito 2004 *Journal of Hydrology*), or been part of synoptic studies of a large number of lakes (e.g. Sass et al 2008, *Water resource res.*). As a result, our choice of which lakes to sample was done with the aim to include both larger and smaller lakes located within and outside the fire perimeter as well as located in coarse and fine-textured settings. We have included a table in the manuscript which describes lake characteristics, and we have added a large table as supplementary data with data for individual lakes and wells.

2. L25, p6096 - what is the likelihood: tell the reader.

Our response: This sentence refers to the review by Flannigan et al (2009), who report that fire areas in Canada are likely to increase substantially throughout this century. There are no associated likelihoods given and the wording has been altered to: “Given the projections of increased fire frequencies...”

3. Study area: p6907m line 18: how significant was this wildfire in the range of local fires that occur; we have no sense of this.

Our response: We have added a description of the fire-regime of the region to the introduction. In addition to the general description of the fire-regime, we know that the last time the study area was partially affected by fire were by smaller fires in 1952 and 1956. The text now states: “The study area was partially affected by the Utikuma Complex wildfire that broke out on May 15th 2011 and eventually burned an area of 880 km², including the surroundings of many lakes within URSA (Fig. 1). The mean fire return interval of the Boreal Plains in Canada is ~150 years (Flannigan et al., 2009), and the last time the study area was partially affected by wildfire were in 1952 (96 km²) and 1956 (57 km²) (Alberta Sustainable Resource Development, 2013)”.

4. L16 p 6098: slopes not slope

Our response: This change has been made.

5. p6097: are the lakes only influenced by the glacial deposits? More info on thickness of these to show bedrock not important, or otherwise, is needed.

Our response: We have added a sentence to the site description: “The study area has shale bedrock, but is overlain by glacial deposits between 20 and 240 m thick (Vogwill, 1978). The surface geology is dominated by coarse-textured glacio-fluvial sand deposits in the north-west part of the study area and fine-textured settings with glacio-lacustrine and moraine deposits of clay and silt in the south-eastern parts (Fenton et al., 2003; Devito et al., 2005).”

6. L15, p 6100 Comments that 2003 is a stable phase 2003 as is 2012, so this status of 2012 should be made clear in the conclusions.

Our response: We have included this in the discussion: “Comparative studies in the foothills and sub-arctic eco-regions of central and northern Alberta, respectively, found that average DOC concentrations were 40-50% higher in lakes with recently burned surroundings compared to nearby lakes with unburned surroundings (McEachern et al., 2000; Allen et al., 2003). In our study region there was no influence of wildfire on lake DOC concentrations between 2003 and 2012, which represented years of similar wetness, but URSA differs significantly from the other studied regions in Alberta with regards to topography, dominant terrestrial ecosystems and runoff generation.”

7. In appendix A, a reference is needed to lake levels so the data can be consulted if wanted.

Our response: Lake levels in URSA are still unpublished. The nearest lake water level gauge maintained by Alberta Environmental Protection is located in Utikuma Lake (data can be found in the HYDAT database, www.ec.gc.ca), but its record was discontinued in 2009. Also, because of its much larger size than the lakes included in this study, it is slower to respond to wetter or drier years, which is why Utikuma lake levels did not start increasing until 2004 following the preceding dry period. Hence, in the text we refer to the information on lake levels as “(unpublished data, K. Devito)”.

8. P6101, L10. Not clear why DOC incubations were diluted prior to the start and why with NaHCO₃? Is the former to take them to the same concentration as the lake samples: :but the latter: : :?

Our response: We now explain our study design in the methods section: “Since this study is interested in the lability of terrestrial DOC within lakes, we used the NaHCO₃ to homogenize pH among samples, in particularly to increase the pH of peatland well samples to approach pH in the lakes (average pH before and after dilution for peatland well samples: 4.32 ± 0.8 and 7.04 ± 0.21 , respectively). Standardized DOC concentrations at 20 mg C L^{-1} is in the range observed among lakes and was also chosen to avoid excess microbial growth and associated anoxia during the incubations.”

By homogenizing the incubation sample pH we also increased the buffering capacity which assured only minor shifts in pH during the incubations. The following sentence has been added to the methods section: “Initial pH of the incubation samples were all between 7.01 and 8.64, following the dilution using NaHCO₃, *see* above, which is in the range of observed pH in the lakes. Sample pH could only be measured at the start and at the end of the incubation as the jars were sealed; the difference between the start and end of the incubation was $+ 0.1 \pm 0.15$ pH units and we assumed linear change in pH during the incubations for headspace samplings at day 1, 3 and 6.”

9. P6101, L 24 How representative is the light intensity used in the incubation experiments of of the field environment?

Our response: We have added to the following paragraph: “Samples incubated under dark conditions were placed under a dark screen, while samples incubated under UV conditions were exposed to light intensities of $20 \pm 0.5 \text{ W m}^{-2}$ UV-A (320-400 nm) and $0.07 \pm 0.01 \text{ W m}^{-2}$ UV-B (280-320 nm). Light intensities were measured on the inside of 10 empty jars using Solar Light PMA2110 UV-A and PMA 2106 UV-B sensors attached to a PMA2100 unit. The incubation light levels were comparable (UV-A) or lower (UV-B) than measured in the field under sunny mid-day conditions during our sampling period, with measured UV-A and UV-B light intensities of $\sim 20 \text{ W m}^{-2}$ and $\sim 1 \text{ W m}^{-2}$, respectively.”

10. P6102, L8: A one point measurement repeatedly is not a calibration but a check for drift: calibration check needs multiple points. This needs to be clarified.

Our response: We have changed the sentence to: “Analyses of headspace samples were done on an infrared gas analyzer (PP Systems, EGM-1), where analytical drift was checked by sampling a 1000 ppm CO₂ standard gas regularly.”

11. P6102, L10. Were all three components assumed to be constant? Next line suggests not, so clarify with if just pH (‘the latter: : :’).

Our response: We now do not assume constant pH during the incubations, but use the pH at the start and at the end of the incubation, with linearly interpolated pH for the sampling occasions in between. This made no significant difference to the results. The method is stated as: “Headspace CO₂ concentration, water temperature and sample pH were used to calculate concentrations of DIC in the water sample (Plummer and Busenberg, 1982). Initial pH of the incubation samples were all between 7.01 and 8.64, following the dilution using NaHCO₃, *see* above, which is in the range of observed pH in the lakes. Sample pH could only be measured at the start and at the end of the incubation as the jars were sealed; the difference between the start and end of the incubation was $+ 0.1 \pm 0.15$ pH units and we assumed linear change in pH during the incubations for headspace samplings at day 1, 3 and 6.”

12. 4.1 results are difficult to follow for those not familiar with the PARAFAC: could the authors redefine the terms again or use composition descriptors than CT etc?

Our response: We have in several instances in the manuscript included short descriptions of the PARAFAC component, in order to make it easier for reader less familiar with the method to follow. In section 4.1. we have changed the following sentence: “Peatland wells also had higher contributions of the C_C and C_X PARAFAC components (associated with humic and fulvic acids) than mineral soil wells, while C_T (associated with amino acids) was nearly absent in peatland wells (Fig. 3A).”

13. L25 6106: I do not agree the authors can say this suggests no effect of the fire (particularly without the hypothetical framework): the correct statement is that fire impact cannot be detected, particularly without knowledge of what is was prior to 2011.

Our response: We have changed the sentence to: “No difference in water chemistry or DOC composition could be detected between wells (both peatland and mineral soil wells) located within or outside the recent fire perimeter.”

14. P6108: L19 ‘caused’ rather than ‘yielded’ (yielded tends to be to give out and so not well described for uptake)? What is the evidence for it being POC and not methanogenesis?

Our response: Suggested change has been made. The precipitate could be seen as small flakes. Methane emissions would not cause CO₂ concentrations in the headspace to drop (they were found to drop from 400 to < 200 ppm).

15. P6108: the discussion of the % loss DOC is interesting, but when given as % loss I cannot tell easily how much DOC was lost inter-sample. Although the % may be higher in one sample type, if the concentration is lower then this could be the same amount of C lost, which would be very interesting and may suggest a limitation. Could the authors give absolute amount as well as % loss? Or if this information is already given clearly (i.e. not in a form where we can work this out from existing data) then can the authors direct the reader to where.

Our response: Because the DOC concentrations were standardized to 20 mg C L⁻¹ at the start of the incubation, expressing the DOC losses in mg C L⁻¹ or as a % of initial concentration, produces the same trends among samples. Only three incubations started at lower initial DOC concentrations, which is mentioned in the methods section, and for these samples there would be some discrepancy whether loss was expressed as mg C L⁻¹ or as a % of initial concentration.

16. Fig 6. Why are the axis legends on the opposite side of the units? This is the only diagram like this and makes is less easy to read.

Our response: The axis legends have been switched to be consistent with all other diagrams.

17. Section 4.4. It is a key finding that 46% of the DOC has been removed by within lake processes. Although the authors discuss direct ppt inflow an subsequently dismiss it as the lake levels showed only minor changes, this detailed paper probably does require a sensitivity analysis on that water

balance as it will undoubtedly influences the estimate of 46% losses (in either direction. Why not calculate the water balance and add in a third component for completeness?

Our response: The uncertainty of the water balance has been added to the mixing model, along with the uncertainty associated with an algal contribution to the lake DOC pool. Including these factors, the estimated average loss of terrestrial DOC in the lakes is increased to 56%. See further discussion of changes to the mixing model at the top of this document.

18. The mixing model bootstrapping provides a measure of the confidence the authors have in their estimates and this is the appropriate approach. However, that confidence is not carried over to the conclusions and it needs to be so. ‘We used a simple mixing model to estimate that nearly-half of terrestrial DOC: :had been removed’ L15, p6117. The true statement is that ‘We used a simple mixing model to estimate that that between ?-?% of DOC in the lakes had been removed...’ That there is a range also needs to be clearer in the abstract. Otherwise the work will be cited with misconceptual understanding and our responsibility is to ensure our work is accurately reported.

Our response: We have incorporated the uncertainty analysis of the mixing model into the main body of the text, rather than to have it in an appendix. We also explicitly include the uncertainty bounds and variation in results among lakes in the abstract, in the results section, in table 1 and in the conclusions.