Impact of forest harvesting on water quality and fluorescence characteristics of dissolved organic matter in Eastern Canadian Boreal Shield lakes" by P. Glaz et al.

Response to **Anonymous Referee #1**
Interactive comment on Biogeosciences Discuss., 12, C4408–C4413, 2015

General comments

This manuscript reports on the immediate impact (less than 2 years) of forest harvesting on particular lake water parameters in the Canadian boreal region. Elevated concentrations of total phosphorus were detected up to two years after perturbation and elevated DOC concentrations one year after perturbation. No other measured parameters were found to differ between before and after forest harvesting. The paper is generally well written and the authors give adequate credit to relevant research relating to the impact of forest harvesting in the boreal forest on lentic and lotic habitats.

However, I have major concerns with the novelty of the research, study design and statistical methods used, as well as the conclusions drawn from such a temporally limited study. Some of these concerns (i.e. study design) cannot be rectified. Forest harvesting in the boreal zone is a major activity, and any changes to its management can have extensive social, environmental, and economic ramifications. It is essential that any research about the direct impact of boreal forest harvesting is done so with enough statistical rigor, and the study design is robust enough, to withstand thorough criticism. Based primarily on the study design I do not think this research meets these requirements. Assessing the impact of a perturbation on the natural environment is best completed using a before-after control-impact (BACI) design (see Underwood 1991 for an early reference) – this is something the authors fail to acknowledge and for which, however, the study design and statistical analysis largely resemble. BACI designs, and subsequent improvements (such as MBACI and staircase designs), are designed to accurately detect responses to perturbations by separating natural variability from that due to the perturbation of interest. In these BACI-related studies one of the most important pieces of information is a good characterization of natural variability in the response variables before the perturbation occurs. This information establishes if the response variables vary synchronously through time in all study sites – if they do not, then you cannot isolate any effects due to a perturbation from that due to natural, site-specific variability. The characterization of this variability should include both seasonal and inter-annual variability in order to account for all major sources of natural variation – however, seasonal and inter-annual variability are not characterized in this study.

We agree with the referee and we are aware that the ideal analysis would have been to use a BACI design, even a MBACI design to separate natural variability from that due to the perturbation. We know pretty well the literature of BACI designs (Underwood 1991, 1992, Downes et al. 2002), Beyond BACI (Underwood 1993, 1994) and improved MBACI designs (Keough and Mapstone, 1995). However, as shown by Archambault (co-author of this paper), Banwell and Underwood (2001), even if we have a limited number

of sampling dates before the perturbation occurred to perform a full BACI design, we can still study the temporal variation of the system. Archambault et al. (2001) developed this avenue to help when the funding does not allow for long period sampling before perturbation (e.g. Canadian funding agencies max 3 years in most cases). In this study, we did not compare the system before and after the perturbation since we did not have enough replicates before to use a full MBACI design but we studied the temporal evolution of it and we used the time as factor.
Furthermore, and as pointed out by Referee #2, the unique aspect of this study is that we did sample one year before the perturbation. Few studies on the impacts of land use on DOM composition and DOC quantity have done this.

As Figure 2 shows, natural variation in some of the response parameters each year is just as large as the apparent response to harvesting. Not only does the lack of this pre-perturbation information place a large question mark over the current results (because impacts detected may not be due to forest harvesting but rather to natural site-specific variability), it may also hinder their ability to detect any impacts from forest harvesting that are occurring. Essentially, it is very difficult (in a statistical sense) to attribute any changes in lake water parameters to forest harvesting using the current study design.

If impact detected on perturbed lakes was due to natural variability, then the eight lakes would have been affected in the same way. However, what we document in this study is that some variables (DOC and TP) do not respond in the same way in perturbed vs. unperturbed lakes and that these differences are significant.

Furthermore, this study relies heavily on the notion that sampling on just one date (in July) in each year accurately characterizes the parameters of interest in each lake. The boreal ecosystem is highly patchy and seasonal, and these lakes are not identical (which is evident in Table 1). Furthermore, many of the key response parameters (i.e. DOC) can change rapidly (minutes to hours) in response to natural events, such as precipitation events. Consequently, it is likely that even subtle differences in the response of each site to external events may heavily bias some of the key response effects, and thus mask or falsely indicate a response to forest harvesting. Sampling multiple times per year at each lake would have at least captured some of the influence that seasonal and natural events (i.e. snow melt and storms) have upon the response parameters.

We are aware that sampling in this study was restricted in time, and due to logistic constraints, we could only sample during one month each year. We agree with the referee that it would have been interesting to sample multiple times per year to make a seasonal study. We know that it restricted our conclusion and we added a sentence in the discussion on that (see page 19, lines 462-465). Moreover, to better reflect the seasonal aspect of the study, the title of the manuscript has been changed to: 'Impact of forest harvesting on water quality and fluorescence characteristics of dissolved organic matter in Eastern Canadian Boreal Shield lakes in summer'.

Although the monitoring of forest management activates on freshwater ecosystems is very important, we (researchers and forest managers) already know that there will likely be a short-term impact – this has been documented in many forest types. The most critical piece of information is if this short-term impact will have long-term consequences for aquatic structure and function (i.e. resilience). The authors do acknowledge this in the Introduction. Even so, the novelty of this research is lost due to its very short-term nature and the inadequacy of the study design to properly separate changes in the response variables due to forest harvesting from that due to natural variability

We disagree with the reviewer on this point. A longer study could be desirable but this study done on three years shows interesting results. The research suggests changes in DOC and TP keeping the quality of the CDOM almost unaffected. Moreover, the spectroscopic data converge to suggest that fulvic acids are the mobile form of CDOM carried to lakes. We think this is an important finding. We learned that fulvic acids respond rapidly to forest harvesting contrary to humic acids. Fulvic and humic acids are the most important components of DOC and CDOM.

Specific comments

How many streams drained into each lake? If so, these streams must also drain the harvested forest areas. Do you have an idea of the relative contribution of stream, overland and sub-surface flow for maintaining lake-water levels? i.e. where does the lake water come from and how does it get there? If there are streams draining into the lake, do these streams drain any upstream lakes? It is very difficult to determine this from Figure 1. This information is very important for understanding the flow paths and transport time from the harvested area to the lake, which may help explain subtleties in the lake water parameters and the response of these parameters to harvesting.

As typically done in this type of study, to evaluate the most direct impact of harvesting, we selected headwater lakes, except for UP1, UP3 and P3. Upstream lakes of UP1 and UP3 were also unperturbed and in the case of P3, the upstream lake was very small. We have clarified this point in the text (see page 5 lines 113-116).

Carbon and nutrients can be transformed (i.e. immobilized, mineralized, evaded as CO_2) before being input into lakes in, for example, riparian 'hotspots' (see Ledesma et al. 2015 - Global Change Biology for a recent boreal reference) or within the stream channel. There is thus the potential for substantial changes in many of the lake water parameters measured before they enter each lake.

We thank the reviewer for this comment and reference which have now been included at the end of the discussion (see page 19, lines 452-454). However, the many processes that can occur before carbon and nutrients enter into the lakes are mentioned in the discussion.

Are any lake-water parameters correlated, both within lakes and pooling all lakes (other than the one correlation reported between the absorption coefficient and DOC)? It is very common for many of the variables measured to be correlated. P, for example, is commonly associated with DOC because it is adsorbed very easily to soil or sediment particles (that are DOC rich). DOC and P also happen to be the only parameters of interested that you have reported as responding to forest harvesting.

We thank the reviewer for this remark. We tested the correlations and we found that some parameters are correlated such as chl *a* and DOC (r^2 = 0.1202, F = 14.0689, p = 0.001), chl *a* and TP (r^2 = 0.0693, F = 7.5166, p = 0.007) but specially DOC and TP (r^2 = 0.2780, F = 8.2109, p = 0.005). As pointed out by the reviewer, this last correlation can be explained as an association between P and DOC. We have now included this information in the result section (see page 12, lines 284-286) and in the discussion (see page 13, lines 300-301).

Discussion – this is too long for the quantity of results presented. I am concerned with the disparity in what is discussed and what the study investigated. This research measured lake water parameters before and after forest harvesting, but they did not quantify the export, transport, or processing of variables before they entered each lake. Despite this, much of the Discussion is concerned with the processes that control the input of nutrients and carbon due to terrestrial disturbance and its transformation during / whilst being transported to the lake. It would be beneficial if the Discussion was more focused on directly relevant mechanisms and was less speculative.

We agree with the reviewer on this point. The paragraph about transport or processing of nutrients and carbon before entering the lake has been shortened.

Furthermore, there are many instances were conclusions are drawn that are not supported by the results or even the study design. For example, on Page 9321 (Line 22-25) the authors suggest that the input of logging slash due to harvesting was responsible for increasing DOC concentrations; however, this manuscript does not measure logging slash and cannot attribute changes in lake DOC concentrations to it.

Ok. This sentence has been removed.

Page 9309 (Line 15-17). It is stated here that lakes were significantly different one year after harvesting, based on the multivariate statistics. But I cannot find any evidence in the Results, Tables, or Figures that show evidence for this significance difference. Furthermore, on Page 9318 (Line 22) it states that 'PERMANOVA analysis revealed no significant interaction between treatment and year for the water characteristics and DOM variables.'. Perhaps this is a typing error in the Abstract?

We thank the reviewer for this remark. Yes, indeed this was a typing error in the Abstract which has now been amended.

Page 9309 (Line 18-19). In the last sentence of the Abstract please specify that the 'return to its original condition' is just in terms of the fluorescence and water quality parameters that you assessed.

This has been corrected.

Page 9316 (Line 16-18). Why was this interaction of primary interest? Please provide an explanation and references for your statistical choices.

This has been added (see page 10, lines 233-234).

Page 9316 (Line 20 - 21). Why were the same variables analysed twice in slightly different designs? Both analyses appear to be investigating differences between treatment and year, and thus the impact of harvesting upon them. It is best to just have one statistical analysis for each hypothesis/question.

We agree with the reviewer on this point. PERMANOVA analysis has been removed from data analysis.

Page 9319 (Line 6). Because you cannot rule out natural, site-specific variability in many of your response parameters, there are also numerous, natural processes which can cause elevated TP and also DOC. Please also include discussion of these possible natural factors.

We are not sure about what natural processes the reviewer refers to in this point. Factors causing elevated TP and DOC concentrations are discussed in pages 12-15 lines 293-351.

Page 9320 (Line 4-7). DOC can also be lost via heterotrophic respiration (i.e. CO2 evasion).

This has been added in the manuscript.

Technical comments

Page 9310 (Line 7). 'important' depends on what you value. I would suggest changing to 'extensive'.

Ok. This has been corrected.

Page 9310 (Line 23). Fellman et al. (2010) is a review article about DOM fluorescence and is not suitable to quote for this text. Please find more suitable references supporting the statement.

We have replaced Fellman et al. (2010) by Findlay and Sinsabaugh (2003). This reference shows the importance of DOC.

Page 9320 (Line 15). Please change 'sunny season' to something more technical such as 'summer' or 'summer and spring'. Also, please expand this sentence. Photodegradation only directly causes CO2 production from inorganic carbon in the absence of water. Furthermore, Winter et al. (2007) did not measure CO2, so remove this reference, or rephrase the sentence to only talk about the photodegradation of organic matter.

This sentence has been removed.

Page 9321 (Line 23). Please change ‘because the fast degradation’ to ‘because of the fast degradation’.

This has been corrected.

Page 9321 (Line 25). Please change ‘for few years’ to ‘for a few years’. Also, do you have a reference to support this statement? Because CPOM and woody debris can actually increase in streams in the first few years after harvesting.

Ok. This statement has been removed.

Page 9322 (Line 2). Remove ‘somewhat’. This has been corrected.

Page 9322 (Line 7). Please remove ‘amazing’. This has been corrected.

Page 9322 (Line 19). Please insert ‘the’ prior to ‘fluorescence’. This has been corrected.

Page 9322 (Line 25). Change ‘showing’ to ‘indicating’. This has been corrected.

References

Archambault, P., Banwell, K., and Underwood, A. J. 2001. Temporal variation in the structure of intertidal assemblages following the removal of sewage. Mar. Ecol. Prog. Ser. 222: 51-62.

Downes, B. J., L. A. Barmuta, P. G. Fairweather, D. P. Faith, M. J. Keough, P. S. Lake, B. D. Mapstone and G. P. Quinn, 2002. Monitoring Ecological Impacts: Concepts and Practice in Flowing Waters**.** Cambridge University Press, Cambridge, UK.

Keough, M. J., and B. D. Mapstone. 1995. Protocols for Designing Marine Ecological Monitoring Programs Associated with BEK Mills. National Pulp Mills Research Program, Technical Report No. 11. CSIRO, Canberra.

Underwood, A. J. 1991. Beyond BACI: Experimental designs for detecting human environmental impacts on temporal variations in natural populations. Aust. J. Mar. Freshwater Res. 42: 569-587.

Underwood, A. J. 1992. Beyond BACI – the detection of environmental impacts on populations in the real, but variable, world. J. Exp. Mar. Biol. Ecol. 161: 145–178.

Underwood, A. J. 1993. The mechanisms of spatially replicated sampling programs to detect environmental impacts in a variable world. Australian Journal of Ecology 18: 99–116.

Underwood, A. J. 1994. On beyond BACI: sampling designs that may reliably detect environmental disturbances. Ecological Applications 4: 3–15.

Response to **Anonymous Referee #2**

Thank you for the opportunity to review "Impact of forest harvesting on water quality and fluorescence characteristics of dissolved organic matter in Eastern Canadian Boreal Shield lakes" by Glaz et al. The authors have submitted a novel study looking at the effects of forest harvesting on water quality and DOM composition in northern boreal lakes. The unique aspect of this study is that they have used a before-after control impact style of design (although they did not identify it as such). Few studies on the impacts of landuse on DOM composition and DOC quantity have done this – many opt for space for time substitution (e.g. Yamashita et al. 2011, Burrows et al. 2013).

Furthermore, the authors express an interest in evaluating and using DOM composition as an indicator of forestry impact. Such indicators are sorely needed and are likely to be an important future area of research. However, I do have a few concerns about the paper in its current form.

General comments:

1. Please explain CLAAG in more detail and contextualize this in terms of other forestry practices that a broad audience might be familiar with. What may facilitate this is if you add a figure showing CLAAG in a watershed and an aerial of a non/CLAAG watershed.

We have arranged this phrase to better explain CLAAG strategy (see page 5, lines 121-123).

2. Write out your specific hypotheses. It will help us to understand the purpose of the study and how/why you undertook the study.

Specific hypotheses are enumerated at the end of the introduction (see page 4-5, lines 99-102).

3. A sentence or two on the history of forestry around each of these lakes would be useful. Is this old growth, second growth, third...? Has the harvest method changed? The legacies of past land use can be long (and hard to find) and may explain some of the variance in your dataset.

This is old growth forest. We have now specified it in the text (see page 5, lines 107-109).

4. Although you have employed a generally commendable experimental approach (BACI), your pre and post sampling is very short and gives us very little idea of the temporal dynamics and broader environmental variability. This makes it difficult to distinguish environmental drivers (climate and hydrology) from land use drivers. Was the rainfall for that month/season/year above or below normal? Similar for the temperature. Was winter snowfall higher/lower? Because your pre and post sampling is so sparse it is critical that you include these climatic and environmental variables so that those sources

of variation can be evaluated. For example the data you provide do suggest that a good bit more ‘precipitation’ fell in 2009 than in 2010. Is that snow and rain? A 54mm difference is not huge, but it may depend on when it fell and how intense it was when it fell and what the difference in rainfall vs snowfall is.

We agree with the reviewer that this information could be very useful to the reader. We have now included a table with monthly, seasonally and annual average of climatic variables during the three years of the study (see Table 2).

5. Can you tell us a little more about sampling. You talk about littoral stations for your sampling – was sampling done from a vessel or from the shore?

Ok. Sampling was done from a vessel. We have now added this information (see page 6, line 140).

In some of your lakes the littoral zone would probably represent the entire lake, in other lakes, I am more skeptical that the littoral zone would be representative of the entire lake. The assumption that, within a stratum, lakes are evenly mixed is often made by managers, and in some lakes this holds true. However, when lakes become small or more complex this rule of thumb tends to break down. This is particularly true when comparing littoral and pelagic zones. You also say that sampling was random, but some of these lakes are reasonably medium – was sampling random near the point of access or random around the entire lake? If it isn’t random, that is ok, just be specific about how you selected the sites.

Sampling was random around the entire lake.

6. I have some concerns/questions/comments about sample handling and analysis – these are collected in the following more specific comments.

6a. Freezing TP samples will change the concentration observed 10.1016/j.scitotenv.2007.11.027 you might comment on this briefly, it may not be a huge effect.

Fellman et al. (10.1016/j.scitotenv.2007.11.027) suggested that samples with high SUVA values should be analyzed immediately for TP and not frozen but samples with low SUVA can be frozen. As shown in Figure 3, our samples had acceptable SUVA values to be frozen.

6b. The use of the NPOC (I am assuming, you might mention if it was NPOC or TCIC) method on the 5000a can bias high DOC measurements low. Lowering the pH, especially below 2 as this method is supposed to do, can change the solubility of DOM driving more hydrophobic DOM to the walls of the glass containers it is being sampled from. This effect can be readily observed on the 5000a (and even the new TOC-L) if you run the TC-IC and then the NPOC. The Hansell reference materials may or may not be affected by this because much of the humic matter may already be absent as those are marine-derived CRMs.

The DOC samples in 2008 and 2009 were analyzed with a Shimadzu TOC V-5000. In 2010, the sample were analyzed with a TOC-V_{CPN} . In all cases, the instrument was used in NPOC mode.
We agree with the reviewer that a change in the pH can change the solubility of organic matter. However, it is accepted that humic substances form most of the DOC in natural water. In stream or lake water, humic substances represent 50-80% of the DOC (Thurman, 1985) and 10-50% in surface estuarine water (Tremblay and Gagné, 2009). Usually, fulvic acids are more abundant than humic acids in dissolved humic substances or DOC. The ratio of fulvic/ humic acids observed in fresh water is around 5/1 (Gagné, unpublished data). In a soil solution, soil or sediment, the ratio of fulvic to humic acids can be lower and humic can dominate the composition of the mixture (Kononova 1966, Tremblay and Gagné 2007). In this study, we measured DOC in lake water and not in soil solution or soil. Thus, fulvic acids were an important component of the DOC. Moreover, the spectroscopic results obtained in this study support the presence of fulvic instead of humic acids in lake waters. By definition, fulvic acids are soluble at any pH. Then, the acidification of the samples does not change the concentration of fulvic acids. We cannot exclude a small decrease of humic acids content in our samples. However, we never saw a precipitate on the cap or the glass wall of our vials. We expect then that the acidification of our samples did not change significantly the concentration of DOC (and HS) in our samples.

6c. (FYI) The concentration in your CRM is low relative to the range you test in the instrument. This means that you may have large absolute errors at the higher end of measurement. It is better if your CRMs are in the range of your samples.

DOC was measured using the high temperature catalytic oxidation (HTCO) method with NDIR detection on Shimadzu analyzers. A calibration curve was used, with five concentrations of potassium hydrogen phthalate between 0 and 10 mgC/L to determine the DOC content of samples. Instrument linear response has been checked over a larger range of concentrations. The Shimadzu instruments are very stable and give a linear response for concentration range between 0 and 100 mgC/L, the highest concentration tested. Hansell's Certified reference Materials was used to check the performance of the instrument. The instruments give always a response in the range of Hansell CRM values.
This information has been added in the text (see page 7, lines 160-168).

6d. How long between collection and analysis of refrigerated samples for DOC and fluorescence?

There was a period of one month approximately between collection and analysis of refrigerated samples for DOC and fluorescence.

6e. More information is needed on your fluorescence procedure. Did you correct for instrumental bias (See Cory et al. 2010 in L&O methods)? List integration time, PMT voltage/gain, and cuvette size.

We agree with the reviewer. The description of fluorescence measurements lack details and we have added this information in the text (see pages 8-9, lines 193-209) .

Did you dilute your samples prior to analysis to avoid inner-filter effects or did you ensure that your samples were in an appropriate absorbance range for a mathematical inner filter effect calculation (see Ohno 2002 in ES&T)? At 10+ mg L-1 DOC you almost certainly had inner filter effects.

Prior to fluorescence analysis, the absorbance of each sample was measured with a UV-VIS spectrophotometer (PerkinElmer Lambda 35). If the absorbance of the sample was higher than 0.05 AUFS, the sample was diluted to obtain absorbance in the range 0.02-0.03 AUFS. At this absorbance, the first and secondary inner filter effects are negligible (Lakowicz, 2006) and no correction has been done for the inner filter effects. Under these conditions, the fluorometer was never saturated. This has now been added in the text (see page 8, lines 196-201).

Both of instrumental bias and inner filter effects will significantly change the shape of your EEM and the fluorescence indices you calculate – sometimes enough to change your interpretation from microbial source to terrestrial source.

Cory et al. (2010) and Lakowicz (2006) have discussed how difficult it can be to eliminate instrumental bias. In this work, we avoided inner filter effects and corrected the EEM for lamp and instrumental bias as suggested by Cory et al. (2010). Then we used FI index to discuss of DOC. It is possible to use the same fluorometer, without corrected data, to find trends or relative variations for a large sample set (Senesi 1990).

7. In general the use of optical metrics could be expanded to improve the analysis of how DOM in the system may or may not change following forestry.

7a. 9321ln27-28 Simple CDOM absorption is not a measure of DOC quality, it is a measure of quantity.

We agree with the reviewer. CDOM absorption at a specific wavelength is a quantity instead of quality parameter. We have deleted this statement.

The input of DOM to lakes from surrounding landscape could produce changes in the amount and the chemical composition or quality of organic matter.

In this study, the quantity and the quality of the CDOM were monitored by measuring aCDOM at a specific absorption wavelength (355 nm), and by acquiring the UV-VIS spectra of the CDOM (200 to 600 nm) for each sample. The quality of the CDOM did not appeared to be affected by forest harvesting since no differences were observed between the UV-VIS spectra (data not shown). This observation suggests that the quality of CDOM in lakes was similar between years. In regard to aCDOM (355), this parameter also did not appear to be affected by forest harvesting since no significant differences were found for the interaction between year and treatment although total DOC concentrations increased one year after. These observations suggest that the CDOM composition did not measurably change, which is also supported by fluorescence measurements.

Normalizing it to an independent measure of quantity (e.g. Shimadzu DOC) is a measure of quality.

We do not completely agree with the reviewer on this point. Normalizing absorbance by the DOC content provide the absorbance by mass unit carbon. This is rather a quantity than a quality indicator because we do not know about the chemical structure of the carbon present. In the specific case where we measure the absorption at 254 nm, we measure the quantity without knowing the chromophores producing the absorption. However, chemistry allows hypothesizing the presence of aromatic moieties in the DOC at 254 nm (NMR data Weishaar et al., 2003). If we divide the absorption or absorption coefficient at 254 nm by the Shimadzu DOC, we obtain a quantitative ratio representative of the absorption by carbon unit that we interpret considering the qualitative hypothesis that absorption signals at 254 is representative of aromatic moieties.

The slope of the relationship between CDOM and DOC among treatments would also be an indicator of quality and could be calculated.

As previously discussed, the slope or ratio CDOM/DOC is a quantitative indicator and not a strong indicator of the quality of the DOC because we do not have the information on the carbon moieties causing the absorbance.

7b.The fluorescence index. This is a relative indicator of source – relative to your known DOM characteristics and relative to your instrument (if it hasn't been corrected for bias). Also, for some reason the McKnight et al. 2001 values of the FI persist as reference benchmarks in the modern literature. Those values were generated without instrumental bias correction and are much higher than values collected on the same instrument corrected for bias - see Cory et al 2010 in L&O Methods for a thorough discussion of this and better FI reference benchmarks.

We used fluorescence index to discuss the source of DOM. The FI values obtained in this study (with correction for instrumental bias) were around 1.65. These values are higher than the value suggested by Cory et al. (2010) after correction for instrumental bias.
There is no standard protocol to measure fluorescence index. Cory et al. (2010) suggested to correct EEM for instrumental bias. However, which are the best pH, salinity and CDOM concentration to use? Korak et al. (2014) showed in Figure S-12 and S-13 that FI values can vary by 0.2 units if DOC concentrations change. The highest values are observed at low DOC concentrations. In our study, because CDOM concentration is low

(absorbance in the range 0.02 AUFS), this could contribute to the high value of FI observed. This is now discussed in the text (see page 15, lines 364-372).

7c.Why did you use the Ratio of A/C? What is it telling you? See Coble 2007 (DOI: 10.1021/cr050350+) for an updated view of what those letters can be attributed to – there is a lot of variability and little consensus.

Peaks A and C were the peaks observed in our fluorescence matrices. Any changes in the ratio of the intensity of these fluorophores, independently of their origin, will inform us about change in the proportions of these fluorophores. A constant ratio A/C suggests a constant composition, a stable input or a stable environment.

7d.Calculate the HIX index – the humification index - as this has direct bearing on the type of DOM you have in a boreal forest.

We are of the opinion that we have calculated many spectroscopic parameters ($a_{cDOM\ (355)}$, fluorescence ratio A/C, FI, BIX, $SUVA_{254}$ and spectral slope), all of them showing that DOC composition was similar for the three years in unperturbed and perturbed lakes.

7e.Why did you calculate the spectral slope? What would it tell you?

Spectral slope is used to provide information on change in the composition/quality of CDOM, including the ratio of humic to fulvic acids (Twardowski et al. 2004; Fichot and Benner 2012; Galgani et al. 2011).

7f.Calculate the slope ratio (See Helms et al. 2008 in L&O). It can provide information on relative differences in composition and may indicate photo-bleaching. Given that you have incredibly shallow systems, I would be surprised if photodegradation was not at work.

Please see response to comment 7d. Also, we are of the opinion that the slope ratio is meaningless in our system since physicochemical conditions in our study are completely different from ecosystems analysed by Helms et al. (2008).

7g.Lastly, and this is optional, consider fitting your data to a PARAFAC model – Cory and Mcknight would probably be appropriate for your context – the Fellman 2009 model would also be appropriate if you can get ahold of it. It is better to generate your own if you have enough eems and enough variance in your eems, >50 can usually give you a valid model.

PARAFAC model could be very interesting when the samples show numerous peaks. However, when only two peaks are detected, the use of PARAFAC seems unnecessary.

Specific comments:

9309.5: Name the design as Before After Control Impact (BACI).

Our design is not a formal BACI design since we only have one sampling date before the perturbation.

9309.8-10: This is unclear. What do you mean by ‘when all sampling dates were considered.’

We have replaced ‘all sampling dates’ by ‘three sampling dates’ (2008, 2009, 2010).

9309.18-19: You cannot conclude this with your dataset.

We have added ‘in terms of water quality parameters assessed in this study’ at the end of the phrase (see page 2, lines 30-31).

9309.21-22: Awkward, rearrange to: ‘Boreal forest, which contain...’.

This has been rearranged.

9310.1-4: Here and elsewhere, you might consider reading and citing some of the Hubbard Brook literature as that study was designed around the impact of logging on water quality.

We thank the reviewer for this reference which has now been added.

9310.4-6: This is too much detail, move this sentence down.

This sentence has been removed.

9310.4-7: Aren’t hydrologic impacts also important?

We replaced ‘important’ by ‘extensive’.

9311.2-4: Delete this sentence, it is redundant with 9310.18-20.

This sentence has been removed.

9312.21: Why July? Explain why you selected this month and not a month when there is more rainfall and the terrestrial area might be better connected to the aquatic environment.

The access to the sites was difficult before July and after July it was about logistic issues. This was also the best time for the forestry company who exploits the area to provide land use and field facilities. Finally, this study was a part of a broader project to assess impact of forest harvesting on trophic structure of lakes. Sampling was done in July to get stable isotopes signatures of benthic invertebrates and fish. Stable isotope signatures of these organisms measured in July gave us a good picture of what was assimilated by these organisms over June which corresponds to the period of loss of lake ice.

9313.22-25: This is full of forestry jargon and makes no sense. “Preserving advanced growth” sounds like preserving large trees, but you later say that the method removes all trees over 10cm. What is ‘site stocking’? What is ‘seed source (larger trees)’. I looked at the Landsat imagery for these sites and this just looks like a clearcut / heavy partial cut.

We agree with the reviewer and we have removed the sentence which contains too much forestry jargon.

9314.2: 0.22 um filters are almost always membrane and not GFF. I cannot find a 0.22 um sartorious GFF.

Yes, indeed we referred to membrane filters. This has been corrected.

9314.11: Consensus not Certified. This has been corrected.

9314: Eqn 2. What is 1, 9314.24 what is l, I don’t see that in eqn 2.

We thank the reviewer for this remark. In fact there was a typing error since it is not 1 but ‘l’. We have now replaced ‘l’ by ‘L’ in capital letters to avoid confusions. L is the pathlength of the cell used in the absorbance measurement in meters.

9315.6-eqn(3): I would delete this for two reasons. First, you do not really use these data in subsequent analyses. Second, such linear transformations of data add little to the analysis because they do not change the variance of the data. Most statistical analyses focus on the variance in the data. A linear transformation does not change the variance so any results will be the same. It is sufficient to say that higher values of SUVA reflect more aromatic DOM.

We completely agree with the reviewer on this point and we have deleted this equation.

9316.21-22: Have you tested for overdispersion? The problem with PERMANOVAs is that they tend to confound location effects and disperion effects. That is to say you could have a significant difference because you have different means (locations), or because you have different variances (dispersion).

As suggested by the Reviewer #1, PERMANOVA analysis has been removed from our analysis.

9317.1: A range can’t have a standard deviation. It can have 1 minimum and 1 maximum value.

This has been corrected.

9317.9: I think this underscores the problem of insufficient characterization of natural environmental variability in this manuscript.

This is discussed further in discussion.

9319.19-25: This is a very awkward section. Please revise to clarify your meaning.

This section has been rearranged to clarify our meaning.

9321.27-28: Simple CDOM absorption as you have presented it is not a metric of DOC quality.

This has been corrected.

9325.14-16: Why is this hidden here at the end? This needs to be in the site description, also report it for all 3 years of study.

See Table 2 and response to above comment.

9325.28-9326.2: Your data are not strong enough to support such a strong statement.

This statement has been removed.

Technical comments:

9312.11: add 'forested' after 'on the' and delete 'and all areas is forest land' on line 9312.15.

This has been corrected.

9311.23: complementary not supplementary.

This has been corrected.

9317.1: statistically not statistical.

This has been corrected.

Limit numbers in all tables to 2 or fewer sig figs.

This has been corrected.

I am not sure the purpose of figure 4, I would delete it. The [DOC]-UV absorbance relationship is pretty well established.

We agree with the reviewer. Figure 4 has been deleted.

References

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Response to **Anonymous Referee #3**

Summary: The manuscript by Dr. Glaz and co-authors describes the changes in lake chemistry and DOM quality in boreal lakes following forestry activity in the lake's catchments. The overall findings suggest that changes for Phosphorous and DOC concentrations occur, but that the responses are no longer significant after the first year following harvesting has passed. The study is of interest for the readership of the journal 'Biogeosciences', as it links the basic understanding of lake and catchment biogeochemistry with the applied question of how long legacies of land management from forestry operations will remain in boreal lakes. The quality of the work is good. I was intrigued by the replicated study design and the extensive statistical analysis using both, regular and permutated versions of an ANOVA. Overall, I support the publication of this manuscript after the comments listed below have been addressed.

Main comments:
- The model for residence time (Equation 1): According to my understanding, the inherent assumptions of this model are not appropriate. First, it is easily possible to use a common runoff coefficient for boreal forests from the literature (See for example Bosch and Hewlett, 1982 as a classical study or Schelker et al., 2013a as a more recent) to assume the percentage of P that becomes runoff (value around _0.5 for undisturbed forest) instead of just using precipitation. Second, this value changes as a response to harvest to a higher value (value around 0.7 to 0.85, same references), which will subsequently decrease the residence time in perturbed lakes. Please consider these mechanisms and add them to your model. Then the authors should evaluate how these simple improvements change their results.

We thank the reviewer for this remark and for the references. We have added the runoff coefficient to our model. However, the mean annual lake residence time of water in the lakes studied are so short (< 0.16 years) that this improvement in the model does not change the interpretation of the results.

I suspect that using a slightly more complex model that acknowledges evapotranspiration in a simple way will make it easier to argue that the DOC signal is more related to the inflows, versus the in-lake processes, which will improve the discussion section.

We completely agree with the reviewer on this point. However, we did not find a simple way to model evapotranspiration and thus we assumed no loss of water by evaporation and evapotranspiration to the atmosphere.

Finally, the wording 'residence time' is not very good, as such a simple model suing annual precip. will only be able to estimate the 'mean annual lake residence time'. Please consider renaming the variable.

We agree with the reviewer and we have renamed the variable as 'mean annual lake residence time'.

- The introduction is generally well written, some sections are, however, too general and do not properly review the actual biogeochemical processes. Sometimes only lists of processes are provided without more discussion on why they are important. It feels like it is only scratching the surface of the topic. I have indicated several instances where improvements can be done in my detailed comments below.

- The same is to some degree true for the discussion section. Here a bit more than just a list of possible mechanisms is needed. Instead, please compare your results in detail to other, similar studies and develop a red line for the discussion. Even if the authors cannot prove which mechanisms are acting, they should be able to narrow down which actual processes are most likely to create the observed patterns in their study system. Also here I have given some points to look at below.

Minor comments:

Introduction:

P9310, L16: suggest replacing ’vegetation’ with ’vegetation cover and plant community’.

This has been changed.

L17: A good reference for the temp and moisture statement is Schelker et al., 2013b.

We thank the reviewer for this reference which has now been added.

L26: The statement is so general, that it does not contain very much information that is needed for the reader to understand the current paper. I doubt that this many references are needed here. Suggest removing at least two.

Two references have been removed (Anesio et al. 2004 and Judd et al. 2006).

P9311, L2, replace ’a recent study’ with ’the recent study’; it is the same study you talk about in the sentence before.

As suggested by Reviewer #2, this sentence has been removed.

L7: what is a ’system’ here? ecosystem? aquatic system?

We have specified a system as an ‘ecosystem’.

L21: ’to lakes’ or ’into lakes’ sounds more right to me.

This has been corrected.

L24: ’an interesting and a supplementary technique’ sounds fishy... how about ’a new tool’, ’an appropriate tool to study...’.

This has been changed.

L27: suggest the authors extend here a bit adding information on how all the other cited publications, such as Fellman et al., 2009, McKnight et al., 2001... have used the tool to distinguish DOM from different origins. Also, there are many, many more recent papers, that do this . Please give a better overview here.

We agree with the reviewer and we have amended the text accordingly.

L28 and following: aim and hypothesis read well.

Methods:

L9312, L15: any guess what the percentage open water in this landscape is? Right now it sounds like there isn’t any, but considering Fig.1 it would be good to add this.

We do not know the percentage open water but Fig. 1 clearly shows the presence of numerous lakes and one river in the study area.

L17: What does the term: ’morphometric’ mean here? Something like geomorphologic? I am not familiar with it - suggest to replace.

This has been corrected.

P9313: L4: Great to present the detail that all treated lakes have >65% harvest of the catchments area, which should help get a good quantification of the treatment effects.

L6: Please see main comment on the estimation of residence times.

See answer to main comment on the estimation of residence times.

L16: the brand ’Alpha’ is really not that interesting. The important question is out of which material the bottle(s) were made. Please revise.

This has been corrected.

P9314: L 15: remove ’bottles’.

This has been corrected.

L17: Why only three locations?

This is about logistic issues. Also, we considered that three locations were enough to perform ANOVA analysis on those parameters.

L23: please provide reference for formula. It can't have come from nowhere.

We have provided a reference for the formula (Kirk 1994).

P9315: L8: A commonly used abbreviation for the Spectral Slope is Sr or S_sub_R. May be worth introducing it here.

We have used S as abbreviation for spectral slope. Sr is the abbreviation for the slope ratio (Helms et al. 2008).

P9316: Very clear description of the stats! Also, I fully appreciate the use of permutation-tests!

Results:

P9317, L6 suggest to add 'first' to 'year after harvesting'.

This has been corrected.

P9318, L4-7: I suggest to do two things here: First, use abbreviations for emission/ excitation (for example Em. and Ex.) and use these throughout the manuscript. Second, as you have described these to humic peaks already in the methods, simply refer to them as peak A and C and do not restate all the details.

This has been corrected.

L19-20: percent sign is the only unit that should be behind the number without a space. Also, it should be added to both numbers.

As suggested by Reviewer #2, aromaticity equation has been removed.

Discussion:

9319, L6-9: Does Kreutsweiser really show all this? As I remember, some of these findings are only mentioned as 'believed' responses to harvesting. For example, I am not aware of any study that can really show higher soil microbial activity following harvesting with current microbiological methods. If there is, I am happy to be corrected. Also, the statement is so general, that it has little value for the discussion. I would therefore suggest to be either more specific on what has been explicitly shown and what is 'only believed' to happen following harvesting or to remove this statement entirely and go directly into the 'processes affecting P' discussion.

We agree with the reviewer and we have removed this statement.

L11-13: Is this an important process in boreal regions, if _20m buffers are kept around lakes? I doubt it, as the slopes of the landscape are simply too low for extended pmineral-particle transport. Maybe this can be included in the discussion. This would also point towards the DOC link with transport.

As pointed out by Reviewer #1, P is commonly associated with DOC. Furthermore, we tested the correlation between DOC and TP and we found that they were significantly correlated.

9320, L6: I am not sure if sedimentation is only possible as a complexation with clayminerals, as flocculation in the water column can occur for different reasons (see for example literature on ’marine snow’). Suggest to revise that ‘mineral complexation is one mechanism’ leading to sedimentation.

We agree with the reviewer and we have removed this statement.

L15: ’the photodegradation could be efficient to transform autochthonous organic matter to CO2’ - I am not sure I agree here. To my understanding the C-DOM pool in these lakes will be dominated by humic fractions. This stable, allochtonous DOM would be predominantly degraded by sunlight to more bioavailability forms, which is then respired by heterotrophic bacteria (Wetzel et al., 1995), doing the opposite of what you state here. Please clarify. I agree overall with the statement that the terrestrial deliveries will be/ are most important, but the current argumentation does not convince me of this as a reader.

Following the suggestion by Reviewer #1, this argument has been removed.

L25-30: This long sentence is another long list of possible processes, but as a reader I don’t know more of why they are relevant for interpreting your results. Please decipher for each of the named processes, what their expected effect would be (+reference), what of this is observed in the results and what indications there are for or against this process playing an important role in overall understanding.

We agree with the reviewer on this point and we have now removed this long sentence to better focus on processes occurring in our system.

9321, from L5 it starts reading better. Just continue to go a bit more into details of what exactly the references tell and do not list them all together. Also, the authors should remember that the largest C-efflux from a forest floor is soil respiration, which has been shown to change as a result of forest disturbance (for example Grant et al., 2007). So whatever decomposition happens at a soil surface may be disconnected from what contributes to the aquatic DOC.

We thank the reviewer for raising this point which has been added into the manuscript (see page 14, lines 341-342).

P9322, L2: the hypothesis of 'no change of DOC character following harvesting, despite concentration changes' has been around for a while for streams. Maybe Burrows et al. 2014 would be a reference for this.

We thank the reviewer for this reference which has now been added into the manuscript (see page 15, line 356).

Figure 1: is not very good. I would suggest zooming out on the small map that people can see where this really is. Also, almost all text is too small and the abbreviations are not explained. Any chance to also plot the lakes catchments as dashed lines?

Ok. We have enlarged the small map and all the text. The abbreviations are indicated on the figure caption.

Figure 2 and 3: Same as above: too thin lines and too small text/axis titles and numbers.

Axes titles and numbers have been enlarged and lines are thicker now.

Figure 4 is great – simple and clear.

Tabel 1: A very good summary of the general lake characteristics.

Table 2 and 3: Good summaries. I suggest you mark all significant p values as bold. This is commonly done to allow the reader to quickly categorize the relevance of the variables.

This has been changed.

References

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