Biogeosciences
Discussions
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Interactive comment on "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.

Anonymous Referee #3

Received and published: 26 August 2015

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.

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. 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 be consider renaming the the variable.

- 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 can not 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'

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

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.

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

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

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

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

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.

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.

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

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.

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

P9314: L 15: remove 'bottles'

L17: Why only three locations?

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

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

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'

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, sinply refer to them as peak A and C and do not restate all the details.

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.

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.

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 p-mineral-particle transport. Maybe this can be included in the discussion. This would also point towards the DOC link with transport.

9320, L6: I am not sure if sedimentation is only possible as a complexation with clay-minerals, 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.

L15: 'the photodegradation could be efficient to transform autochthonous organic matter to CO_2' - 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.

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.

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.

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.

âĂČ 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?

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

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.

References:

Bosch, J. M., and Hewlett, J. D.: A review of catchment experiments to determine

the effect of vegetation changes on water yield and evaporation, J. Hydrol., 55, 3-23, 10.1016/0022-1694(82)90117-2, 1982.

Burrows RM, Magierowski RH, Fellman JB, Clapcott JE, Munks SA, Roberts S, Davies PE & Barmuta LA, 2014. Variation in stream organic matter processing among years and benthic habitats in response to forest clearfelling, Forest Ecology and Management, 327, 136-147.

Grant, R. F., A. G. Barr, T. A. Black, D. Gaumont-Guay, H. Iwashita, J. Kidson, H. McCaughey, K. Morgenstern, S. Murayama, Z. Nesic, N. Saigusa, A. Shashkov, and T. Zha (2007), Net ecosystem productivity of boreal jack pine stands regenerating from clearcutting under current and future climates, Glob. Change Biol., 13 (7), 1423-1440.

Schelker, J., Grabs, T., Bishop, K., and H. Laudon (2013b), Drivers of increased organic carbon concentrations in stream water following forest disturbance: Separating effects of changes in flow pathways and soil warming. Journal of Geophysical Research: Biogeosciences 118 (4), 1814-1827.

Schelker, J., Kuglerová, L., Eklöf, K., Bishop, K. and Laudon, H. (2013a). Hydrological effects of clear-cutting in a boreal forest – Snowpack dynamics, snowmelt and streamflow responses. Journal of Hydrology 484(0), 105-114,

Wetzel, R. G., P. G. Hatcher, and T. S. Bianchi (1995), Natural photolysis by ultraviolet irradiance of recalcitrant dissolved organic matter to simple substrates for rapid bacterial metabolism, Limnol. Oceanogr., 40(8), 1369–1380.

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Interactive comment on Biogeosciences Discuss., 12, 9307, 2015.

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