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

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Review of Olefeldt et al by Susan Waldron www.carbonlandscapes.org

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.

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.

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 non-expert to be able to understand significance and rationale from the paper alone.

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.

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?

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?

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

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.

4. L16 p 6098: slopes not slope

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.

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.

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

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. . .?

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

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.

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

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

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 canot be detected, particularly without knowledge of what is was prior to 2011.

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?

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.

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.

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?

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.

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