

Response to interactive comments of Reviewer 3 (bg-2018-516)

We thank reviewer 3 for the helpful comments that will aid in significantly improving our paper, particularly through clarification of the measurements made and our interpretations of water and DOC mobilization in the boreal watershed context. Our response to specific comments (reprinted in bold) are provided below

The study by Bowering and coauthors presents a thorough survey of carbon exchanges between above and belowground terrestrial pools, compared across pristine and harvested boreal Canadian landscapes. The findings are linked to environmental conditions, in the context of changing climate and hydrology in the region. The relatively high temporal resolution of the dataset provides insight into cross-season differences in the controls on soil DOC export between harvested and pristine plots, a clear strength of the paper. Also, I really liked how the authors explicitly discuss the importance of their findings for the parameterization of larger forest carbon cycle modelling efforts. I recommend below a few general and specific changes to the current manuscript that could further strengthen the paper.

General:

-Introduction as written does not cover the effects of forest harvesting and the state of knowledge regarding forest/soil C cycling impacts. A bit of context here is important because the cross comparison of plot types is a big theme. Also, page 11, lines 22 and on contains key findings that would be better showcased if the effects/unknowns related to harvesting are introduced earlier in the paper. To that end I recommended below citing a recent review on this topic (James & Harrison. Forests 2016, 7, 308; doi:10.3390/f7120308) that could be used as context in the introduction and discussion.

The introduction will be revised to include more information on the known/unknown effects of harvesting on soils and DOC.

-Consider adding a simple drawing that summarizes the fluxes and pools of C measured here, perhaps boxes and arrows sized to pool sizes and flux rates, respectively. Not critical since table 1 has much of the information, but a figure like this could really help readers follow key findings as they are presented in the discussion.

Will consider for the revised manuscript to see if inclusion of such a figure will aid in communicating the findings more readily. We would like to see if this can be done without creating any misinterpretations given that not all C fluxes were assessed in this study.

-The concept of the net ecosystem carbon budget (NECB; Chapin et al. 2006 Ecosystems; Webb et al. 2018 Ecosystems for a nice review) is not directly presented, but could be useful context. Even though not every single C flux is measured here, the discussion does revolve around this concept, and the authors are measuring a key flux term (hydrologic DOC export) that has often been overlooked in earlier efforts to build C budgets. Consider introducing this

early in the introduction and again in the first 2 paragraphs of the discussion. Such discussion would fit nicely with the summary drawing figure suggested above.

We will include NECB within the first paragraph of the introduction where the fate of soil C is discussed.

Specific:

P1, l. 18-26. Abstract could be shortened. Consider summarizing results/correlations more succinctly.

The abstract will be shortened and edited to highlight key findings.

P1,l.25. Flushing means what exactly? DOC removal? Maybe say flushing of DOC. P3,l.16. grammar

Yes, flushing refers to the removal of DOC from soil pores during large water flux events. We will change to “flushing of DOC” to clarify.

P3,l.20. Could add conclusion sentence summarizing the outstanding issue that is motivating your study.

We will consider this idea and revise the conclusion statements accordingly.

P7,l.4. Add shot sentence explaining how soil respiration calculated.

Will add

P7,l.20. What package in R was used for the LMEs?

The “nlme” package was used. Will provide in manuscript

P8,l.26. Introduce the soil thickness measurements shown in Table 1 here too.

Will be revised

P8,l.26. In Fig. 1, reorder the panels so that soil respiration is numbered according to when it is introduced.

Will revise accordingly.

P8,l.31. What do you mean by partial melt?

Only a portion of the snowpack melted during this period. Will be revised in manuscript to clarify.

P9,I.7. Should current fig. 1b be current fig. 1c?

Should be both 1B and 1C. Will revise

P10,I.3. reword “were not found” to “was” if singular.

Will revise

P10. Order of figure introduction is confusing throughout entire page. Could rearrange existing text so that corresponding panels from Fig. 1 introduced first, Fig. 2 second.

Will consider and revise.

P10,I.11. How much? Consider adding a percentage value.

Will revise

P10,I.16. Snow depth?

Yes, will revise

P10,I.18. Rain throughfall?

Yes, will clarify

P11,I.9. Add “was” before “observed”.

Will revise

P13,I.12. Take pgph 1 step further with conclusion sentence that links back to your results.

Will consider and revise

P13,I.24. Whys is winter included here? Don't 3a and 3b depict linear increases, while 3c depicts the plateau? Should the reference to fig. 3b be included in line 25? Maybe I missed something but this could be clarified.

It seems that the plateau begins towards the end of autumn when large water fluxes begin to occur, as a result of reduced ET, and that's why 3b was included. The trendlines to Fig 3 you suggest below could quantify and clarify this section.

P14,I.1-3. Excellent conclusion. Consider repeating exactly like this in the abstract to shorten there.

Great, thank you. This is helpful feedback. Will include in the abstract.

P14,I.5. Tough to support the statement that winter fluxes were “dynamic” with only 1 measurement there, so consider rewording that.

Will consider and reword

P14,I.18. Could end this section with stronger discussion of the implications of these results. Same comment goes for the next section too. Is the timing of the precipitation the key? How well is this established in earlier studies? Could take this back to the broader literature.

These are good points and would strengthen the discussion of our key results concerning seasonality of DOC. Will carefully consider how to include a stronger discussion of these implications and placing this within a broader context.

P15, I.5. Important end to the sentence, but awkward as currently written. Consider rewording.

Will revise

Fig. 1. Center the Y-axis titles on each panel.

Will revise

Fig. 3. Consider adding trendlines to quantify the different seasonal relationships.

Will consider and revise