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Interactive comment on "Dissolved organic carbon mobilized from organic horizons of mature and harvested black spruce plots in a mesic boreal region" *by* Keri Bowering et al.

Anonymous Referee #2

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The manuscript presents a thorough assessment of DOC fluxes in boreal landscapes and how they might be affect by forest harvesting and climate change. Principally, the study is well designed and the manuscript is nicely written and the results contribute to our understanding of DOM mobilization in boreal forests. The major shortcoming of the manuscript is the estimate of water (and thus DOC) fluxes that is based on water collected using passive pan lysimeters. Although they seemed to be well designed (using glass beads to mimic a hydrological continuum), it remains uncertain how well they functioned (e.g. by tracer). While water recovery was tested to be 90%, measured water drainage was found to exceed rainfall inputs (+50%) and thus measured drainage was about twice as high as what one could expect. In the discussion, the discrepancy

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was explained by lateral flow contributing. This implies that the lysimeters acted as funnels draining a greater footprint area and thus, comparisons of DOC fluxes with soil CO2 effluxes are not valid as they originate from different areas. To me an appropriate estimate of water fluxes seems crucial for the manuscript as the discussion centers all around a mass balance comparing DOC with soil CO2 effluxes. I would strongly recommend to use a water balance model to estimate DOC export from the organic layer or provide clear evidence on lateral flow or the footprint area. More information on the set-up of the lysimeters and the site conditions (slope) should be added. In contrast to the uncertainties related to the quantitative estimates, conclusions made in relative terms e.g. harvest effects, seasonality etc. are still valid and merit publication.

Specific comments:

Abstracts L. 23 ff An Abstract should be informative and contain the key data. The implication/conclusion section is much too long, 10 lines. I missed values and comparison with soil CO2 effluxes and forest management aspects.

Methods Page 5, Line 5ff lysimeter set-up "It was desirable for this study". . .please describe what was exactly done and give details on glass beads (size classes), depths of the glass bead layer, length x width of the lysimeter, connection of lysimeter to sample container etc.. How was it installed? Was the organic layer completely removed beforehand? A sketch added to the Supplemental Information might be helpful. According to the test described it seems that lysimeters functioned well but why did they not collect lateral water in your test but later during the regular monitoring? The appropriate capturing/estimate of water fluxes is crucial for estimating DOC fluxes and thus lysimeters known to create sampling artefacts should be tested rigorously (e.g. by a tracer) or backed up with modelling of water fluxes.

Page 8, Line 15 453 cm as snowfall, typo? If indeed snow depth is meant, please transform it to water equivalent.

Page 8, line 19 I would recommend to report no decimal for rainfall (which is beyond

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any precision possible). . .

Page 9, Line 26 clarify that you mean the SOC stock in the organic layer.

Page 10 How can the water flux in the O horizon (1366 and 2040 mm) exceed or be in the same range as the input via rainfall (1305mm)? Estimates of water fluxes are crucial as DOC fluxes directly depend upon water fluxes. Generally, this is done via modelling of water fluxes (see papers by Fröberg et al., Kindler et al., 2010 GCB). The values you provide indicate that the lysimeters worked well (which is not always the case) but that they might fetch water from a greater area or include a lateral component. How does the topography of the site looks like (no information given in the methods. . .).

Page 10 Line 16 please rephrase the sentence – and clarify that 'corresponding to a total depth of 84 cm and 110 cm' was the snow depth when snow/water was sampled (?)

Discussion Page 11, Line 13ff As the DOC fluxes seem to be very high due to an overestimate of water fluxes, the discussion includes a high uncertainty. At a rainfall of 1300 mm, evaporation rates of 100-200 mm and a evapotranspiration of approx. 3-500 mm, the DOC fluxes are probably a factor of two smaller than estimated here. This is also relevant for the comparison with other C fluxes/pools.

Page 11, Line 30ff here it needs to be clarified that the greater water flux drives the management effects

Page 12, Discussion of lateral water fluxes. The appropriate estimation of water fluxes is crucial for the overall manuscript (and appears very late in the discussion. Based on the values given, I was wondering much earlier that something went wrong). Lysimeters are known to have artefacts as they alter the soil continuum: they can act either as a funnel or as a barrier depending on the soil conditions. I would thus not rely on the assumption that the lysimeters used here captured water fluxes (horizontal and lateral ones) correctly. Probably, there is lateral flow (what is the slope of your site?),

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but the estimate provided here is too speculative. Moreover, is laterally moved DOC a real export? How can you compare total DOC export (lateral and vertical) with soil CO2 effluxes in quantitative terms? I recommend to model water fluxes and use these values to estimate vertical DOC loss from the O-horizon.

Page 14 comparison with soil CO2 effluxes. You might estimate the seasonal pattern of DOC vs. soil CO2 effluxes (or their temperature dependencies. DOC production was found to be less temperature dependent than CO2 production (in soil warming studies). Table 1 : Mineral soil bulk density of 2.8 g/cm3 is hardly possible as rock density is generally assumed to be 2.65 g/cm3

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