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Interactive comment on "Plant functional types, nutrients and hydrology drive carbon cycling along a transect in an anthropogenically altered Canadian peatland complex" *by* Sina Berger et al.

Anonymous Referee #2

Received and published: 4 August 2017

The manuscript by Berger and co-authors is very interesting because the authors provide a comprehensive dataset on how soil carbon cycling changes along a transect of four study sites (from undisturbed to disturbed conditions) in a peatland complex in Ontario from April 2014 until September 2015. They used a variety of methods that complement each other in space and time (e.g. chamber flux measurements of CO2 and CH4, DIC and CH4 concentration measurements at different soil depths, stable isotope measurements of CO2 and CH4, FTIR analysis of organic matter and porewater and measurements of ancillary variables such as air and water temperature, photosynthetically active radiation and water table depth below surface).

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The authors raise the major question, how peatland carbon fluxes respond to anthropogenically changed hydrological conditions and long-term nutrient-infiltration effects. Their major answer is that plant functional type may be a key variable to predict how soil carbon cycling in peatlands will respond to future nutrient inputs and changes in hydrology. Shrub dominated disturbed peatlands may turn into carbon sources, while graminoid-moss dominated peatlands "may maintain the peatland's carbon storage function".

However, I have few major concerns but after a thorough revision and/or modification of the manuscript it would be great to see this manuscript published in the Biogeoscience journal.

Major comments:

The authors point out that it is not new that plant functional types may have a strong influence on ecosystem soil carbon dynamics but I completely agree with the authors that "there is a gap of knowledge in terms of interactions between peat and plants under IN-SITU CONDITIONS". This makes this manuscript very valuable. However, I am not an author of the paper "Peatland vascular plant functional types affect methane dynamics by altering microbial community structure. (Robroek et al. 2015, doi: 10.1111/1365-2745.12413)" but the authors of this manuscript should cite that paper and compare both results. Robroek et al. (2015) nicely demonstrate that resilience of peatland CH4 dynamics, and therefore also CO2 dynamics, to climate change may depend on interaction between microbes and plant functional types.

I think the manuscript would greatly benefit from a more thorough discussion about the potential role of methanogens driving soil methane dynamics at the four different sites. In the current study, the authors measured stable carbon isotope ratios of CH4 and CO2 comprehensively. Hence, apparent fractionation factors could be easily measured (Angel et al. 2011; doi:10.1371/journal.pone.0020453 or McCalley et al. 2014; doi:10.1038/nature13798), the different pathways of methanogen-

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esis identified and discussed. Now, the authors attribute the change of isotopic signals to changes in methane oxidation. This is very speculative and not sufficient. The change in 13CH4 may result from a shift from hydrogenotrophic to aceitclastic methanogenesis, especially during drier months (see Hodgkins et al. 2014; www.pnas.org/cgi/doi/10.1073/pnas.1314641111 2014 or McCalley et al. 2014; ; doi:10.1038/nature13798). However, this should be discussed in the manuscript.

The authors state that it is clearly visible that ratios of C/N, C/Mg and C/K in peat soil are decreasing from site 1 to site 4. I do not see this pattern when I look at Table 1. C/K is higher at site 2 and 3 than at site 1. C/Mg is lowest at site 1. I guess C/N ratios do not differ between site 1, 2 and 3. Furthermore, I guess there are no significant differences in C/P ratios between the different sites. N/P ratios are higher at site 2 than site 3 and C/Ca ratios are lowest at site 2. Please, check your data.

However, it would be great to have a look at the submitted publication or if the authors would incorporate more convincing information. Otherwise the authors cannot state that “it becomes evident that the peatland was exposed to nutrient infiltration form the water reservoir and thus elevated nutrient concentrations occurred in vicinity to the water reservoir.” (P13, L2-L6) and should reformulate the whole discussion section!

Specific comments:

Titel: Currently, I do not see that nutrients drive carbon dynamics at your sites.

Abstract: If you mention the other methods in the abstract, you should mention FTIR analysis as well.

P1, L17-L19: All the sites are characterized by wet conditions. These are peat soils.

P1, L19-L20: Low 13CH4 may be caused by more hydrogenotrophically produced CH4.

P1, L24: or more aceticlastically produced CH4. More labile organic matter may favor aceticlastic methanogenesis.

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P3, L8: I do not see a gradient in nutrient availability.

P3, L10: Please, calculate apparent fractionation factors for methanogenic pathways.

P3, L16: I do not see that nutrient inputs are greatest in peatland periphery (see Table 1).

P3, L 18: Why should CH4 emissions be greatest at the graminoid dominated sites? There is no link to this hypothesis in the introduction.

P3, L19-21: You should also discuss CH4 production pathways.

P4, L22-28: This paragraph is very essential for the main message of the manuscript. Unfortunately, the data do not support these statements. It would be great to see more data that support these ideas.

P5, L8: Please, write out FTIR analysis.

P5, L8-L10: I am not familiar with FTIR analysis. "For pore-water samples 2 mg of oven-dried organic matter. . ." is that correct?

RESULTS section: The presentation of the results are too cluttered. Results that are not significant are described quite often (see P10, L7, L10) and sometimes it is not clear if results are significant or not (see P10, L16-17, L19-26). It would be better to mark significant differences in the figures and to highlight significant results or only few non-significant results in text, if they really enrich and/or support the guiding questions in the manuscript.

P13, L15-L16: This is repetition of results.

P13, L19-L21: This is very speculative. Did you check FTIR ratios of inflowing water? May be you can provide some references.

P13, L22-L23: I am not convinced by this statement. The difference between site 4 and the three other sites may be simply by chance.

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P13, L24-L27: So, it is not the vicinity to the reservoir but the vegetation that drives carbon cycling processes?

P14, L11-L19: This is repetition of results.

P14, L25: It would be great to see the data.

P15, L1-L2: Site 4 shows second highest CH4 release. Then you cannot state that graminoid sites show highest CH4 emissions. I would emphasize to reformulate the introduction and the hypothesis in such a manner that it becomes clearer to the reader why you have stated your hypothesis. Now, the discussions seems to be much too blurred.

P15, L6: What means “healthy” Sphagnum moss community?

P15, L22 – P16, L33: see major comments.

P17, L2 – L19: see major comments.

Figures: It would be great to have figures with higher resolution. In Figure 6, I can hardly identify the difference between the circles.

Table1: Please, mark significant differences.

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