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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.

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Dear Reviewer,

Thank you very much for your thoughtful review of our manuscript. We very much appreciate your time your time and your valuable ideas shared to improve our manuscript. Please find our answers to your comments below. As some of your concerns were also raised by reviewer 1, we kindly ask you to read our response to reviewer 1 too.

* 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 CO_2 and CH_4, DIC and CH_4 concentration measurements at different soil depths, stable isotope measurements of CO_2 and CH_4, 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). 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 CH_4 dynamics, and therefore also CO_2 dynamics, to climate change may depend on interaction between microbes and plant functional types.

– Okay. Will be done.

* 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 methanogenesis 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.

– We understand yours and reviewer 1's concern. Of course, the dominant CH4 production and transport pathway cause differences in isotopic signatures. It is not that we missed those pathways and obtaining fractionation factors but we decided to exclude it from our manuscript because we think distinguishing methane production pathways with our data would be critical. Please see our response to Reviewer 1 for a thorough explanation of this point.

* 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.

– You are right. This table was supposed to be a short summary of the previous study, which is now published in Soil Biology & Biochemistry (114 (2017) 131-144 http://dx.doi.org/10.1016/j.soilbio.2017.07.011). We agree that this table, providing only this little information is not convincing, as the results from the SBB paper are quite complex. We are going to remove the stochiometric ratios from table 1 and to provide a better description of the most important results from the SBB paper, providing more convincing evidence that those sites in closer vicinity to the reservoir are more affected.

* 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!

– We would be pleased if you were to have a look at the paper (SBB 114 (2017) 131-144). Here, the observed differences in the transect are elaborated. Due to the complexity of the dataset only a short summary can be provided in the present manuscript. We will revise our text to improve clarity and refer to the now published study. We apologize that the manuscript had not been available at the time of submitting this paper.

* Specific comments:

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

– We came up with a new title: "Differential response of carbon cycling in a continental Canadian peatland to long-term nutrient input and altered hydrological conditions"

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

– Will be done.

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

– What we were trying to say was that the site 3 hollows experienced higher water levels. We will make our point clearer.

* 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.

– Yes, we agree, but given the unsaturated conditions and strong water table drop downs during the summer and as explained further above, we think that it would be critical to distinguish methane production pathways; an influence of methanotrophic conditions is much more likely. Indeed, we think that currently not many studies have presented results from around the unsaturated zone and thus most existing studies focused on discussion pathways at greater, saturated depths.

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

– That is probably because the provided data here was in its current presentation not convincing. The table presents only a small fraction of the data provided in the SBB paper, however, the line of argument of the SBB paper is based on a greater data set (peat ages and accumulation rates, depth profiles of element concentrations, stochiometric ratios of surface peat, δ15N-values and C/N ratios of the vegetation as well as composition of the vegetation) in order to properly describe the impact of the water reservoir on the study area and its sites. When considering only single factors (e.g. only stochiometric ratios of surface peat), their explanatory power decreases; it is the entirety of factors which shapes our knowledge of the study area. We are sorry for taking the stochiometric ratios out of context. We are going to remove the stochiometric ratios from the table and we will provide a more convincing description of the study sites in terms of nutrient availability in the methods section while referring the reader to the SBB manuscript. After removing the stochiometric ratios from table 1 we will add more information on the vegetational gradient so that differences between sites become more obvious.

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

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– Please see our answer above. We have calculated such fractionation factors for a previous version of the manuscript, but eventually removed the figure and the related explanations and story because we were very skeptical of the results. Please let us know if you agree with our decision. We will seriously consider yours and reviewer 1's suggestions.

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

– The nutrient data from table 1 is going to be removed. Instead the sites will be described better and the reader is going to be referred to the SBB paper which has more convincing evidence of greatest nutrient inputs to the peatland periphery.

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

– A better explanation will be provided and this point will be introduced in the new hypothesis 2.

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

– As we removed the fractionation factors and everything related to the different CH4 production pathways from the previous manuscript version, we also removed the related explanations from the introduction. But since you and the other reviewer came up with concerns, we agree that we should add a paragraph to our discussion which explains about the different pathways, which also explains why we are not distinguishing CH4 production pathways with our data.

* 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.

– We agree, the table and the statements are not convincing here. The reader will be referred to the SBB paper and a more convincing description of the sites will be added.

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* P5, L8: Please, write out FTIR analysis.

– Will be done.

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

– Yes, it is correct, but maybe the sentence is a bit misleading. The pore water samples were dried in an oven until all the water was gone. Then the remaining solid material was scratched off the bottom of the sample containers and underwent FTIR analysis. We will provide a better description.

* 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.

– We will try to compromise between you and reviewer 1 in this respect. We will improve the presentation of the results, focus on significant results and provide less space for results that were not significant, but we will also try to include results from the sites 1 and 2 as reference sites into our discussion.

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

– The sentence will be rewritten.

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

– We agree. Unfortunately, we did not sample the inflowing water. Reviewer 1 was also concerned about this statement and provided an alternative explanation (higher productivity of the graminoid vegetation → higher input of labile compounds), which we

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will include into the discussion. By doing so we can remove the present statement.

* 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.

– We understand your concern. You are right, the observed effects can have different causes. However, looking at the entire transect the peat quality found at the sites suggests a quite similar history before dam construction at the site. So, based on our analysis the factor we can identify is the enrichment in nutrients and the concomitantly altered vegetation. Of course, site 4 thus had longest exposure to more minerotrophic conditions from intrusion of lake water. However, we would also consider this effect as anthropogenic then. Our results are derived from an in-situ study; of course, experimental set-ups under controlled (laboratory) conditions provide more explicit results and such results can be more reliably related to certain factors. As compared to such studies, in-situ studies have short-comings indeed; however, in-situ studies are needed to verify concepts based on such controlled conditions and we think that we have taken all necessary pre-cautions to avoid misinterpretations and to not over-interpret our data. As mentioned above, when revising our manuscript, we will provide more convincing information in terms of differences between our sites, so that you will hopefully agree with us in terms of the significance of results derived from those sites.

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

– We are not sure if we can provide a final answer to that question with our data. We think we are dealing with a complex interplay between vegetation, microorganisms and location factors. Site 4 and site 3 appeared to have received a similarly high amount of nutrients (well, site 4 probably received a bit more); around site 4 a dense Myrica belt established while at site 3 graminoids established. (The SBB paper provides information on vegetation etc.) With Myrica being present at the site, site 4 developed in a different way than site 3, where graminoids are established. So, we think it is both, the vicinity to the reservoir and the vegetation community that drives carbon cycling. By our study we thus also want to support that the response of a peatland to nutrient input and altered hydrological conditions may not be as simple as identified in studies with controlled variation of individual factors.

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

– The paragraph will be rewritten.

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

– Please see the SBB paper.

* 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.

– Only the CH4 emission from site 3 is significantly increased; the CH4 emissions from the sites 1, 2 and 4 are not significantly different. But we agree, the sentence can be misleading and will be rewritten. Anyway, hypotheses 2 from the introduction will be rewritten as well as other parts of the introduction, taking into account your suggestions.

* P15, L6: What means "healthy" Sphagnum moss community?

– The mosses at site 4 showed severe signs of desiccation and thus inactivity in 2014 and 2015 during the summer months (June $\sim$ September) and recovered afterwards. (Reduced photosynthetic activity of Sphagnum mosses while facing severe drought was previously observed in several studies (e.g. Alm et al., 1999, Aurela et al., 2007)). Given the pitiful appearance of Sphagnum mosses during the summers at site 4, and given also that Sphagnum covers only 60 % of the site 4 area, we concluded that Sphagnum was in retreat at site 4. In contrast, at the sites 1, 2 and 3 the Sphagnum mosses looked green or red (whatever their natural color was) and always moist, which we then termed a "healthy" appearance of Sphagnum mosses. We apologize for utilizing the colloquial expression "healthy". We will work in a more appropriate description of the vegetation of our study sites into our manuscript while revising it, to avoid any misunderstandings.

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

* P17, L2 – L19: see major comments.

– Please see our answers above.

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

– Figure 6 will be provided in a higher resolution.

* Table1: Please, mark significant differences.

– Stochiometric ratios from table 1 will be removed as explained above.

References:

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