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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 #1

Received and published: 3 July 2017

Reviewer comments on manuscript bg-2016-238 by Berger et al.

This manuscripts reports an interesting dataset on C cycling at a temperate peatland, affected by increased nutrient input from a nearby reservoir. Carbon dioxide and methane fluxes were measured over a period of 1.5 years from four sites representing variable wetness, vegetation type and distance from the reservoir, and the flux measurements were accompanied by detailed soil profile measurements of CH4 and DIC concentration. Carbon stable isotopes were used in order to gain more information about CH4 production, oxidation and transport.

The paper is well written and logically structured, and the appearance of the figures

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is very good. The methods are described in great detail, and the authors are clearly experts in selection and implementation of their field and analytical methods. The value of this work is in the high quality and completeness of the data set. I still belief that these data could be used more effectively, and the overall relevance of the paper could be greatly improved, by taking the following comments into consideration.

***Major comments

1. The match between the content of the manuscript and the title is not ideal at the moment. The title and especially the starting sentence of the abstract make one expect a comparison of carbon cycling between anthropogenically altered vs. natural sites. If this is the focus, it would be important to describe the transect better in the abstract and also in the methods section (page 4, lines 2-3 & from lines 16 onwards): How much did the hydrological condition change along the transect, and was the human impact related to drying or wetting or to fluctuating water table? And, in the abstract, how much did the nutrient infiltration change along the transect (data in Table 1)?

Further, instead of reporting just the results from the two highly affected sites 3&4 in the abstract, you should compare the results between anthropogenically altered vs. natural sites. This would justify the last sentence which claims clear anthropogenic effects on C cycling.

This comparison between affected and unaffected sites should be the view-point throughout the MS. For example: âĂć By rearranging Fig. 3 & 4 so that instead of showing various parameters from the same site in the same figure you would show a single parameter from all of the sites in the same figure. âĂć By adding here some indication of the reservoir effect: Distance of the reservoir in the table itself, or descriptive sentence in the table caption. âĂć By focus the introduction better from general description of factors affecting in peatland C cycling towards a description of the effects of anthropogenic activities on it. It should be stated clearly, citing the relevant literature, why is it important to understand effects of increased nutrient inputs and changed

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water level on carbon cycling. This topic it touched on the paragraph starting on page 2, line 24, but also there anthropogenic effects are not sufficiently discussed to match with the title of the paper. Also, the motivation statement on page 3, lines 4-8 is very general. Could you develop a more specific research question that suits for this particular study? âĂć By rewriting the Concluding remarks section to answer the questions posed by the title and the introduction section.

As you state in the discussion section (page 14, lines 13-15), it is hard if not impossible to separate the wetness effects from the nutrient infiltration effects. Thus, to draw any conclusions about anthropogenic effects on C cycling, it should be carefully considered how the data presentation is organized to serve that purpose.

2. The MS includes interesting isotopic data of the CH4 emission and porewater DIC and CH4. A better explanation of how the stable isotopic data can be interpreted would be very much needed already in the introduction section. In the discussion section (page 15, line 24) you mention that the isotopic signature in methane is affected by methane production, oxidation and transport but you do not explain anywhere why and how the isotopic composition is affected by these processes. Furhter, the discussion of isotopic data is related to methane oxidation. Could the dominant methane production pathway (acetoclastic, hydrogenotrophic) or transport pathway have caused differences in isotopic sigantures and how? At the moment, the discussion on isotopic signatures is related to methane oxidation only.

3. In many occasions, you refer often to your own, yet unpublished work (Berger et al., submitted). Since that work seems to contain information quite crucial for the present paper, it is somewhat problematic that the paper is not available for the reader. If the submitted paper has not been published meanwhile, you should consider elaborate those results in more detail when necessary, e.g., in the methods section, page 4, line 2-3 about the hydrological changes caused by the reservoirs and page 4, line 23.

***Minor comments

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page 1, lines 18-19 & page 11, lines 19-21: The study period includes a full year of measurements. It would be good to give values also on cumulative annual fluxes. This was enable using this carefully collected data in flux syntheses, and facilitate comparison with literature values.

page 2, lines 3-5: One-sentence paragraphs should be avoided. I suggest combining this sentence with the next paragraph. See also page 4, line 29; and page 17, lines 16-19.

page 2, lines 26-28: Reference missing.

page 3, lines 12-15 & page 4, line 2: Also here, I would like to see a mentioning about how the hydrology is altered – drying or wetting, or more variable in the course of the year?

page 4, line 13: microtopography is a single word

page 4, lines 18-19: Listing the sites starting from number 4 is counterintuitive. Would it be possible to change the order in which you mention the sites, or simply change the numbering? You indicate that site 2 was further away from the reservoir than site 3, but it would be better to describe the whole transect, e.g., that the distance from reservoir decreases with growing number.

page 4, lines 25-28: It is not clear if and how this is related to the vicinity of reservoir?

page 5, line 8: At the first appearance, write the complete instrument type instead of the abbreviation FTIR.

page 5, line 18: Regarding UV-VIS, see the previous comment.

page 6, line 12: Was the image analysis based on satellite/aerial or other imagery? Please specify!

page 8, lines 5-6: Have you tested if there were any discrimination against the lighter isotope during diffusion into the silicon collectors?

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page 8, lines 13: Please check the sentence (some words missing/in a wrong order).

page 9, lines 29-31: Please check the sentence (some words missing/in a wrong order).

page 11, lines 22-24: These are important results for this paper. But, can you really say that it is anthropogenic effect, or just a consequence of different location (edge effect, more mineral site?). It's interesting that the site receiving more nutrients is showing lower CO2 uptake.

page 13, lines 19-21: Or, is the higher lability of organic matter caused by higher productivity and high input of labile compounds from vegetation? This site showed the highest C accumulation (page 11, lines 19-21). If it is a reservoir effect, should not the organic matter at site 4 be even more labile? Now, the site 4 was showing the highest proportion of aromatic compounds.

page 13, lines 19-21: Please add references: In recent studies by Bragazza et al. . . .

page 14, line 21: Decrease in the CO2-sink strength in response to what?

page 16, lines 3-4: You write about "deepening of soil oxygenation probably promoting a highly active methanotrophic bacteria community, which drew CH4 from the atmosphere down to that depth". Why do you think it was atmospheric and not peat-derived CH4 that was oxidized at 15 cm? Atmospheric methane cannot diffuse to the soil against the concentration gradient (when the pore-water concentrations are above ambient).

page 16, line 5: Why enriched signals would mean low CH4 production? Do you mean more CH4 production via the acetoclastic pathway that results in heavier methane than the hydrogenotrophic pathway?

page 16, lines 20-21: Besides transporting CH4 through the aerobic peat layers without exposing it to oxygen plant-mediated transport also strongly discriminates against the heavier methane (Chanton, 2005). Because of this, plant transport can create even

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lighter methane that is present anywhere in the peatland. It would be important to mention this in the discussion about isotopic signatures.

page 16, lines 23-28: Yes, probably most of the methane is oxidized during the diffusion, and thus, the amount of methane reaching the atmosphere by diffusion is low. So even with low coverage of aerenchymous plants, most of the methane that is actually entering the atmosphere is emitted through them.

page 16, lines 28-30: It seems to me that you have done all the necessary pre-cautions to avoid methodological biases in the data. Please specify, what actually makes you suspect some methodological problems particularly in low water table conditions.

page 17, line 9: Instead of just saying results, it would be better to specify which particular parameter you mean here.

page 17, lines 16-19: Long and complicated sentence, please consider splitting it into two sentences.

Fig. 1: For better clarity, please mark the Luther lake reservoir in the figure.

Fig 6 (and page 12, lines 28-29). In this figure, you have decided to use the porewater d13C-CH4 at 5 cm. However, the methane pool at this depth does not necessarily represent very well the origin of the methane emissions, since the ebullitive and plant-mediated fluxes are originating from deeper layers. Hornibrook (2009) was using the average from 0 to 50 cm. It would be interesting to see how the figure looks if the porewater methane at depth is not excluded. Although the differences between different depths were not significant, Fig. 5 shows that especially at sites 1 and 2, the porewater CH4 has different isotopic composition at 5 cm than at deeper depths.

Fig 6. caption, line 5: A typo in the word 'circles'. Table 1: The differences in stoichiometry are not evident, and I do not see a clear transect here. Could you test the differences statistically and mark it in the table? Or, would the amounts instead of ratios reveal the pattern more clearly?

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