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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,

we very much appreciate your thoughtful review of our manuscript. Thank you for your time and your valuable ideas. We absolutely agree that taking up your suggestions greatly improves our manuscript and we will do so as outlined. Please find our answers to your comments below.

* This manuscript reports an interesting dataset on C cycling at a temperate peatland, affected by increased nutrient input from a nearby reservoir. Carbon dioxide and

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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 C1 BGD Interactive comment Printer-friendly version Discussion paper 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)?

– We understand your concern. This present study is following up on a study, which was very recently published in SBB (114 (2017) 131-144; http://dx.doi.org/10.1016/j.soilbio.2017.07.011). While writing the current paper, this other study had not been published so we could not well refer to it. The study site was described in detail in this SBB-paper. As elaborated there, the water reservoir affects the entire northern tip of our peatland site. We find in our data that areas further away from the reservoir are likely less affected, but it is not entirely possible to distinguish

‘natural’ from ‘anthropogenically altered’ sites among the sites 1-4 (800-200 meters distance from the reservoir). Only areas further away from the reservoir could likely be regarded as pristine. So, our transect of study sites as presented in this present study rather assesses strongly altered sites vs. less altered sites with respect to the investigated features. Unfortunately, this entire gradient was not as obvious in the beginning of the study, so this part is indeed missing. So, our objective for this paper was to investigate carbon cycling along this transect and to identify effects of altered conditions along the transect on carbon cycling and fluxes. We agree: the transect should be described better. In that sense, the abstract and the method section will be extended, providing the information you requested.

As far as the title is concerned, another option would be: “Differential response of carbon cycling in a continental Canadian peatland to long-term nutrient input and altered hydrological conditions”

* 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.

– We agree. So far, we were only pointing out the results from the sites 3 and 4. The reason is that here we found most statistically significant differences. At the same time, the sites 3 and 4 were the most altered ones. So, we assumed that these significantly different results for distinctly altered sites would be most convincing, and focused our discussion on the sites 3 and 4. The last sentence of the abstract is indeed somehow misleading, we agree. We will consider including results from site 1 and 2. As reviewer 2 mentioned that we should reduce the presentation of non-significant differences, we will try to balance these two issues.

* This comparison between affected and unaffected sites should be the view-point throughout the MS. For example: âAËŸ c By rearranging Fig. 3 & 4 so that instead of

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′ 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. âAËŸ c By adding here some ′ indication of the reservoir effect: Distance of the reservoir in the table itself, or descriptive sentence in the table caption. âAËŸ c 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 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? âAËŸ c By rewriting the Concluding remarks section to answer the questions ′ posed by the title and the introduction section.

– Unfortunately, the manuscript cannot provide a comparison between “affected” and “unaffected” sites (see also above). However, we can provide a comparison of carbon cycling of strongly altered and less altered sites. We will clarify this point to avoid misunderstandings here.

We re-arranged figures 3 & 4 as suggested (please see the following two figures below). We agree that this very much improves clarity. The development of pore gas DIC and CH4 concentrations over time could, however, not be re-arranged as the 3D-plots would get too complicated.

Having re-arranged the figures as you suggested, differences between the sites in terms of CO2 and CH4 fluxes become indeed much more obvious.

The stochiometric ratios from table 1 will be removed. Table 1 in its previous form was thought to summarize some results from the SBB-paper, but it turns out that presenting only that little information about the sites is quite misleading. Instead a better description of the study sites will be added to the methods section and the reader will be

referred to the SBB-paper. However, more information on plant species abundances will be added to table 1 in a way that the vegetation gradient becomes more visible.

The introduction will be revised according to your suggestion. The paragraph (starting on page 2, line 24) dealing with factors affecting peatland C cycling will be extended towards a description of the effects of anthropogenic activities. In this regard, we will better elaborate the results from long-term peatland fertilization experiments (Mer Bleue, Whim Bog, Degerö Stormyr) as well as impacts of inundation on neighboring peatland ecosystems (e.g. Kim et al., 2015; Ballantyne et al., 2014) and we will also provide a more adequate summary of what is known about gaseous carbon fluxes in relation to an altered plant community (e.g. Robroek et al., 2015). Taking also into account our own recently published paper (Berger et al., 2017) it should then be much more obvious why there still is a need to study changes in peatland C-cycling after increased nutrient inputs and changed water levels.

As far as a more specific research question is concerned, we developed new hypotheses which better meet the focus of our study:

1) peripheral nutrient input accelerates C cycling at the affected sites, reflected in more decomposed peat, 2) increased abundance of vascular plants can increase CO2 uptake but also change patterns of CH4 production and emission, in particular if graminoids dominate, 3) long-term nutrient enrichment in combination with hydrologically altered conditions may cause differential responses of carbon cycling and does not necessarily cause a loss of the C-sink function of peatland ecosystems.

Including these changes into the abstract, introduction, site description, presentation of results, and discussion, of course, the concluding remarks section will be adjusted accordingly.

* 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.

– We agree. We believe that re-arranging the figures 3 and 4 really helped in this regard.

* 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.

– A better explanation will be added. This will also cover the interpretation regarding underlying pathways and methanotrophic activity.

* 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.

– We understand your concern. Of course, the dominant CH4 production and transport pathway cause differences in isotopic signatures. This will be better explained in the revised version in the introduction. You might be surprised by reading that a previous version of our manuscript contained also an interpretation to differentiate dominant methane production pathways. According to Whiticar et al. (1986) the isotope fractionation factor αC was calculated:

αC = (δ13C-CO2 + 1000) / (δ13C-CH4 + 1000).

Accordingly, Fig. 3 had been part of our manuscript, but was removed later.

We removed the figure and everything related to distinguishing methane production pathways for the following reasons:

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1) Whiticar et al. (1986) stated that αC values between 1.04 and 1.055 indicate the prevalence of the acetate fermentation pathway, whereas αC values higher than 1.065 support a shift towards the CO2 reduction pathway. On the other hand, values of αC typically observed for the acetoclastic pathway could also arise from methanotrophic activity, yielding an enrichment in 13C of the residual CH4. Values of αC measured in our study site covered a broad range from 1.013 to 1.082. This is a wide range compared to other studies which found values between 1.028 and 1.061 (Hornibrook et al., 2000), 1.03 and 1.07 (Kotsyurbenko et al., 2004), 1.046 and 1.075 (Steinmann et al., 2008) or 1.022 and 1.053 (Hornibrook et al., 1997). As Fig. 3 shows, the average water table in many cases dropped below our second sampling depth (-15 cm) and sometimes even approached to -25 cm. The fact that αC values were lowest when water levels were lowest during the summer 2015, i.e. the clear relation of αC to water table levels, suggests that not only a change in methane production pathway but even more so methane oxidation affected our αC values. We think that existing studies often did not cover this range of partly unsaturated conditions at sampling depths and thus less data is so far available from samples clearly affected by methanotrophic activity. Moreover, a discussion of values of αC is from our point of view too often limited to a discussion of pathways although at the water table under partly oxic conditions methanotrophy may be much more likely to affect the isotopic signature of methane.

2) We think that distinguishing methane production pathways would thus only be possible when considering only αC values from below the water table levels, i.e. from the saturated zone. However, as water table depth may even be different within replicates of gas sampling spots within one site and the water table does not necessarily coincide with the transition from oxic to anoxic conditions (roots!) obtaining αC values from saturated, anoxic layers only was difficult. So, we decided not to present the fractionation factors in such detail and therewith we excluded the story on different CH4 production pathways.

To summarize: we think distinguishing methane production pathways with our data

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would be critical. That is why we would prefer to refrain from it. However, since reviewer 2 also raised that point, we will add a paragraph, which discusses methane production pathways and how isotopic composition is affected by these processes and which also explains, why we think that our data is not suitable for distinguishing methane production pathways. We hope that the reviewer could agree with our position here following the given explanation above; in case not, we would consider adding more discussion of pathways, but we would first like to clarify and support why we prefer to primarily discuss the impact of methanotrophy.

* 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.

– The paper is published now in Soil Biology & Biochemistry 114 (2017) 131-144 http://dx.doi.org/10.1016/j.soilbio.2017.07.011 We apologize that this information could not be presented earlier.

***Minor comments

* 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.

– Will be done.

* 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.

– Will be done.

* page 2, lines 26-28: Reference missing.

– Will be added.

* 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?

– It is a bit tricky to do so in a generalized way. As explained in the SBB-paper, we lack particular historic data for a detailed overview of the impact of the reservoir flooding on the adjacent peatland. We only know for sure that flooding of the reservoir in 1954 rewetted large areas of the peatland, which had been subject to drainage during previous decades. So, the reservoir had a clear wetting impact on the peatland. Through measurements with an eddy covariance station (results will be part of another, future manuscript) we observed that during dry and hot days the water loss to the footprint area through evapotranspiration was much smaller than the soil water level dropdown measured with our water level sensors (even more so when reservoir water was released through the dam). On the other hand, when there was heavy rainfall, soil water levels rose more than the amount of precipitation measured in our peatland would have suggested. That clearly indicated that peatland and reservoir water levels were strongly connected in a way that on hot, dry days, the reservoir drew water out of the peatland, while on rainy days the reservoir pushed water into the peatland and the amount of water moved seemed to be related to the amount of rainfall and to the severeness of summer heat. However, such fluxes of water cannot be adequately analyzed without a sound modelling approach. Nevertheless, it became obvious that the reservoir seems to control to a large extent the observed peatland water level fluctuation. Probably, the water levels of the entire northern tip of the peatland are more variable than it would be expected under natural conditions. Unfortunately, from our own water level data (May to September 2012 and November 2013 to September 2015) it appeared difficult

evaluating how the reservoir water level is affecting the peatland water levels in more detail. Based on our available data and the work published in the earlier manuscript about the site, we have strong support that those sites in closer vicinity to the reservoir are more affected. The focus of this present manuscript are the peatland hollows. When only looking at the hollows, site 4 and site 3 were indeed wetter than the sites 2 and 1, but when also considering hummocks (as done in the SBB manuscript), site 4 and site 1 were drier than the sites 2 and 3. We will add more information to the introduction and to the description of the study site in terms of how the peatland water levels are affected by the reservoir.

* page 4, line 13: microtopography is a single word

– Will be corrected.

* 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.

– We are sorry, but we don't think that would be possible. As the site names were established in the SBB paper, changing the names now would be a bit confusing. . .

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

– You are right. Sorry for the confusion. We were thinking it could be helpful to provide that additional information as through FTIR and UV-vis the manuscript includes data on quality of peat and pore water. However, presenting that information in that context might be misleading. We will probably remove the two sentences here, and add them to the more general description of the site a bit further above.

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

– Will be done.

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

– Will be done.

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

– It was done via aerial imagery with images obtained from UAV flights. This information will be added.

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

– This is indeed a good point. We tested this some years ago and found no differences in the isotopic composition for CH4 and CO2 (at low pH and predominance of H2CO3/CO2). This is however, not published as meanwhile also other studies have used this technique and we thus assumed that it is accepted. The concentrations in silicon tubes adjust at time scales of hours (tested by us for CO2, CH4, H2, and N2O) as described in Kamman et al. (2001). So isotopic discrimination when sampling monthly should be smaller than the analytical error of our measurements. The samplers were for example previously used in Goldberg et al. (2008, 2010), Knorr et al. (2008), Zou et al. (2011), Berger et al. (2013), Novak et al. (2015).

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

– Will be done. “Silicon tubes for isotope sampling had a volume of 20 ml in 5 cm depth and 5 ml in the other depths. Bigger sampler sizes in the close to surface peat layer were necessary as a sample volume of at least 20 ml at sufficiently high concentration ($2.5 < x < 2000$ ppm) was needed for the isotope analysis.”

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

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– Will be done.

* 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.

– We agree that the observed effects can have different causes. However, looking at the entire transect the peat quality found at the sites suggest 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. We will provide a statement in this regard in our discussion.

* 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.

– That is a very interesting question! You are right, it could very well be the case that the higher lability of organic matter is caused by the vegetation (higher productivity → higher input of labile compounds). That idea will be included into the discussion. We first came to our conclusions as DOM is usually a small but easily accessible pool and should thus reflect a residual enrichment of refractory compounds. Presence of labile DOM we thus attributed to external input. Indeed, this would then also apply to site 4. This discussion was not straightforward and will be modified considering your suggestion.

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

– Will be added.

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

– In response to drought. That information will be added.

* 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).

– You are right. That is speculative. We thought it probably was atmospheric CH4, which entered the soil because the CO2 and CH4 concentrations in the upper peat layers were particularly low on that day (as indicated in Fig. 3). The water table even was below -15 cm depth (please see Fig. 3). Also, the surface peat looked quite dry during that summer period and the Sphagnum mosses were white and inactive (later in the year they recovered). So, our idea was that the CH4 in 5 cm depth was of atmospheric origin. But we have indeed no proof for this interpretation – we are aware of that. Therefore, we will adjust our discussion here and will be more cautious. Another observation which however supported our idea was that we could sometimes observe a CH4 flux from the atmosphere into the soil for certain plots of our study area. Such phenomena could be observed from dawn to sunset and in some occasions, this was observed for several weeks, unfortunately, we could not yet figure out an explanation. Of course, we know that peatlands can consume CH4 from the atmosphere (there are several studies), but we were surprised to observe this for several weeks in a row for certain plots. This data is not part of this manuscript but there is going to be another manuscript dealing with CH4 flux dynamics in which this issue will probably be addressed. Thus, we will downgrade our interpretation here as suggested.

* 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?

– Actually, we were trying to avoid speculations about acetoclastic and hydrogenotrophic pathways, because values of αC typically observed for the acetoclastic pathway could also arise from methanotrophic activity. Both processes would yield an enrichment in 13C of the residual CH4. We believe that it is rather CH4 oxidation, which took place because of the low water tables and the unsaturated conditions during the summer. So, we tried to boil it down to: methane production would yield less enriched 13C - CH4, while CH4 oxidation would leave behind comparatively more enriched CH4. We then interpreted more oxidation as less net production. We will clarify this point.

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

– Okay! Will be mentioned.

* 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.

– That was our idea, too.

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

– For gas sampling for later isotope abundance analyses we used the same chambers

as used for the flux measurements. It is known that chamber measurements tend to overestimate CH4 fluxes a bit for several reasons (spatial heterogeneity, artificial pressure fluctuations induced by the chambers. . .). So, artefacts cannot be fully excluded. To counteract possible concerns in terms of data quality we verified our chamber flux data with eddy covariance flux data (Fig. 4). The sets of data are nicely comparable, however, CH4 fluxes measured with chambers were slightly increased in July and August when the water tables were lowest, which we think could have something to do with deeper CH4 pools becoming connected to the atmosphere under unsaturated conditions with dropping water tables as explained in Estop-Aragones et al. (2016). With chamber induced pressure fluctuations such CH4 pools might have been forced out of the peat. We would like to clarify that in the course of quality assurance/ quality checks while processing data, most likely all low-quality data was eliminated. Thus, we are sure that our data provided (fluxes and isotope data) is of very high quality. By mentioning about the issue in the first place we intended to point out the common shortcomings of the method; instead it probably downgraded our results, so we might remove the related sentences from the manuscript to avoid any misunderstandings.

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

– Will be done.

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

– Will be done.

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

– Will be done.

* 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

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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 pore-water 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.

– We redrew the panel (e) of figure 6 using also δ13C-CH4 -values from the deeper CH4. (please see Fig. 5)

* Fig 6. caption, line 5: A typo in the word 'circles'.

– Will be corrected.

* 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?

– Thank you for pointing it out. We decided to remove the stochiometric ratios from the table and to describe the transect in the text while referring the reader to the SBB paper. Since the results are very complex, only presenting this little information turned out not to be helpful at all.

Figure captions:

Figure 1 (new Figure 3): Development of (a) Twater [° C], (b) wtd [cm], (c) CH4 fluxes [g CH4 m-2 d-1] and (d) – (f) CO2 fluxes (NEP partitioned into Reco and GPP) [g CO2 m-2 d-1], $\pm$ 1 SD (n=6) in hollows of the sites 1–4 from April 1st, 2014 through September 22nd, 2015. Negative CO2 and CH4 fluxes indicate uptake, positive fluxes indicate a release to the atmosphere. The dashed grey line in the NEP graph indicates a 0-flux.

Figure 2 (new Figure 4): Development of mean CH4 and mean DIC concentrations [μmol L-1], in hollows of the sites 1–4 from April 1st, 2014 through September 22nd,

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2015.

Figure 3 (not to be included into the manuscript): αC values obtained from silicon samplers in 5, 15, 25 and 35 cm depth at sites 1 to 4 from June to September. squares = June (06/11), circles = July (07/08), triangles = August (08/27), diamonds = September (09/17). n = 1-3. Blue bars indicate the water table level at each site (averaged over one week before sampling). Vertical dashed lines indicate the thresholds for acetoclastic and hydrogenotrophic methanogenesis according to Whiticar et al., 1986. Values marked by the red circle probably reflect atmospheric signatures of CH4 and CO2 as the water table as well as the concentrations determined in the samplers were very low on these dates.

Figure 4 (not to be included into the manuscript): A comparison of our eddy covariance vs. chamber measurements for the NEE (left) and CH4 flux (right). The gray lines indicate the 1:1 lines. A good agreement is apparent; however, the chamber measurements detected higher CH4 emissions in July and August (circled in gray).

Figure 5 (manuscript Figure 6): dominant flux pathway of CH4 according to (Hornibrook, 2009). Empty circles = site 1, circles with diagonal lines = site 2, circles with crosses = site 3, circles with vertical lines = site 4. Dashed line represents transport via ebullition or active plant transport without any isotopic fractionation. Values are means of pore-gas samples from 5-35 cm depth and chamber flux measurements. Graphs show mean values and standard deviations from three replications at each site. n = 1-3.

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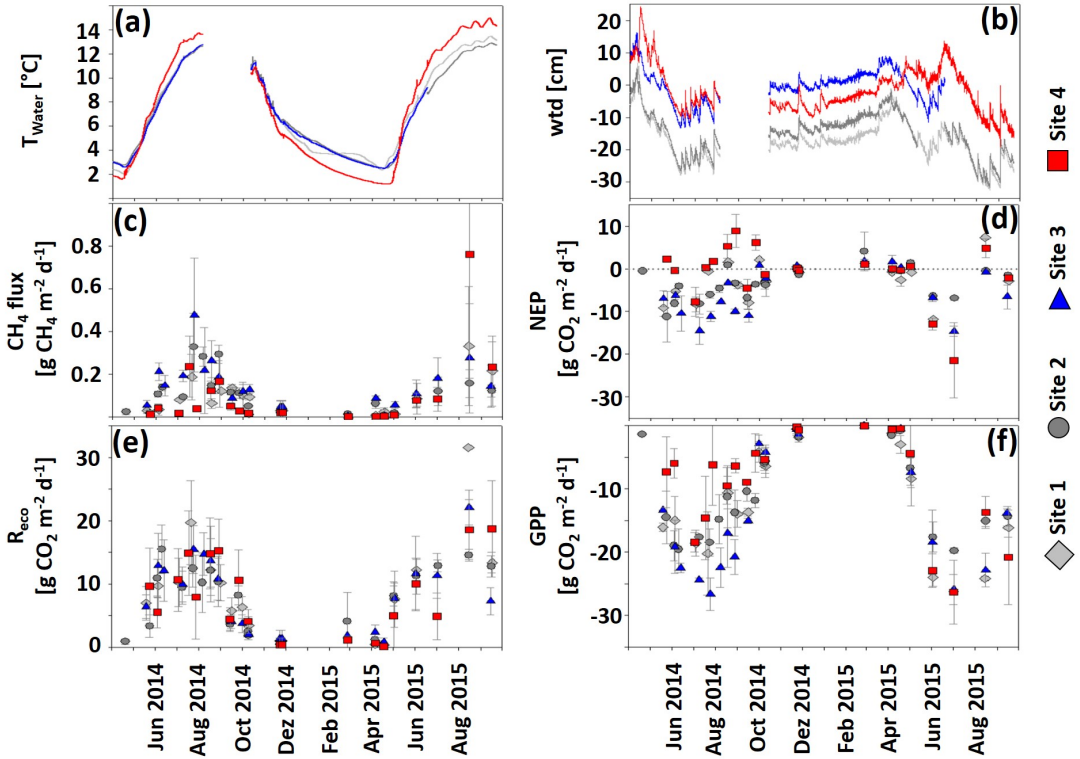

Fig. 1. (new Figure 3): Development of (a) Twater [° C], (b) wtd [cm], (c) CH4 fluxes [g CH4 m-2 d-1] and (d) – (f) CO2 fluxes (NEP partitioned into Reco and GPP) [g CO2 m-2 d-1], ± 1 SD (n=6) ...

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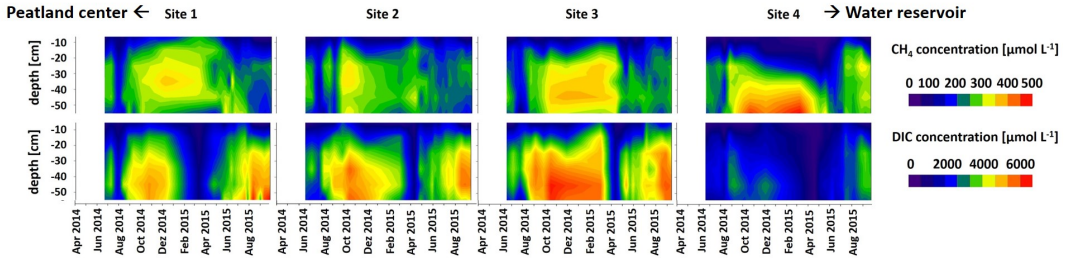

Fig. 2. (new Figure 4): Development of mean CH4 and mean DIC concentrations [μmol L-1], in hollows of the sites 1–4 from April 1st, 2014 through September 22nd, 2015.

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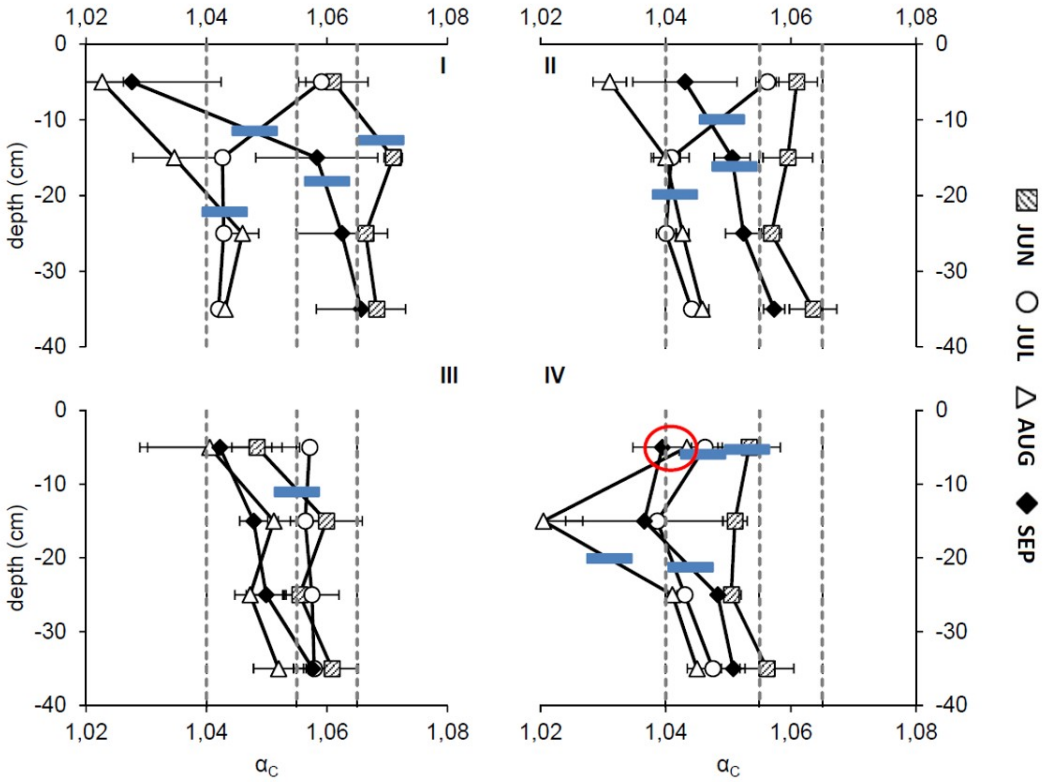

Fig. 3. (not to be included into the manuscript): αC values obtained from silicon samplers in 5, 15, 25 and 35 cm depth at sites 1 to 4 from June to September. squares = June (06/11), ...

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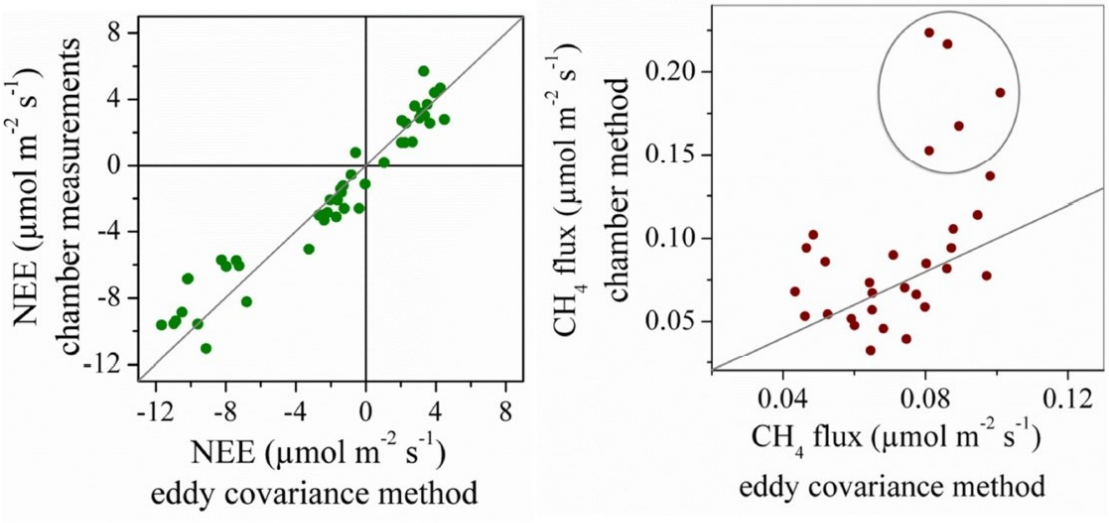

Fig. 4. (not to be included into the manuscript): A comparison of our eddy covariance vs chamber measurements for the NEE (left) and CH4 flux (right). The gray lines indicate the 1:1 lines. A good agreement..

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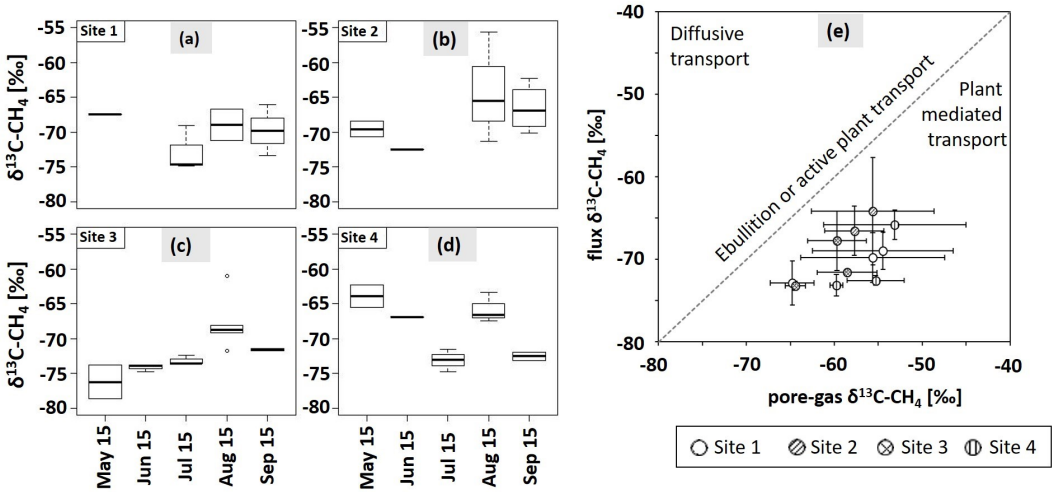

Fig. 5. (manuscript Figure 6, panel e): dominant flux pathway of CH4 according to (Hornibrook, 2009). Empty circles = site 1, circles with diagonal lines = site 2, circles with crosses = site 3, ...

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