Dear reviewer,

we appreciate your thoughtful and detailed review! Your constructive comments will help to improve our manuscript and particularly the discussion. Please find our response (blue) to your comments (black) below.

Reviewer 2:

General comments

The MS of Münchberger and co-authors contributes to the knowledge on processes of methane turnover (production+oxidation) and transport in rarely studied southern bogs. Authors combined field sampling with the advanced analytics (porewater chemistry and stable isotope analyses) to report relationships and peculiar mechanisms between microrelief forms, dominating vegetation communities and the net processes affecting the CH4 efflux from the Patagonian peatland during two consecutive summer seasons. Field-based studies are critically important for understanding processes related to functioning of ecosystems and therefore interesting for the broad scientific community. Accepting the field experiments typically operate with much larger spatial and temporal variability in measured parameters (and as the result, relatively lower statistical power as compared to controlled conditions), still there are several issues which I would like to point out for the discussion and improvement. Below authors find general comments while specific recommendations and technical corrections are incorporated directly in the draft file attached.

1. First of all, the MS is rather long and too repetitive and descriptive. Thus, the Introduction is definitely too extended, especially regarding the common knowledge about methane in the very beginning and peatlands in general. Authors could immediately start the story of the importance of southern peatlands and have the necessary information on peatlands' biochemistry and vegetation specialty in there. Then the information on the isotope issue would be sufficient to formulate hypotheses without any loss of logic.

We understand your concern and appreciate your suggestions of how the topic of the manuscript could be introduced in a more straightforward way. However, the strength of our dataset is the combination of both, a comprehensive chamber measurement campaign and advanced pore water chemistry. To address our manuscript to the readers of both communities, researcher focusing on gas exchange in peatlands as well as those mainly dealing with biogeochemical processes in the peat, we decided to shortly introduce the main mechanisms of both research areas. Southern peatlands are introduced only towards the end of the introduction since the rhizosphere effects on methane dynamics due to aerenchymatic vascular plants are of relevance for other peatlands with a dense cover of vascular plants such as rushes and sedges. We believe that starting the introduction with the importance of southern peatlands would result in a too narrowed topic of the manuscript. Therefore, and as reviewer 1 did not comment on the structure and length of the introduction, we hope that reviewer 2 could agree with our positions here.

Nevertheless, we will try to shorten descriptive as well as introducing and transitional passages in our manuscript. As we understand that parts of the discussion are too repetitive and descriptive, we will also shorten the respective paragraphs. We kindly refer to our answer to general comment 4 to 7 below for details on this aspect.

2. In the proposed hypotheses, it has to be clearer why pools are so much different from lawns in terms of methanogenesis pathways. This was not strait forward from the introduction; I suggest to omit statements as "remains less affected" because they are more confusing then explanatory; please, rephrase.

We agree that the vegetation in the pools needs more explanation in the introduction as otherwise the development of the hypothesis III is not straightforward. The hypothesis will

be rephrased accordingly. Also hypothesis II will be rephrased into a better explanatory version.

3. In the Methods section, I was confused with relatively short time (3 min) of chamber exposition even under the conditions of rather low atmospheric temperatures and low fluxes expected. Why also transparent and not opaque chambers were used for CH4 fluxes measurement?

Indeed, chamber measurements with a chamber not connected to a fast gas analyzer need up to 30 minutes or more to determine methane fluxes. The chamber used in our study was connected to a portable gas analyzers (Ultraportable Greenhouse Gas Analyzer, 915-001, Los Gatos Research) with a 1 hz sampling rate. The instrument accuracy according to the manufacturer was < 2 ppb. Therefore, this instrument provides the opportunity to determine concentration changes even at low CH4 concentrations and within a short period of time (see for example McEwin et al., 2015; Berger et al., 2018 or Mastepanov et al., 2013 who used a similar gas analyzer with 1 hz sampling rate). Test measurements with a prolonged closure time and instantaneous on-site monitoring of gas concentrations changes within the chamber proved that a short measurement time was sufficient to determine the CH4 fluxes also at our study site. From this, we could furthermore exclude that zero fluxes are a methodological artifact.

And indeed, despite the short measurement time and low fluxes, only 105 of 537 flux measurements showed a concentration increase with a slope not significantly different from zero (page 7, lines 18-19). During all other measurements (at least a small but) significant concentration change was observed already during the short measurement time.

Low or zero fluxes were even not an artifact due to a short measurement time at low temperatures. Otherwise collars with low or zero fluxes would have shown a response to temperature. We kindly refer to figure S02 in the supplement provided to the manuscript. This figure show that most individual collars did not show any response to temperature. Please compare also to the explanation on page 7, lines 22-26.

We used transparent and opaque chambers since the Los Gatos gas analyzer can measure CH4 and CO2 simultaneously. Thus, our measurement campaign was designed to determine also the NEE. As the CH4 fluxes did not differ systematically between light and dark measurements, we included also the light measurements in our data set to increase the sample size.

4. Discussion section contains repetitive and partly speculative information and therefore is currently too long. For instance, in the discussion of results on 13C-CH4 depth profile (page 14, lines 5-8) authors seemingly “oversell” their results: "scattered between" may also indicate no significant difference (this is not clear from the data). Indeed, Fig. 4d demonstrates rather narrow d13C-CH4 range along the whole depth profile. So, in fact, d13C-CH4 signal alone was not informative enough to approve the strong oxidative properties of rhizosphere of A. pumila. I agree that both methanogenesis and oxidation may co-exist in close vicinity, but still it may not explain lack of d13C-CH4 variation between upper and lower horizons unless CH4 produced in the rhizosphere region is even more depleted in 13C than in deeper layers. The explanation of this phenomenon because of "more reduced...microsites" is not fully clear. More than below the rhizosphere? Why?

We understand that parts of the discussion are too repetitive and partly speculative. The comparatively small shift of the mean d13C-CH4 signature to more enriched values within the rhizosphere was a surprising and unexpected result also to us. Indeed, a strong effect of only methanotrophy should result in a less negative signature of d13C in CH4 compared to the signature below the rhizosphere. Therefore we completely agree with the reviewer, that the d13C-CH4 signal alone does not provide a clear indication for oxidative effects in the rhizosphere. Taking into account the near-zero CH4 emissions, high DIC:CH4 ratios and a d13C-CH4 depth pattern not following the d13C-CO2 and a depleted d13C-CO2 signal, we can nevertheless only explain these results by a strong effect of methanotrophy.

In addition, we attempted to explain the lack of variation in the mean d13C-CH4 signal. Throughout the rhizosphere, the d13C-CH4 signal was associated with a wider standard variation compared to deeper peat layers. Our possible explanation for this is that the mean d13C-CH4 signal represents a mixed signal from methane production and consumption and, thus, indicates a co-existence of aerobic and anaerobic microsites. Maybe our explanations were too detailed here and thus became speculative. In fact, the word scatter was indeed used incorrectly here to describe the wider standard variation.

As the reviewer correctly points out, the occurrence of hydrogenotrophic methanogenesis is a possible explanation for surprisingly negative d13C-CH4 signatures. Reviewer 1 suggests occurrence of acetogenesis as a possible explanation. We agree that both pathways could explain the pattern in the d13C-CH4 signal, although we propose in accordance with reviewer 1 that hydrogenotrophic methanogenesis was probably relatively more important below pools while acetoclastic methanogenesis in combination with acetogenesis seemed to contribute more below Astelia lawns. But as we did not quantify other parameters such as labile organic matter from roots, acetate concentrations or its carbon isotopic signatures, we cannot clearly determine the relative importance of both pathways for our study. We kindly refer to our answer to general comment 5 below for more details.

In the revised discussion, the word "scatter will be rephrased. We will follow the helpful suggestion of the reviewer (page 14, comment 3) and incorporate aspects of the paragraph on page 14, lines 17-32 into the paragraph above (lines 7 and following). Thereby, repetitive and speculative information concerning the coexistence of microsites will be reduced. We will furthermore emphasize that the lack of d13C-CH4 variation between upper and lower horizons is an unexpected result. We agree with the reviewer that the d13C-CH4 depths pattern needs further explanation that is so far missing in the discussion. We will try to balance the suggestions of both reviewers by discussing the arguments for both, hydrogenotrophic and acetoclastic methanogenesis and acetogenesis, carefully. The sentence "more reduced... microsites" (p 14, lines 7-8) does not refer to "more reduced" compared to below the rhizosphere, but compared to oxidized microsites in close vicinity to reduced microsites. This misleading phrase will be clarified.

5. Contribution of acetoclastic pathway to methanogenesis in the rhizosphere of A. pumila was not convincingly verified (e.g. page 14, lines 23-27) and looks therefore speculative: having acetoclastic methanogenesis and co-existence of oxidation should generate much more enriched d13C-CH4 values in comparison to deep peat. Fig. 4d cannot support this. Seemingly, change of fractionation factor with depth was not significant either. The available data are not enough to approve existence of acetoclastic methanogenesis, and this has to be acknowledged.

We agree with the reviewer, that a clear effect of methanotrophy and acetoclastic methanogenesis on the d13C-CH4 signature should result in an enriched d13C-CH4 signal and a distinctly lower fractionation factor throughout the rhizosphere compared to deeper peat layers. We were thus surprised by the small (and probably not statistically significant) change in the fractionation factor. Thus, we attempt to explain a fractionation factor in the overlap range of ac from hydrogenotrophic and acetoclastic methanogenesis.

As already observed for the d13C-CH4 signal, also the fractionation factor shows a wider standard variation within the rhizosphere. The standard variation of the fractionation factor even tended to be larger with increasing depth down to the lower boundary of the rhizosphere. This pattern comes along with a presumably with depth decreasing root density (Fritz et al., 2011) in these depths. We therefore interpret this as a further indication for the occurrence of microsites as lower root density makes a more heterogeneous peat matrix more likely and thus a higher variation in the fractionation factor between sampling sites.

We agree with the specific recommendation on page 15, comment 1 that occurrence of hydrogenotrophic methanogenesis at elevated H2 levels in surface peat layers might be a possible source for depleted 13C and an explanation for only small changes and a comparatively higher standard variation in the fractionation factor. Another reasonable explanation is the occurrence of acetogenesis, as suggested by reviewer 1.

In the revised discussion, it will be underlined that only small changes in the fractionation factor between upper and lower horizons are an unexpected result if methanotrophic effects are assumed. We will better explain possible sources for depleted 13C-CH4 within the rhizosphere and discuss the possibility of the occurrence of acetogenesis. The difficulties to separate the isotopic effects arising from methanogenic pathways will be elaborated. As we did not quantify other parameters such as labile organic matter from roots, acetate concentrations or its carbon isotopic signatures, we will carefully discuss the possibility of both, hydrogenotrophic and acetoclastic methanogenesis based on the data obtained in our study.

6. Another critical point is again a speculative discussion of the results on pools and lateral flows on the site (page 15, lines 23-35). Explanations on gas diffusion along gradient were clear for me (from pools to lawns) but water movement is not the same. Pools are local depressions, so water should flow from lawns into pools. If this flow is so low, then the gas diffusion in opposite direction can be stronger, but this means almost standing water. In case there is a lateral flow of water (what is very natural), then the gas flow can't be counter to it. Therefore, I could understand the inflow of oxygen from lawns into pools, but not CH4 from pools to lawns. The overall picture may change if there is a slope, but then lawns and pools have to be arranged accordingly. Pools will get matter of those lawns which are exposed higher and transfer it downwards to other lawns. If there is a slope on the site, then the conceptual figure should somehow reflect it. Such important information was not provided in Mat&Meth or any other parts of MS.

We thank the reviewer for this careful and critical examination of our concept. We attempt to explain low CH4 concentrations in the pore water below pools, as the isotopic signals below pools did not indicate a methanotrophic effect. Upward diffusion of CH4 against the concentration gradient is not possible and CH4 emissions were low, so we can only explain this by lateral exchange of CH4.

Indeed, pools are local depressions in the micro-relief, and we therefore agree that water should flow from lawns into pools. Nevertheless, the micro-relief is not very pronounced at our study site with Astelia lawns being elevated by only about 5-20 cm above the water table and the pool surface. This is indicated in the conceptual figure. We therefore assume, that the micro-relief does not exert much impact on the water flow in deeper peat layers and water flow from lawns to pools should be restricted to the uppermost decimeters of the peat profile. In contrast, the rhizosphere stretches over almost 2 m within highly decomposed peat. So we propose that there is a large zone with negligible water flow throughout the rhizosphere where water movement from pools to lawns would be reasonable. Due to low water movement, diffusive transport dominates and both, CH4 transport from pools to lawns and O2 transport from lawns to pools could be reasonable. We will explain this concept more clearly in the revised version of the discussion.

7. The section 4.4. is rather long and at several places contains repetitive text (e.g. page 17, lines 15-17, 21-23, 27-28; the effect of A. pumila roots was very clear, no need to repeat many times). I recommend condensing text strongly.

We understand that the interpretation of the observed emission pattern is repetitive concerning biogeochemical processes in the peat. As reviewer 1 did not comment on the length of this section, we will carefully shorten repetitive explanations in paragraph 4.4., starting in line 12 on page 17.

8. Depending on the available information from authors, the conceptual Fig. 6 can be changed (see more detailed comments in the text).

We kindly refer to our answer to general comment 6.

Specific recommendations and technical corrections incorporated in the draft file of the manuscript

Page 1

- Comment 1, line 25: How was this tested? If so, how far from root surface the "suppressive effect" is possible?

We agree, this interpretation of our results written in the abstract needs more explanation. Will be rephrased.

- Comment 2, lines 30-32: Please, rephrase the sentence in a more simplistic way. Too difficult to read and understand.

Will be rephrased.

Page 2

- Comment 1, lines 6-7: Please, check whether this value is still relevant. There is no recent reference cited.

Recent references will be checked.

- Comment 2, line 7: 28-fold

This technical correction will be changed accordingly.

- Comment 3, lines 10-12: What about low temperatures? I am not sure bogs do exist in tropics (excluding mountain regions).

We will add some information about the importance of temperature.

- Comment 4, line 17: This is not clear: is this prerequisite for the oxidation? Not trapped=not oxidized? If so, how exactly could CH4 be trapped and how does CH4 oxidation occur in e.g. rice paddies?

Indeed, this wording is confusing. We will rephrase the sentence.

- Comment 5, line 30: (delta 13C)

Will be added.

- Comment 6, line 32: Actually, values could go even to positive range.

Will be corrected.

Page 4

- Comment 1, line 17: Not clear. Do authors mean, reflect the signal of methanogenesis type? Please, rephrase.

Indeed, the hypothesis is not clear. We will rephrase it.

- Comment 2, lines 19-20: How is this known? From the introduction above, it is not clear the pools are without (vasular) vegetation. Northern pools often contain aerenchimatous plants of different species compared to lawns and hummocks, or at least Sphagnum. Please, clarify above.

This comment was already answered above in the “general comments” section, comment 2.

- Comment 3, line 23: I find the Introduction a bit too extended, especially regarding the common knowledge about methane in the very beginning and peatlands in general. Authors could immediately start the story of the importance of southern peatlands and have the necessary information on peatlands' biochemistry and vegetation specialty in there. Then the information on the isotope issue would be sufficient to formulate hypotheses without any loss of logic.

This comment was already answered above in the “general comments” section, comment 1.

Page 5

- Comment 1, line 4: Not any more? Should it be in Present Tense as the previous sentence?

The tense will be corrected and the whole manuscript checked for correct tense.

- Comment 2, line 5: Does this mean pools were nevertheless vegetated? If so, which species dominated? What was the bottom of such pools?

The vegetation in the pools will be described in more detail.

- Comment 3, line 6:

The tense will be corrected and the whole manuscript checked for correct tense.

- Comment 4, line 7: Liter of what, peat? For peat, could you provide other volumetric dimensions, e.g. dm-3?

Will be clarified and checked.

Page 7

- Comment 1, line 16: This could be too short time for CH4 flux measurement especially if outside temperatures were relatively low. How was it determined? Could zero fluxes be the reason of short exposure time?

This comment was already answered above in the "general comments" section, comment 3.

Page 8

- Comment 1, line 4: was

The tense will be corrected and the whole manuscript checked for correct tense.

- Comment 2, line 24: How? What was the volume of the sample? 3 ml? For such small volumes a separate device (Small volume unit) is necessary. Please, expalin.

Yes, the reviewer is correct. The missing information in the description of the device will be added.

- Comment 3, line 30: Confusing: organic or inorganic? Please correct.

Will be changed to "dissolved inorganic carbon".

Page 9

- Comment 1, line 22: Again, liter is not clear for peat as volume containing roots.

Will be clarified as for comment 4 on page 5.

- Comment 2, line 23: Please, provide here a value with the reference to study (studies). This will help better compare the differences between plant species.

This information will be added.

Page 10

- Comment 1, line 1:

This technical correction will be changed accordingly.

- Comment 2, line 1: This is confusing: zero flux is not detectable (otherwise it is a positive or negative). Please, rephrase. I am still wondering if outside air temperature is -0.5 C, how could 3 min be enough to measure any CH4 flux.

We agree that this phrase is confusing. We will rephrase it. Concerning a short measurement time even under low temperature, we refer to our answer to comment 3 in the "general comments" section.

Page 11

- Comment 1, lines 26-27: There was no rhizosphere below pools, so what then caused the gradient?

We agree that the phrase is misleading. We will describe the concentration gradients and explain subsequent diffusion pathways in more detail here.

- Comment 2, line 28: Suggest to rephrase: Carbon isotopic values in pore water and apparent fractionation.

We agree and will rephrase the section title.

Page 12

- Comment 1, line 7: fractionation

This technical correction will be changed accordingly.

- Comment 2, line 22: with?

This technical correction will be changed accordingly.

Page 13

- Comment 1, lines 30-31: Please, check the definitions: typically, "alternative" means alternative to oxygen. So, oxygen cannot be alternative to itself.

We agree with the reviewer. The phrase was misleading and will be changed to "… either O2 or alternative electron acceptors…"

Page 14

- Comment 1, lines 6-7: With this, authors attempt to oversell their results: "scattered between" seemingly indicate no significant difference. Indeed, Fig. 4d demonstrate rather narrow d13C-CH4 range along the whole depth profile. So, in fact, d13C-CH4 signal alone was not informative enough to approve the strong oxidative properties of rhizosphere of A. pumila. I agree that both methanogenesis and oxidation may co-exist in close vicinity, but still it may not explain lack of d13C-CH4 variation between upper and lower horizons unless CH4 produced in the rhizosphere region is even more depleted in 13C than in deeper layers. The explanation of this phenomenon because of "more reduced...microsites" is not fully clear. More than below the rhizosphere? Why?

This comment was already answered above in the "general comments" section, comment 4.

- Comment 2, line 17: According to this oxidation concept, the most 13C enriched CH4 has to be allocated at the shallowest depth. However, in contrast, it is ca. 10‰ more depleted than next depth levels (20-50 cm). In addition, d13C-CO2 is relatively more enriched that in deeper layers. How is this possible?

We inspected again our dataset to explain this. During a measurement, the isotopic signal of each sample is determined repetitively. So in fact, the signal determined from one sample is a mean of many measurements. To further improve the data quality, we excluded the isotopic signal of one sample with an elevated SD. This results now in a less enriched mean of d13C-CO2 in the uppermost peat layer.

The sampling devices were installed below the water table, but only mean of water table is given in the figures. We will check the line denoting the water table in the figures which is not exactly at the correct place. So, in the uppermost depth not much influence from roots can be expected. Accordingly, the CH4 was not enriched due to methanotrophic effects, but comparatively depleted by methanogenesis (please compare to answers to comment 4 and 5 in the general comment section).

- Comment 3, lines 17-22: This information is already repetition of the message above. I suggest to merge both parts telling the story as here but with the reference to results as in the previous paragraph. Otherwise, it is excessive.

We understand that this part of the discussion is repetitive and kindly refer to our answer to comment 4 in the "general comments" section. We will follow the helpful suggestion of the reviewer and incorporate aspects of the paragraph on page 14, lines 17-32 into the paragraph above (lines 7 and following).

- Comment 4, lines 23-26: This contradicts to the data measured: having acetoclastic methanogenesis and co-existence of oxidation should generate much more enriched d13C-CH4 values in comparison to deep peat. Fig. 4d cannot support this. Seemingly, change of fractionation factor with depth was not significant either. The available data are not enough to approve existence of acetoclastic methaogenesis. Please, discuss this

This comment was already answered in the "general comments" section, comment 4 and 5.

- Comment 5, lines 27-28: Again, there is not enough evidence to support the hypothesis. As it is stated, this is speculation and should be rephrased.

We will phrase this information more carefully and kindly refer to our answer to general comment 5 in the "general comments" section.

- Comment 6, lines 29-30: Yes, but it was small below the rhizosphere too! Speculation!

This comment was already answered in the "general comments" section, comment 4 and 5.

- Comment 7, line 32: They also increased at the very top of profile. No information about significance of differences in fractionation factor between depths is provided.

This comment was already answered in the "general comments" section, comment 5.

Page 15

- Comment 1, line 6: This may also mean occurrence of hydrogenotrophic methanogenesis in anaerobic rhizosphere zones. For example, Galand et al. (2002) FEMS, demonstrated dominance of H2-trophic methanogens in upper peat layer in a boreal northern peatland. Is there any evidence for southern peatlands too? This may partly explain relatively depleted d13C values of CH4 in the rhizosphere zone.

We agree with the reviewer that elevated levels of H2 in the upper rhizosphere below Astelia lawns may indicate production of CH4 by hydrogenotrophic methanogenesis. We will include this possible explanation. We kindly refer to our answer to general comment 4 and 5 in the "general comments" section for more details.

- Comment 2, lines 17-18: This can be misleading: 2nd hypothesis specified processes based on isotopic values, whereas here authors refer more to the concentrations/fluxes thereby considering rather 1st hypothesis. The latter, however was not supported. Please, rephrase. Also regarding the 2nd hypothesis, "less affected" is not appropriate for the hypothesis. Please, check the respective comment above.

Indeed, the interpretation in lines 17-18 needs a better link to the hypotheses. Will be rephrased.

- Comment 3, lines 19-21: This is not clear: How could roots of A. pumila appear under pools? Was this observed during coring? If so, then the conceptual diagram should demonstrate that roots of A. pumila expand below pools. Check and correct accordingly.

This will be clarified: It is possible, but we do not know whether the roots appear under pools. Therefore we did not include this in the conceptual figure. We propose that roots control CH4 dynamics below pools only by releasing O2 that is used to consume CH4 thereby maintaining concentration gradients. We will clarify this on page 15, lines 19-21. Please refer to the "general comments" section, comment 6 for further details.

- Comment 4, line 22: Of what, CH4 or oxygen?

Will be specified. Please refer to the "general comments" section, comment 6 for details.

- Comment 5, lines 23-26: This statement is unclear: whereas gas diffusion along gradient is clear for me (from pools to lawns) water movement is not the same. Pools are local depressions, so water should flow from lawns into pools. If this flow is so low, then the gas diffusion in opposite direction can be stronger, but this means almost standing water. In case there is a lateral flow of water (what is very natural), then the gas flow can't be counter to it. Therefore, I could understand the inflow of oxygen from lawns into pools, but not CH4 from pools to lawns. The overall picture may change if there is a slope, but then lawns and pools have to be arranged accordingly. Pools will get matter of those lawns which are elevated and transfer it downwards to other lawns. If there is a slope on the site, then the conceptual figure should somehow reflect it. Check!

This comment was already answered above in the "general comments" section, comment 6.

- Comment 6, lines 28-30: What is meant, suppression of methanogenesis or CH4 oxidation? This is important in context of measured isotope values. Please, specify.

We did not check know whether the roots appear under pools, but it would be reasonable. Therefore, here suppression of methanogenesis is meant here as an explanation for low CH4 concentrations in pools. This will be specified in the revised discussion. Please refer to the "general comments" section, comment 6 for further details.

Page 16

- Comment 1, lines 8-9: This is not fully clear: the limiting factor for hydrogenotrophic methanogenesis is typically H2 which is very reactive, since C source is CO2. H2 was sufficient, CO2 concentrations were also available, so why was then H2-reduction methanogenesis although dominating (depleted d13C-CH4) but not intensive? Maybe other anaerobic processes (sulfate reduction) outcompeted methanogenesis?

The reviewer is right, we need to check the argumentation here. Despite H2 concentrations and DIC:CH4 ratios suggest methanogenic conditions, CH4 production seemed to be limited

below pools even at peaking H2 levels. Very negative d13C values suggest that methanogenesis was thermodynamically unfavorable. Instead, methanogenesis might have been outcompeted by other electron accepting processes, such as sulfate reduction as suggested by the reviewer. Peaking H2 concentrations indicate that fermentation processes were active, but suggest that methane was produced only very locally if all. We will provide a more detailed explanation in the revised discussion.

Page 17

- Comment 1: The section 4.4. is rather long and at several places contains repetitive text (e.g. page 17, lines 15-17, 21-23, 27-28; the effect of A. pumila roots was very clear, no need to repeat many times). I recommend condensing text strongly.

This comment was already answered above in the "general comments" section, comment 7.

Page 26

- Comment 1, lines 4-5:
- Comment 2, line 5: On the figure (f), platform 3 instead platform 2 is denoted. Check!

The reviewer is right, we need to check this.

Page 30

- Comment 1: It was discussed a lot about lateral flows, which how, are not reflected in this conceptual diagram. It is also not clear if the site has elevation/slope property. In such a case, please demonstrate the respective relationships.

This comment was already answered above in the "general comments" section, comment 6.

References

Berger, S., Praetzel, L. S. E., Goebel, M., Blodau, C., and Knorr, K. H.: Differential response of carbon cycling to long-term nutrient input and altered hydrological conditions in a continental Canadian peatland, Biogeosciences, 15, 885-903, 2018.

Mastepanov, M., Sigsgaard, C., Mastepanov, M., Strom, L., Tamstorf, M. P., Lund, M., and Christensen, T. R.: Revisiting factors controlling methane emissions from high-Arctic tundra, Biogeosciences, 10, 5139-5158, 2013.

McEwing, K. R., Fisher, J. P., and Zona, D.: Environmental and vegetation controls on the spatial variability of CH4 emission from wet-sedge and tussock tundra ecosystems in the Arctic, Plant Soil, 388, 37-52, 2015.