Dear editors of the *Biogeosciences*

Revision for "Small spatial but large sporadic variability in methane emission measured from a patterned boreal bog" (manuscript no. bg-2017-443).

We want to thank the two Referees for their constructive comments that certainly improved the report. Accordingly, we have revised our manuscript following the suggestions made by the Referee #1 and the Referee #2 Tim Moore, or replied in detail why we have not followed their guidelines in some places. Here you will find the original statements of the referees and our responses to them.

Firstly, we would like to address the extreme high methane emissions that we reported in the manuscript having been measured sporadically in 2013 and 2014. After checking these high fluxes once more, we found nine highest fluxes measured in 2013 to result from a calculation error after all. Originally, we were quite skeptical about such high fluxes and checked a variety of potential sources of mistake, such as gas chromatograph data processing, typing errors, nonlinearity caused by ebullition and correctness of the flux calculation formula. What finally was the reason behind this very unfortunate calculation error was a wrong reference in the data processing sheet, which did not occur with most of the measurements, but only in these 9 cases. After correcting these cases, we also went carefully through the rest of the data to make sure that all the calculations are now correct and all the fluxes reported in the revised manuscript are true. The highest methane flux in the corrected data is now 1254 mg m^{-2} d^{-1} instead of the previously reported 17 000 mg m^{-2} d^{-1}. Consequently, we rerun the statistical analyses with the whole data set, as there was no need to exclude the highest fluxes from the analyses anymore. Our results remain generally the same as previously, but the statistically significant differences between the plant community types in 2013 changed a little. We reported previously that hummocks had higher methane fluxes than other plant community types that year. Now, our result is that hummocks and high lawns had higher fluxes than high hummocks and bare peat surfaces in 2013. Additionally, there were also higher methane fluxes from bare peat surfaces than from high hummocks in 2014. We have corrected the manuscript and the figures according to the new results of the analyses of the whole data set and excluded the incorrect report and discussion of the "extreme high fluxes". We sincerely apologize for our mistake.

Referee #1:

Comment: Line 14: please add a name and location of the bog.

Response: We have added this information of the study site in the abstract.

Changes in manuscript: Line 14: we have added "situated in Siikaneva in southern Finland" to the sentence.

Comment: Line 17: add "species composition" to the list of variables that differ between the studied plant community types.

Response: We have added this as suggested.

Changes in manuscript: Line 17: “species composition” has been added.

Comment: Line 36: Peatland can also be drained. Since here we are dealing with natural ecosystem, the term "mire" could be more correct.

Response: The Referee has a good point that “peatland” can also refer to a drained ecosystem. To be precise we have now changed the text in the beginning of introduction to “Mires or undrained peatlands”.

Changes in manuscript: Line 36: “Peatland” has been changed to “Mires and undrained peatlands”.

Comment: Line 111: How far weather station is from the bog?

Response: The Juupajoki-Hyytiälä weather station is located about 6.3 km east from the studied bog site in Siikaneva. We have now added this information.

Changes in manuscript: Line 111: we have added “station that is located 6.3 km east from the bog site” to the sentence.

Comment: Line 114: "Bog pools" could be better term than “open water ponds”.

Response: We agree that “pool” would be better than “pond”. We have modified the sentence.

Changes in manuscript: Line 114: “open water ponds” has been changed to “open water pools”.

Comment: Line 114 about bare peat surfaces: ... or “mud bottom hollows". Since this term is used in figures and other studies please introduce it here as well.

Response: The Referee is correct that the term “mud bottom hollows” has been used in some other studies instead of the term “bare peat surfaces” that we are using in the manuscript, and the term “mud bottom” has accidentally remained in our manuscript in the caption of the figure 2 from a previous draft of the manuscript. We are now consistently using the term “bare peat surfaces”.

Changes in manuscript: No changes. The wrong term is corrected in the caption of the figure 2.

Comment: Line 121: To explain the differences in fluxes it is important to know the depth, type, degree of decomposition etc. of peat layer below measuring points.

Response: We agree. Unfortunately, we did not study the peat layers below the measuring points.

Changes in manuscript: No changes.

Comment: Line 125: add “bog microforms or ...”

Response: We have added this.

Changes in manuscript: Line 125: “or bog microforms” has been added to the sentence.

Comment: Line 149: Since sporadic ebullition is typical for bogs it is not correct to exclude them as likely errors.

Response: The Referee is correct that sporadic ebullition is typical for bogs and it should be taken into account. However, in here we focus only on diffusive gas flux, which we can reliable measure with our method. We are aware that in some previous studies the proportion of methane ebullition has been calculated based on non-linear chamber fluxes. Unfortunately, in our very wet measurement site, we were afraid that the ebullitive fluxes could be mixed up with artefacts caused by the measurement protocol. These artefacts can easily be differentiated from linear, diffusive fluxes, but not from ebullitive fluxes.

Changes in manuscript: No changes.

Comment: Lines 224–230: Table can give better overview of these data.

Response: We fully agree.

Changes in manuscript: The information on the lines 224–230 is now found in a newly added table (Table 1) and the text has been shortened accordingly.

Comment: Line 269; results that there were higher methane fluxes from hummocks than from the other plant community types in 2013: This is surprising finding and needs an explanation.

Response: After we had corrected the calculation error resulting in the very high fluxes and rerun the statistical analyses, we found that hummocks and high lawns had higher fluxes than high hummocks and bare peat surfaces. We fully agree, that this is a surprising finding, but we do not have an explanation. We would also like to draw attention to the very small differences among these plant community types.

Changes in manuscript: Line 270: we have added a sentence “This result in 2013 was surprising, but the differences between the plant community types are small.”

Comment: Line 273: Peat temperature response of methane flux where the response was assumed… ". Difficult to follow the meaning of this part of the sentence.

Response: We fully agree that this was confusing as part of the results. For this reason, we have now explained this in the methods more clearly than before and removed this part from the results.

Changes in manuscript: The sentence starting on the line 273 has been removed. See also the modifications in the 2.5 Analyses section starting on the line 212.

Comment: Line 280: "WT did not explain variation in methane fluxes…". Interesting. Since WTD also directly influence the temperature of the top layer of anaerobic peat with most active methane production.

Response: Yes, indeed. This surprised us as well, but this is likely to be related to the general wetness of the whole site.

Changes in manuscript: No changes.

Comment: Line 299: High fluxes can be caused by the gas release from deeper peat layers therefore to compare fluxes from different locations one need to look on deeper peat as well.

Response: The high fluxes were caused by our unfortunate error, as explained in the beginning of the letter. We have now removed this part.

Changes in manuscript: This part has been removed.

Comment: Line 326: High WT, close to the surface, also mean higher temperature and these factors should be discussed in combination.

Response: This is an interesting point, but the combination of WT and peat temperature could not be studied, as WT did not significantly explain the variation in methane fluxes.

Changes in manuscript: No changes.

Comment: Line 333: Authors may add reference Karofeld, 2004 (The Holocene, 14 (1)) who studied mud-bottom hollows from this point of view.

Response: We have added this as suggested.

Changes in manuscript: line 333: Reference 'Karofeld, 2004' has been added as suggested.

Comment: Line 347: add 'Frenzel & Karofeld, 2000' to the reference list.

Response: We have added this as suggested.

Changes in manuscript: Line 347: Reference ‘Frenzel and Karofeld, 2000’ has been added as suggested.

Comment: Line 357: Please add also the effect of decreasing peat temperature in uppermost anaerobic layer caused by lowered WT and insulation by drier plants.

Response: This is an interesting point, but the combination of WT and peat temperature could not be studied, as WT did not significantly explain the variation in methane fluxes.

Changes in manuscript: No changes.

Comment: Line 376: See also Karofeld, 2004.

Response: We have not added this as a reference because we removed the two sentences from the discussion.

Changes in manuscript: The two sentences starting on the line 372 has been removed.

Comment: Line 381: add “thickness of” in front of “aerobic peat layer”.

Response: We agree that this clarifies the sentence.

Changes in manuscript: Line 381: “thickness of” has been added in front of “aerobic peat layer”.

Comment: Line 382: Please consider also the effect of temperature change in the top layer of anaerobic peat with lowering water table.

Response: Again, this is an interesting point, but the combination of WT and peat temperature could not be studied, as WT did not significantly explain the variation in methane fluxes.

Changes in manuscript: No changes.

Comment: Line 392: To explain such kind of not typical fluxes one have to look also deeper peat layers and sporadic gas release from there.

Response: This is a good point. Unfortunately, we did not study the peat layers below the measuring points and focused only on diffusive gas flux in this study.

Changes in manuscript: No changes.

Comment: Line 418: See also Karofeld & Tõnisson, 2014, Hydrological Processes 28 (3).

Response: The phenomenon to which this discussion was related was caused by our unfortunate error, as explained in the beginning of the letter. We have now removed this part of the discussion.

Changes in manuscript: The discussion about extreme high methane fluxes on lines 410–429 has been removed.

Comment: Line 423: Due to the hysteresis effect and depending from peat type and decomposition the effect of air pressure on gas release may take longer time to accumulate and then release.

Response: This is completely true and a good point. However, the phenomenon to which this was related was caused by our unfortunate error, as explained in the beginning of the letter.

Changes in manuscript: This part of the discussion has been removed.

Comment: Line 432: But why with EC maximum values measured with chambers (and likely caused by active bubble release) where not detected?

Response: Again, the phenomenon to which this discussion was related was caused by our unfortunate error, as explained in the beginning of the letter, and we have now removed this part of discussion

Changes in manuscript: This part of the discussion has been removed.

Comment: Line 437: This sounds more like Results and not Discussion.

Response: We fully agree and removed most of this part from the discussion and replaced it with "…and chamber fluxes were occasionally higher than the EC fluxes".

Changes in manuscript: Please see the modified text on lines 345–441.

Comment: Discussion is a bit too long but at the same time on explaining or discussing some important aspects (temperature effect combined with changes in WT, high emission from hummock).

Response: We shortened the discussion as suggested.

Changes in manuscript: The discussion has been shortened as mentioned in the previous replies above.

Referee #2 Tim Moore:

Comment: The authors express some surprise in the weak relationship between water table position and methane flux, anticipating a larger flux where there is a higher water table, as has been shown elsewhere. As with all relationships between a gradient of an environmental variable and the object of interest (in this case methane flux), the strength of the relationship depends on at least two things: one is the range of the variable, and another is how other variables interact along the gradient. Strong water table:methane flux relationships have been shown elsewhere, but they tend to occur over large water table gradients (50 or more cm, rather than the 25-30 cm encountered here) and when they are of a small scale, the relationship is not simple, for example Bellisario et al. (1999). Moreover, water tables are not static but rise and fall and that can create hysteresis so it is not at the highest water table that maximum emission is reached (e.g. Brown et al. 2014). While mean water table data are presented in Figure 1, was there much variation in water table position during the three seasons, and does this play a role in the observed temporal variability in flux?

Response: This is a good point. We were only focusing on the differences among the plant communities. As there were no differences in the seasonal trends among these communities and WT as such did not explain the fluxes, we did not go any further with this. We now have plotted the residuals of the model together with continuous WT data from the site for each year separately. Please see the corresponding graph below. Although, there seems to be some indication of hysteresis in 2012 spring, we do not see any general pattern when looking all the years. Therefore, we did not add this analysis to the manuscript.

Changes in manuscript: No changes.

Comment: The interaction between the influence of aerenchymous plants facilitating emission and the non-aerenchymous plants providing root exudate to stimulate methanogenesis, to partially explain the small spatial variability, is well presented, though no evidence is put forward to support the processes involved.

Response: Yes, this is truly only speculation, as we do not have measurement data to back this up.

Changes in manuscript: No changes.

Comment: Perhaps the most surprising result of the study is the occurrence of large positive or negative methane fluxes, which because of linearity in change of gas concentration in the chambers, could not be discounted (10% of the measurements were excluded because of non-linearity or other reasons). It would be interesting to see a graph depicting the magnitude:frequency of the observed fluxes (n = 516); it is not clear whether the extreme fluxes (2.5%) included both positive and negative fluxes (lines 190 to 195 could be clarified).

Response: As explained in the beginning of this letter, the extreme fluxes were after all a result of a very unfortunate error. For this reason, this part is no longer relevant.

Changes in manuscript: The result and discussion parts regarding extreme high positive methane fluxes have been removed.

Comment: These extreme fluxes are large by normal measurements, though it should be noted that they were observed over 35 minutes and then scaled up to a daily estimate: it is unlikely that the methane stored in the peat, or its generation, could sustain such a high flux for a day: what are rates of methane production for these systems (g/m2/day, or how much methane is stored in the peat profile?). There seems to be no strong attention to the reasons why a large methane emission could be observed: perhaps, given that these are 'real' fluxes, more information could be given on their spatial and temporal patterns (rather than 'random and sporadic'): this is hinted at in lines 293 to 299: where and when did the fluxes > 1 g/m2/d occur?

Response: Again**,** as explained in the beginning of this letter, the extreme fluxes were after all a result of a very unfortunate error. For this reason, this part is no longer relevant.

Changes in manuscript: The result and discussion parts regarding extreme high positive methane fluxes have been removed.

Comment: Perhaps more disconcerting is the occurrence of large uptake rates of methane, up to 300 mg/m2/d and it is difficult to conceive of a mechanism which would allow such large amounts of methane to be 'taken up' microbially, through methanotrophy. As noted, the largest methanotrophic potentials are usually observed around the position of the water table, which in these sites are close to the peat surface, so the diffusive pathway for methane consumption is fairly short. Nevertheless, it is somewhat surprising that these large consumption rates appear primarily on bare peat surfaces (line 291), whereas one might expect less microbial activity than where vegetation cover was denser (though the arenchymous R. alba occurs in the bare peat spots). Are these large consumption rates related to water table position (and hence largest potential rates of methane consumption)?

Response: We also found this rather surprising. Unfortunately, we did not find an explanation for this. The highest net uptake occurred in spring when the measurement plot was still frozen, but this was not the case during the other net uptake events. Of course, ice does not explain the net uptake of methane either. We would also like to note that together with all of the fluxes, we also inspected these again and found nothing wrong.

Changes in manuscript: No changes.

Comment: Line 15: add "with chamber exposure of 35 minutes".

Response: We have added this to the sentence.

Changes in manuscript: Line 15: we have added "with chamber exposure of 35 minutes" to the sentence.

Comment: Line 15: change “for quantifying” to “to quantify”.

Response: We have corrected this.

Changes in manuscript: Line 15: “for quantifying” has been changed to “to quantify”.

Comment: Line 21: change “were higher…” to “was higher…”.

Response: We have corrected the sentence according to the new results of the statistical analyses of the whole data set.

Changes in manuscript: Starting on the line 21: “The only exception were higher fluxes from hummocks and high lawns than from high hummocks and bare peat surfaces in 2013 and from bare peat surfaces than from high hummocks in 2014.”

Comment: Line 38: change “water level” to “the water table”.

Response: We have changed this.

Changes in manuscript: Line 38: “water level” has been changed to “the water table”.

Comment: Line 41: change “methane” to “CH_4”. As you have defined this term, you should use it throughout.

Response: We have defined “methane” as the term “CH_4” as it is used in the figures. However, in the text we prefer “methane” as it in our opinion improves the readability and is not very much longer than “CH_4”.

Changes in manuscript: No changes.

Comment: Line 42: add “the” in front of the “water table”.

Response: We have corrected this.

Changes in manuscript: Line 42: “the” has been added to in front of the “water table”.

Comment: Line 143: Do you have a continuous measurement of water table somewhere at the site, so that you can situate individual measurements in the dynamics of water table (see comment about hysteresis)?

Response: Please, see our response to the previous comment on this matter and the graphs in the end of this letter.

Changes in manuscript: No changes.

Comment: Line 171: add a space between “m” and “s^{-1}” in the unit of friction velocity.

Response: We have corrected this.

Changes in manuscript: Line 171: a space has been added between “m” and “s^{-1}” in the unit of friction velocity.

Comment: Line 194: A bit confusing why you would exclude the upper 2.5% and retain the lower 2.5%, as both groups were 'valid' measurements.

Response: This is not relevant any more, as explained before.

Changes in manuscript: Lines 186–199 have been removed.

Comment: Line 260: Would be nice to see a frequency:magnitude graph of all measurements, including the 'excluded' ones.

Response: Again, this is unfortunately not relevant any more, as explained before.

Changes in manuscript: No changes.

Comment: Line 286: add “a” in front of “high WT”.

Response: We have corrected this.

Changes in manuscript: Line 286: “a” has been added in front of “high WT”.

Comment: Line 287: about methane oxidation: well, you do not know it was 'oxidation', all we know is that the methane concentration in the chamber showed a rapid decline (from 1.8 to ?? ppm over 35 minutes) and you assume it was through microbial consumption/oxidation, though this would imply very high rates of methanotrophy. there maybe other reasons, such as advective effects, though there seems to be no support for an alternative.

Response: As pointed out we are not able to say if all the negative fluxes are due to net oxidation. Accordingly, when referring our results we now use term “negative net flux”.

Changes in manuscript: Throughout the manuscript, “oxidation” has been replaced with “negative net flux” when referring to our own results. In addition, we have added a sentence stating, that these negative fluxes may be linked to methane oxidation (Line 393).

Comment: Line 301: add “an” in front of “ecosystem level”.

Response: We have corrected this.

Changes in manuscript: Line 301: “an” has been added in front of “ecosystem level”.

Comment: Line 304: maybe you could add “but real” to the sentence “as they were random and sporadic events” as defining the extreme methane fluxes.

Response: Unfortunately, this is not relevant any more, as explained before.

Changes in manuscript: This sentence has been removed.

Comment: Line 319: add “a” in front of “little higher”. In English 'a little higher' has a different meaning than 'little higher'. The former means it is higher, whereas the latter implies that they are the same. I think 96 vs 57 and 72 is in the former category.

Response: The Referee is correct that we meant “a little higher” in this case. We have corrected this.

Changes in manuscript: Line 319: “a” has been added in front of “little higher”.

Comment: Line 345: and whether you could ever explain the high rates of methane uptake..... on our discussion: “Studying the microbial communities and their methane production and oxidation potentials in Siikaneva bog would be the next step to understand why methane fluxes are so similar over the different plant community types in the site.”

Response: We agree here, such study would also help to explain the reasons behind net methane uptake.

Changes in manuscript: A sentence has been added to Line 345: **“**This could also clarify to what extent the high negative net fluxes are explained by microbial methane oxidation.”

Comment: Line 348: change “effect” to “affect”.

Response: We have corrected this.

Changes in manuscript: Line 348: “effect” has been corrected to “affect”.

Comment: Line 355: correct “boreal climate” to “boreal climates”.

Response: We have corrected this.

Changes in manuscript: Line 355: “boreal climate” has been corrected to “boreal climates”.

Comment: Line 356: add “the” in front of “WT”.

Response: We have added this.

Changes in manuscript: Line 356: “the” has been added in front of “WT”.

Comment: Line 356: change “enables” to “potentially creates”.

Response: We have changed this.

Changes in manuscript: Line 356: “enables” has been changed to “potentially creates”.

Comment: Line 365: add “a” in front of “positive correlation”.

Response: We have corrected this.

Changes in manuscript: Line 365: “a” has been added in front of “positive correlation”.

Comment: Line 375: a comment on the reference to Whiting and Chanton, 1993: careful, this study was driven by the rice outlier.....

Response: This is true. We removed the reference and the sentence completely.

Changes in manuscript: The two sentences on lines 372–375 have been removed.

We hope that the editors and the Referees find the quality of our revised manuscript sufficient to warrant publication in the *Biogeosciences*. We thank the Associate Editor and the Referees for their valuable and constructive comments.

On behalf of co-authors,
Sincerely yours,

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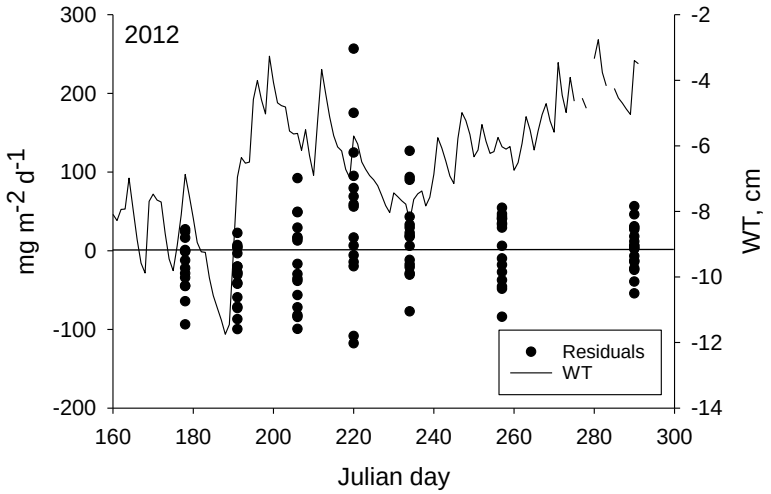

Figure 1. Residuals of the model plotted together with water table (WT) in 2012.

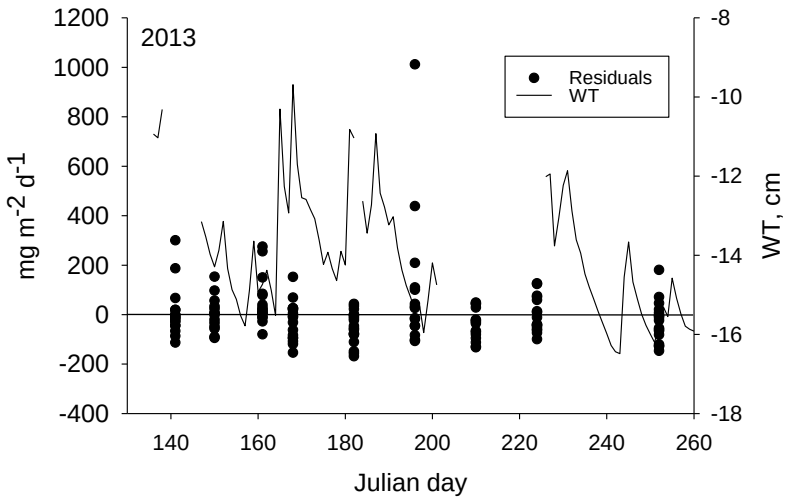

Figure 2. Residuals of the model plotted together with water table (WT) in 2013.

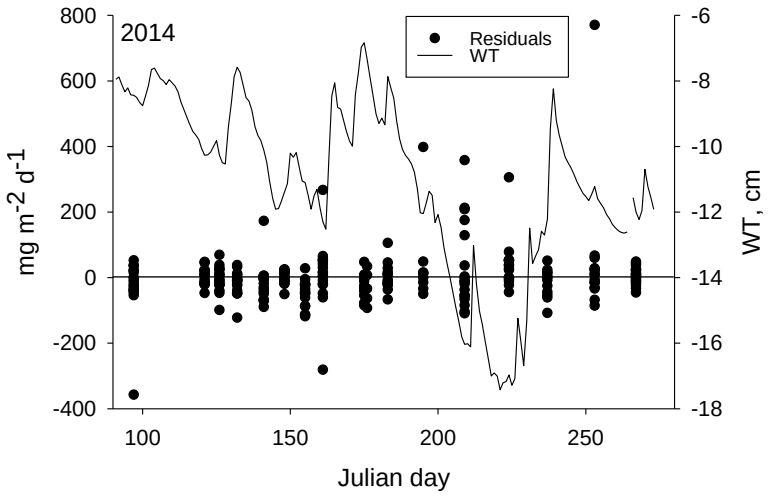

Figure 3. Residuals of the model plotted together with water table (WT) in 2014.