Report #1

The revised manuscript by Järvi-Laturi et al. presents very valuable findings of vegetationdriven year-round methane flux variation in a northern fen. One of the greatest contributions to the field of methane research in this manuscript is the inclusion of non-growing-season methane flux data and the presented evidence of vegetation contribution to wintertime methane fluxes. I think these results are very interesting and the importance of these findings has been highlighted more clearly in the revised version. I believe that this paper will motivate more researchers to study wintertime methane fluxes and particularly the contribution of plants to them, an aspect that has been lacking for a long time. I recommend accepting the revised manuscript as is.

Response: We are happy to hear that you found our manuscript improved and thank you for appreciating our work.

Report #2

The authors have thoughtfully considered comments of two reviewers and thoroughly revised the manuscript to address the concerns raised. The clarifications that the authors have added to the methods and results have made it much easier to follow the findings and conclusions from the study. I recommend publication but suggest a few very minor revisions for further clarity in the final version.

Lines 118-120: In the response to reviewer 1, the authors provide several pieces of evidence to indicate that there is minimal diurnal variation. I suggest that reference to this information is provided here to support the stated assumption.

Response: Our assumption regarding limited diel variation is supported by continuous CH_4 flux measurements from two automatic chambers located at the same site near our study plots (Mastepanov et al., unpublished data). Based on this dataset, we calculated the daily standard deviation of CH_4 fluxes for each chamber during the peak flux season (July), when flux values ranged around 12 to 15 mg $CH_4/m^2/h$ and diel variation is typically most pronounced. We found that the average daily standard deviation was 0.758 mg $CH_4/m^2/h$ for Chamber 1 and 0.696 mg $CH_4/m^2/h$ for Chamber 2, indicating limited diel variability. To compare, we also looked at the standard deviation for snow season fluxes and discovered standard deviation of 0.518 mg $CH_4/m^2/h$ for Chamber 1 and 0.439 mg $CH_4/m^2/h$ for Chamber 2. This clarification has been added to lines 124–125 in the manuscript: "*These assumptions were based on data from two automatic chambers located at the same site near our study plots, which show limited diel variation in fluxes (Mastepanov et al., unpublished data).*"

Line 159: I had previous inquired about microtopography at the site and the authors note in their response that microtopographic variation is minimal and that WTD measured 1 m from the plot likely provides a reasonable estimate of WTD at the plot. I suggest adding a sentence with that information here as other readers are likely to also question the representativeness of the WTD measurements.

Response: To clarify microtopographic variation we have added text on lines 158-159: "*As the microtopographic variation and WTD fluctuation at the site is minimal (see 2.1), WTD measured 1 m from the plot likely provides a reasonable estimate of WTD at the plot*."

Report #3

Below you find my report:
The authors present a one year study from a northern rich fen in which methane emission measurements were conducted on 36 plots in order to assess the influence of the vegetation community composition on methane flux. Further, the authors report that the biomass of vascular plant, sedges and especially C. rostrata and the ratio of biomass of vascular plant to biomass bryophytes correlated well with measured methane fluxes. Pointed out in particular is the strong influence of C. rostrata with regards to methane emissions during the snow free season. Here C. rostrata seems to have a large enhancing effect on methane emissions.

The study is based on an impressive number of flux measurements and a sophisticated spatial setup to cover potential spatial variability.

The research conducted is of high quality and I have no doubt that the approach is scientifically valid and robust, however I have some major criticisms that need to be assessed:

Response: We thank you for the positive feedback and the criticism. Below, we have addressed the comments one-by-one:

1. The conclusions drawn from the results are very strongly based on aboveground biomass. As only aboveground biomass was sampled firstly I think it is necessary to more thouroughly discuss the multitude of impacts the belowground biomass can have on the methane flux, be it negative or positive. Secondly, the aspect that the biomass was sampled outside the plots to which the methane fluxes were associated too adds to the uncertainty and this uncertainty should be stressed even more in the discussion as other abiotic factors may influence methane flux very strongly. These factors are also known to be highly variable in space and time (WTD, soil temperature, micro topography etc.) This is not a general criticism of the method, however, I think this needs to be stressed more strongly. Thus, the conclusions, although valid, should be slightly relativised in my opinion. With this regard I find it overstating that plant community composition controls the entire spatial variation of methane flux, as it is mainly an effect of C. rostrata. Please consider the special plant traits of C. rostrata in your discussion and consider changing the title slightly.

Response: Firstly, in our paper we concentrate on the spatial variation of the fluxes, rather than the functional aspects of it, like aboveground vs. belowground parameters. However, we acknowledge the importance of the belowground parts of the plants and agree that it is probably one of the main drivers of the spatial variation of methane fluxes. As a response to this comment, we have added two sentences related to this topic on lines 381–385: "*This study focused only on aboveground parts of the plant communities, and therefore the multiple effects that the belowground parts may have on methane production, consumption, transport and emission (e.g. Määttä and Malhotra, 2024) were not considered in the analyses. However, we acknowledge that belowground plant characteristics likely play a significant role in explaining the variation in our methane flux data that could not be explained by the other studied variables.*"

Secondly, our study site is a sloping flow-through fen, which has rather homogenous microtopography, water level, and soil temperature (as partly explained in manuscript in section 2.1). Further, the locations of biomass sampling were chosen to reflect the height of the vegetation at the plots to lower the uncertainty of the sampling (see lines 137-138). Due to these reasons, we believe that the biomass samples reflect the biomass of the plots strongly enough for making these conclusions.

Thirdly, it was, in fact, the point of this paper to talk about species as an indicator of a plant community, and to show that spatial variation of methane fluxes in this particular fen is not dependent on the usual environmental controlling factors (water level or soil temperature, but see lines 408–410 about the role of deeper soil temperature).

Fourthly, we think those traits of *C. rostrata* (*i.e.*, deep rooting system, partially evergreen nature) that could affect the flux rate are already stated in the text. We write in lines 359 onwards e.g. the following: "*High amounts of vascular plant, sedge, and C. rostrata biomass likely enhance methane production and release by supplying labile organic carbon substrates for methanogenesis through deep root systems throughout the year (Alm et al., 1999; Joabsson et al., 1999, Saarinen, 1996). High flux rates from C. rostrata dominated plots (Fig. 7) may be due to the species' high methane transport rate (Ge et al., 2023), the high porosity of its roots (Ge et al., 2023), and a low capacity to oxidize methane into* CO_2 *in the rhizosphere (Ström et al., 2005). Additionally, the perennial nature and deep rooting traits of C. rostrata (Saarinen, 1996; 1998) could support methane production and transport during the cold season by providing substrates for microbial processes in deeper peat layers and a potential pathway from belowground to the atmosphere. Approximately 40 % of C. rostrata shoots at our study site overwinter green (Cunow et al., unpublished data), indicating the potential to transport gases also during wintertime*."

As two of the referees commented the wording of the title, we have chosen to slightly change the title and replace the word "controls" to the word "explains".

2. The second main criticism is regarding the visualization of the data. In my opinion the visualizations of the multivariate analyses should be included in the manuscript as it would underline the descriptive nature of the resuls section and make it more easy to follow.
As you are talking about spatial variation I think it would add to the value of the manuscript if you include a visualization of the spatial variation (perhaps a map of mean fluxes, or divided by season, one for snow season and one for snow-free season, size or colour as indicator for methane flux). Thereby it would be much easier to follow the overall story.

Response: We understand that lifting some figures from the appendix to the manuscript might help following the text and have now done so. When choosing locations for the figures, we were concerned about overloading the manuscript and therefore decided to place CCA graphs to the appendix instead of the manuscript.

We have added two supplementary figures to appendix B (Figs. B7, B8) to visualize the spatial variation of the mean fluxes in snow-free and snow seasons. We refer to these figures in lines 206 and 212).

3. Just for consideration:
As a large part of the variability of methane fluxes is unexplained you could consider using linear mixed effect models as your title suggests that methane is the response variable and you could include species as a random effect and results would make it statistically more robust as to which effect the plants have. It would be interesting to know how much of the variability can actually be explained.

Response: We thank you for this suggestion and agree that it could be an interesting analysis. However, at this stage, we decide not to perform any new analyses for this manuscript.

Generally, I find this work important especially under the consideration of the very strong data set and the importance of winter time flux measurements in order to assess regional if not global uncertainty.

Therefore, after the assessment of the above mentioned general concerns and below mentioned minor aspects by the authors, I would recomment to consider it further for publication.

Minor aspects:

55: have a „stronger“ focus

Response: corrected

Comment:
High pH values → it would be nice if you discuss the conditions of this fen in comparison to other northern rich fens shortly in the discussion to put the boundary conditions into perspective.

Response: Thank you for this observation. Although our study site exhibits relatively high pH values, rich fens in the northern boreal region do not universally share this characteristic. Groundwater pH in such habitats is often slightly below 7 (Hájek et al., 2021), reflecting regional differences in bedrock chemistry and hydrological inputs. For example, Olefeldt et al. (2017) describe a rich fen in interior Alaska with surface water pH of 5.2–5.4, yet still classified as rich due to its minerotrophic status and vegetation dominated by brown mosses and sedges. These examples highlight that rich fen classification depends more on hydrological connectivity, nutrient availability, and species composition than on absolute pH levels.

Our site's pH values (~7) are at the upper end of the range typically observed in northern boreal rich fens. For example, Laitinen et al. (2021) reported water pH values ranging from 4.0 to just over 6.4 across 18 eastern Finnish sloping fens. This suggests that while our site clearly qualifies as a rich fen, it may represent an unusually base-rich example within the northern boreal context.

We have added a short description of the boundary conditions to "2.1 Study site" in lines 76-77: "*These values are relatively high and place our site at the upper end of the pH range typically observed in northern boreal rich fens, which often exhibit pH values below 7 (e.g., Hájek et al., 2021; Olefeldt et al., 2017; Laitinen et al., 2021).*"

Likewise I find it necessary to at least also compare the measured fluxes with flux values from other comparable studies. To date I only found comparisons to as how large the contribution of e.g. the winter season fluxes is to the total annual flux (Alm et al.).

Response: In the previous manuscript version, we included a reference about flux rates from northern boreal rich fens (Jammet et al. 2017, see line 314). We chose to refer to this study as it resembles our study site. It should be noted that there are not too many studies published from northern boreal rich fens where methane fluxes would have been measured similarly to our methods.

77: WTD with one milimieter precision seems overly accurate.

Response: We agree with the referee and have corrected the WTD values to one centimeter accuracy.

84: half of the area was fenced → discuss if there were any differences?

Response: Added in lines 89–90: "--- *and the differences in vegetation and methane fluxes between inside and outside of the fence were likely related to the hydrological conditions, rather than the effects of the exclusion (Väisänen et al., unpublished data).*"

Suggestion: change the units of flux to scientific notation

Response: We have considered the units and choose to keep them as they were.

consider also using flux measurements with lower R^2, what is with very low fluxes?

Response: We have considered them, as explained in lines 115-116: "*We accepted the measurements with an R^2 value ≥ 0.95 (n = 3589) and inspected all the rest (n = 691) individually, leaving out measurements showing very strong non-linearity or any other sign of failed measurement (n = 159).*"

119: assuming an average flux over all days sounds like it could be prone to large overestimation - what about just linearly interpolating the data

Response: We do not believe this is a significant concern in our study, as the flux measurements were conducted at high frequency, with only a few longer gaps. Given that our analysis spans a full year, we expect that any potential under- or overestimation errors, depending on whether the preceding day had unusually high or low fluxes, are likely to be balanced out over the course of the year.

Moreover, if the referee's concern relates to diel variation in fluxes, we would like to emphasize that such variation is minimal at our study site, as discussed in lines 124-125 and in our earlier response. Therefore, we consider the use of average daily fluxes to be a reasonable and robust approach in this context.

127: using the biomass to determine the community composition? Does this go together? Biomass depends on species composition but you cannot determine community composition with biomass. Please explain more clearly.

Response: We determined the community composition by calculating the number of shoots for vascular plants and by estimating a percentage coverage for bryophytes (within the collar), which is explained in detail in section 2.4.

133: I do not quite get the marker. Was it a square within which you sampled the plant material?

Response: As stated in the text from line 137 onward: "*...we first selected an area where vegetation heights resembled the heights of the vegetation within the collars. Then, we randomly threw a marker and selected the first ten non-flowering individuals of target vascular plant species close to the marker.*" We did not collect the samples within any particular area but selected the first individuals that could be found near the marker, which in our case was a hat that had randomly been thrown in a particular area where the vegetation was similar to the study site.

134: what is close? Sorry if this goes too much into details, but I do not quite get the randomised sampling.

Response: The sampling was done approximately 50-150 meters from the plots. We have edited the sentence in line 132 to clearly explain this.

134: where were the flowering individuals sampled?
Response: The flowering individuals were sampled similarly to the non-flowering ones. In other words, when we were collecting samples for a species which we needed also flowering shoots from, we sampled both non-flowering and flowering individual simultaneously.

135: see my main criticism: what about the below-ground biomass?

Response: See the response earlier.

153: why did you calculate a ratio? Please explain already here.

Response: We calculated this ratio as it could be a potential parameter in methane modeling. We added an explanation in lines 158–159.

173: just for clarity: you used BM for the calculation of difference (Bray-Curtis etc.)?

Response: Yes.

181: Here I think it would be great to have a visualization of DCA or CCA, especially the CCA in order to classify the potential bias in the data (plots) due to environmental variables. A visual approach would make the results section better understandable.

Response: The visualizations are now available in the main document.

Figure 3: This is a very nice plot. I would suggest to take a fix factor for the second y axis so that the numbers of the y axis are located at a certain horizontal line of the plot. This would make reading the plot easier. And still axis text could be somewhat larger.

Response: We thank the reviewer for this suggestion but, after trying, found that the temperature values as well as the axis title turned harder to read. Therefore, we chose to use the original figure format.

225: here again I would prefer a plot rather than the description.

Response: We provide cluster dendrograms and indicator values for the clusters in appendix B. The clusters can also be seen in CCA graph, which we moved to the manuscript, as well as on a map in the same appendix (Fig. B6)

241: just for clarity: first you are talking about the significant differences tested and visualized in figure 4, than you are talking about the correlation between the clusters and the fluxes in figure 4. To me testing differences between groups and correlation analyses are to different things and in the latter part you are not stating correlation coefficients but the F-statistic. Please make clear, what you did here or reword.

Response: Thank you for pointing this out. To clarify, all statistical tests in this paragraph are based on ANOVA and subsequent post hoc comparisons (see lines 248-250). To avoid confusion with correlation analyses, we have replaced the word “correlate” with “explain” or “show significant effects," which more accurately reflect the methods used.

307-310: In my opinion this description is a bit tedious as it is hard to extract the central message. And the value of 6 mg seems a bit arbitrary. In principle we see this in boxplots. Consider rewording or deleting this part. Or perform and outlier test and see how many of those are C. Rostrata dominated. You point this out much clearer in 351.

Response: The point of this sentence is to highlight that high fluxes were also measured from plots, which were not grouped under C. rostrata-cluster in the clustering analysis but still had *C. rostrata*. The level of 6 mg $CH_4/m^2/h$ was chosen as this level marks the highest level of fluxes (based on LOESS smoothed marginal means) seen in the full dataset (see figure 3). We have slightly edited the text for readability.

313: the root characteristics and belowground biomass should be more stressed here.

Response: We have added a reference to section 4.2 where this topic is discussed.

316: I would presume that the maxima during July and August are strong indications for a dependance on soil temperature and microbial activity or root activity. This is nicely discussed here.

Response: Thank you!

319: whose extent (?). Somehow reads strangely.

Response: Corrected in line 341.

338: consider: methane production and emission

Response: We think "fluxes" is better in this context, as that is what we have measured. We can only speculate on production, as we did not have any concrete data about it.

377: The peat layer: what is exactly meant with peat layer? As water table was mentioned before.

Response: By peat layer we mean the thickness of the peat layer from pear surface to mineral ground. We added "*The thickness of..*" to the start of the sentence in line 402.

384: as you state methane fluxes tend to correlate stronger with soil temperatures in deeper layers (25 cm).
What exactly do you mean with soil temperatures that connect to the water table? And as you measured the soil temperature at 5cm depth and state that these values did not sufficiently explain methane fluxes I think the statement that vegetation (based on this finding) is the prominent influential factor is a bit strong and should be relativised.

Response: We mean that peat temperature at 25 cm depth is more related to the water level depth (and water temperature) than the temperature of the air, whereas the peat temperature at 5 cm is linked to the air temperature. Since our study site is a flow-through fen with rather homogenous microtopography, water level and soil temperatures (at 5 cm depth), we believe that the temperature from 5 cm acts as a proxy for spatial variability of peat temperatures in deeper layers. This is, in fact, one of the main points of this paper to talk about species as an indicator/characteristic of a community, and to show that this system is not dependent on the usual environmental controlling factors.

Report #4

This referee report concerns the revised manuscript. The initial submission was reviewed by two referees; their detailed comments, together with the author responses, have been taken into consideration by this referee, following evaluation of the revised manuscript at 'face value'.

This is a robust and important year-round study of the relationships between vegetation community composition and methane (CH4) fluxes in a boreal rich fen; an ecosystem type for which such data are very scarce. The spatiotemporal variability of CH4 fluxes has been analysed and interpreted based upon 4121 hard-won individual measurements, using a manual closed-chamber approach over 36 study plots year-round. The revised manuscript reads well, and this is an important contribution to the field, highlighting the potential to upscale emission predictions and improve ecosystem-scale CH4 modelling by identifying vegetation-related emission hotspots.

Response: We thank the reviewer for the insightful comments and appreciation for our work. Below we provide detailed responses for the comments.

The manuscript is very strong in its current form, although there are a few remaining aspects which might be worth further consideration to get the most out of this study:

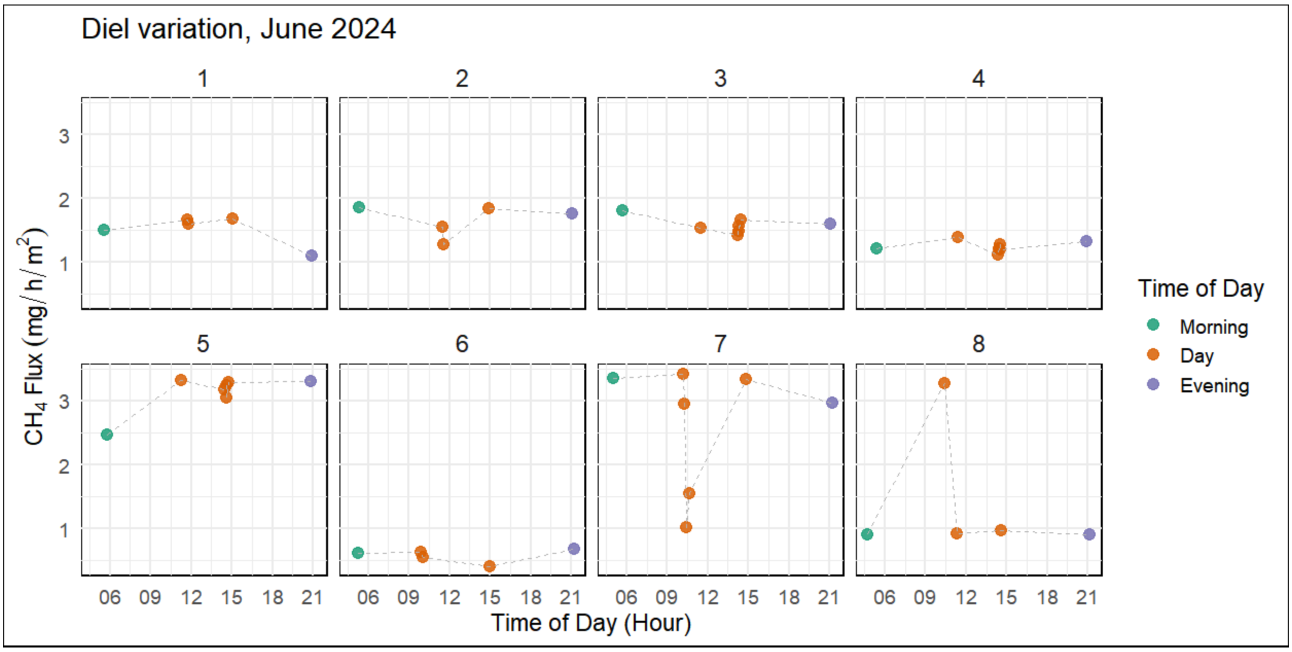
On lines 116-20 the authors state that "*Annual accumulated flux (1.11.2021- 31.10.2022) was estimated by calculating a 24-hour accumulated flux for each available datapoint by multiplying the hourly mean flux by 24. These daily flux values were then summed to obtain the annual total. The days which were missing a measurement were given the value from a previous measurement, assuming the fluxes did not vary remarkably diurnally or over the days*". The assumption that 'fluxes did not vary remarkably diurnally' requires further justification/consideration, in my view (also; should 'diel' replace 'diurnal' – check definitions?). The measurements were taken between 8 am and 6 pm, using a clear polycarbonate chamber. Thus, photosynthesis and stomatal conductance will likely reflect daylight conditions, with open stomata (in living tissues, during the snow-free season). Is it possible, therefore, that extrapolation from day-time measurements to 24-hour flux values could cause a systematic overestimation of daily flux rates, where aerenchymatous CH4 transport is important (I have provided web-links to some potentially relevant papers at the end)? If release of CH4 via the stomatal pathway (as opposed to via leaf micropores and/or the epidermis/cuticle) is potentially important then I think it is worth noting in the manuscript. If no 'around-the-clock' flux measurements are available from this site then this does not undermine the paper; rather, this issue should be noted and discussed. Indeed, based on the results presented here it would be valuable, in any future study, to take some (snow-free season) 24-hour measurements, especially in plots belonging to the Carex rostrata cluster. Note that I am aware of the latitude of Puukkosuo fen (66.377299° N), and the implications for light climate.

Response: In our study site, two automatic chambers recorded fluxes continuously also during our study period. These data provide strong evidence that diel variation in CH_4 fluxes is low at our site. Specifically, during the peak of the growing season and photosynthesis (July), when diel variation (of photosynthesis) is the most pronounced, the average daily standard deviation was 0.758 mg $CH_4/m^2/h$ for Chamber 1 and 0.696 mg $CH_4/m^2/h$ for Chamber 2. During snow season, the average daily standard deviation was 0.518 and 0.439 mg $CH_4/m^2/h$, for chambers 1 and 2, respectively.

Additionally, we have a set of manual 24-hour measurements from eight of the studied plots, which also support the conclusion of low diel variation (see figure below). We have clarified this in the manuscript (lines 124-125) and added a reference to the supporting data (Mastepanov et al., unpublished).

Put together, these findings suggest that our method of estimating daily fluxes by multiplying the hourly mean by 24 does not lead to systematic overestimation.

We also thank the reviewer for pointing out the terminology, and have replaced "diurnal" with "diel" throughout the manuscript for accuracy.



Lines 65-66, 336-37 and 384-85 (the final sentence of the Discussion) state, respectively, "*We hypothesize that (1) the plant community composition affects the methane flux ...*", and " ...*plant functional type and species largely determine the magnitude of the fluxes*" and "*All these findings highlight that vegetation, rather than environmental factors, was the main driver of methane fluxes at our site.*" However, because plant community composition itself reflects (and interacts with) site physicochemical environmental factors, it is important to be very careful with the wording here, and assignment of 'cause and effect'. I would therefore urge the authors to reflect on this one more time, prior to final publication, and consider whether these statements remain robust and objective, or whether some caveats should be introduced. I am not disagreeing with these statements, but plant community composition is not independent of site-level environmental factors, which themselves may influence CH4 fluxes. Indeed, I wonder if the title of the paper could perhaps be amended (slightly!) to "Plant community composition explains spatial variation in year-round methane fluxes in a boreal rich fen"?

Response: We agree that plant community composition is not independent of environmental conditions. We observed that spatial variation in methane fluxes aligned closely with vegetation composition. While we acknowledge that vegetation can act as a proxy for underlying environmental factors, the conditions within our study site—such as microtopography, water level, and soil temperature at 5 cm depth—were relatively homogeneous. This supports our interpretation that vegetation was the primary driver of spatial variation in methane fluxes at this site.

This said, we recognize the possibility that unmeasured environmental variables may also contribute to the observed patterns. To reflect this nuance and maintain objectivity, we have slightly revised the title of the manuscript as suggested by the reviewer.

Related; lines 274-75 state that "*There was no significant correlation between methane fluxes and WTD or soil temperature in any period.*" I found this remarkable, based on Fig. 3, which shows a broad relationship between soil temperature and CH4 fluxes for all vascular plant clusters on a seasonal basis. At the end of the Discussion section (lines 382-84), however, the authors explain that "*methane fluxes did not correlate with peat temperature at 5 cm depth. Indeed, methane fluxes in boreal rich fens associate with deeper soil temperatures, which connect to water table position, rather than with surface temperatures influenced by air temperature (Olefeldt et al., 2017).*" Had soil temperature data been available from deeper in the profile then do the authors consider that they might have been able to detect a relationship between temperature and CH4 flux; or is it solely, as they claim, that "*vegetation, rather than environmental factors, was the main driver of methane fluxes at our site*" (line 385)? Put another way, are the authors confident that this final statement, in the absence of relevant (deeper) soil temperature data, is robust?

Response: As noted in our response to Referee #3, we consider the 5 cm soil temperature to be a reasonable proxy for spatial variability in deeper peat layers, particularly in our study site, which is a flow-through fen with relatively homogeneous microtopography, water level, and soil temperature.

One of the central aims of our study is to highlight the role of vegetation composition as an indicator of methane flux potential, especially in a context where traditional environmental controls appear less variable. While Fig. 3 suggests a general relationship between soil temperature and methane fluxes across clusters, it is important to note that these are averaged values, and such patterns may not hold consistently at the level of individual plots. That said, we acknowledge the possibility that unmeasured environmental factors, such as deeper soil temperatures, could contribute to the observed flux patterns. Nonetheless, we believe our interpretation remains robust within the scope of the available data.

Some more minor points for consideration:

Lines 52-53 - A very bold statement appears here, reliant upon just one reference: "Climate change is predicted to accelerate the natural vegetational succession in boreal rich fens towards Sphagnum-dominated plant communities even in stable hydrological conditions (Kolari et al., 2021)." I would therefore suggest modifying the sentence to "Climate change is predicted to accelerate the natural, autogenic, vegetational succession in boreal rich fens towards Sphagnum-dominated plant communities, even in stable hydrological conditions (see Kolari et al. (2021), and references therein)."

Response: Thank you for pointing this out. We have corrected the reference as suggested in lines 53–54.

Line 135 – Delete the comma, to read " … for those species which were found flowering …"

Response: Corrected.

Line 150 – The units "g/1 %" appear, which in the manuscript font can look like g per litre. I therefore suggest writing this in full; i.e. g per 1%. Response: Good point. Corrected in line 154.

Caption of Figure B7 – correct the spelling of segregated (from segragated). Response: Corrected (now fig. B6).

References relating to CH_4 transport through vascular plants:

https://doi.org/10.1104/pp.94.1.59

https://doi.org/10.1016/0045-6535(93)90430-D

https://doi.org/10.1016/0304-3770(93)90040-4

https://doi.org/10.1016/0304-3770(96)01048-0

https://doi.org/10.1016/j.atmosenv.2003.09.066

https://doi.org/10.1016/j.aquabot.2004.10.003

https://doi.org/10.1046/j.1469-8137.1998.00210.x

https://doi.org/10.1111/j.1469-8137.2012.04303.x

https://doi.org/10.1007/s10533-019-00600-6

https://doi.org/10.1002/lno.11467