General comments:

This manuscript by Järvi-Laturi et al, looks into fine-scale spatial variation in methane flux in a boreal peatland over a full year. By measuring species-specific vascular plant and moss biomass together with chamber-based methane flux measurements, the authors found that increasing sedge biomass, in particular that of *Carex rostrata,* seem to increase methane emissions significantly especially in the snow-free season and full-year scales. Based on their analyses, vegetation composition and biomass seemed to be a stronger driver of methane fluxes than abiotic environmental variables at this site. The authors attributed these results to both enhanced provision of carbon substrates for methanogens and methane transport from soil to the atmosphere.

This is a study that provides the much-needed data and overview of both wintertime and full-year methane fluxes in a peatland, data that still to this day are quite scarce and thus valuable. As the authors mention in the manuscript, regional and global wetland methane budgets contain large uncertainties, some of which are related to the spatial variation in methane fluxes and vegetation composition. Therefore, this study, which looks at small scale (between-plot) methane flux variation, has potential in adding to our understanding of plant-mediated methane emissions in carbon-rich peatland ecosystems. While I see a lot of value and potential in this work, I recommend a list of improvements (major or minor, depending on how biomass measurements were conducted):

Response: We thank you for the valuable feedback and corrections and appreciate the positive comments. Below, we have addressed the comments.

Specific comments:

1. My main criticism is related to the plant biomass measurements and their representativeness of the chamber collars:

   a) It is unclear how you scaled the vascular plant biomass measurements to the collars. You mention that you counted the number of shoots per species within the collar, but did you use this shoot number to scale the mean biomass of 10 samples to the actual collar (or did you actually take average of 20 sample plants if you ignored the fertile/sterile division? See comment 1 c)? If you did not scale these measurements to the collar, you cannot reliably estimate the collar species biomass, and so I recommend to do this and re-analyze your data and fix the results in 3.2, 3.3, 3.4, 3.5 with appropriately scaled biomass values**. If you did do this, please explain this clearly and in more detail in the methods section.**

   Response: We agree that biomass sampling and scaling were not clearly enough reported. There has been a misunderstanding of the methodology which we have now clarified in the manuscript. Below, we explain the process.

   Firstly, we counted shoots per vascular plant species inside each collar and also measured mean height of each species. After this we estimated the percentage coverage of bryophytes. Please, see Table A1 which shows the total number/cover of vascular plants/bryophytes inside all collars and their statistical parameters.

   Secondly, we collected biomass samples for each species from outside the research area to avoid disturbance to the experimental area. These areas located within approximately 50 m range north from plots 46-48 and 52-54 (Fib. B7).

For vascular plants, the biomass sampling was randomized so that we first selected an area where vegetation heights resembled the heights of the vegetation within the collars. Then, we randomly tossed a marker and selected the first ten non-flowering individuals of target vascular plant species close to the marker. We additionally collected ten flowering individuals for those species, which were found flowering inside the collars. With uncommon species, randomization could not be put into practice (*Angelica sylvestris, Carex dioica, Carex panicea, Dactylorhiza* sp.*, Drosera* sp.*, Eriophorum angustifolium, Festuca ovina, Pinguicula* sp.*, Saussurea alpina* and *Viola epipsila*). For these, samples were collected from where they could be found. We believe that our vascular plant biomass samples represent well the vascular vegetation within the collars (Table A1).

For bryophytes, the biomass samples were collected around the experimental area, within approximately 50 m distance from the plots. Sample size was determined by the bryophyte species, being either 5 % or 1 % of the collar area (see species Table A1). The diameter of the 5 % sample was 6.6 cm (three replicates), and of the 1 % sample 2.95 cm (one sample). The sampling locations were selected so that the target bryophyte species could be found as “pure monoculture” as possible.

As we have explained also in the manuscript, the biomass samples were dried and weighed and the dry weights were normalized either by shoot (vascular plant, g/shoot) or by cover percent (bryophyte, g/1 %). Then for each vascular plant species we used the total number of shoots within a collar multiplied by the mean dry mass per shoot to obtain total species biomass for each collar. For each bryophyte species, we used the total percentage coverage within a collar multiplied by the mean dry mass per 1 % area to obtain total species biomass for each collar.

We acknowledge that we do not gain exact biomass values / species / collar with this method. However, we believe this method captures the fine-scale variation between the individuals as both lower and higher plant individuals were sampled for biomass determination and the height variation resembled the variation within the collars.

b) It is also unclear how you took the moss biomass samples and scaled them to the collar. How did you determine which species were “most common” and which were not? What was the percentage cover limit (if there was one)? What was the spatial scale that you used for estimating the “most common” species- was it across the whole study site or within individual collar? If it was across the whole study site, I don’t quite understand the logic of taking a biomass sample equaling to 5 % of the collar area (i.e. 33.025 cm2) especially if the collar had an actual percentage cover <5%, in which case you may have overestimated the species biomass in the collar. Or did all of these “most common” species have >5% coverage in all collars?

If you looked at this at the scale of individual collars, did you take a sample that was equal to 5 % of the collar area per species for the five most common species within each collar and for the rest of the species found within the collar, you took a sample over an area equal to 1% area of the collar (i.e. 6.605 cm2)? What did you do if there were less than five species within the collar? Did every collar really have 10 species within them (now the text kind of makes it sound like there were but it doesn’t seem likely to me)? Please specify this in the methods.

And, most importantly, how did you scale the moss biomass samples to the percentage coverage within the individual collars? As with the vascular plant biomass, if no scaling was done, the moss biomass measurements do not represent the actual collar moss biomass and the data should be re-analyzed with appropriate scaling. Please specify this clearly in the methods.

Response: Our aim was not to determine the bryophyte species by their commonness, and therefore we have removed the mention of this classification from the revised manuscript. Above, we have explained the process of bryophyte biomass sampling in more detail.

c) How did you determine the locations for vascular plant and moss biomass sampling? Were the soil conditions (e.g. pH, soil moisture) similar to the collar? Did you look at and compare the general species composition in the collar vs the plots where you collected the biomass samples (between-species competition could affect some of the plant trait expression and thus biomass), for example by determining percentage cover? How did you decide which plants and moss patches to pick?

For mosses, did you look at e.g. moss stem density in some way to try to estimate the moss biomass in the collar and in the sampling points more reliably than just percentage coverage (the same percentage coverage can represent very different moss biomasses in different collars due to variation in moss stem density and other structural properties)? If not, the moss biomass estimates may be very uncertain and I would recommend discussing these uncertainties explicitly and in much more detail in the manuscript. Given these uncertainties, I would also recommend not to emphasize the ratio between vascular plants and bryophytes as an important methane flux predictor as much as you have so far in this manuscript, or at least combine it with adequate discussion about its uncertainties.

If you did not estimate the similarity (in terms of abiotic/biotic variables) between the collar and the plot where you collected the representative biomass samples, I would be careful making strong conclusions about collar-specific plant species biomass variation.

Response: Above, we have explained how biomass sampling locations were selected in general. Unfortunately, we do not have any soil edaphic data but in locations were the samples were collected, the samples represented the vegetation in the experimental area based on visual estimation. Regarding competition, we refer to the height estimates of vascular plants, which were considered when choosing the sampling locations (Table A1).

With mosses, we prioritized sample purity (i.e., the sample consisted of the target species) and this aim dictated sampling locations and we did not examine any further plant traits. We acknowledge the concern that this may affect the reliability of estimating the biomass ratio of vascular plants and bryophytes and have revised the manuscript accordingly. Although this ratio is not an exact measure, we still think it is a robust indicator of community structure which can be used for e.g. modeling and upscaling purposes.

d) What do you mean by the “fertile” and “sterile” categories for the plant biomass? In my understanding fertile vs sterile categories are used in the context of evolutionary plant biology and plant reproduction (i.e. fertile vs sterile flowers). Or did you use it to somehow determine whether the species had vegetative culms (e.g. for *Carex*) from previous year? It is unclear to me how this classification is relevant to the topic of methane flux spatial variability, especially because you do not talk about these classes afterwards. If you used some kind of scaling for the collar biomass (see comment 1 a), did you take use of these fertile/sterile classes in that as well? Please add a clarification for this separation, what the rationale is behind it, and what you mean by the terms.

It is now also unclear whether the species-specific plant biomass is calculated as the mean of n=20 biomass samples (fertile + sterile) per species per plot, or are the species-specific plant biomasses actually still divided into the fertile (mean of n=10 biomass samples) and sterile (mean of n=10 biomass samples) classes. Based on your results, it seems that you took the mean of 20 samples by combining the fertile and sterile samples? Please add a clarification to your methods.

Response: We have changed the wording from fertile to flowering and sterile to non-flowering. This division was done as the flowering shoots may have higher biomass than the non-flowering shoots. Although this division is not mentioned in the further text, it is used in the biomass estimations – flowering shoots (e.g. n=3 shoots) were first given the mean biomass of the flowering samples (e.g. 3 shoots x 0.02 g/shoot = 0.06 g) and the non-flowering shoots (e.g. n=5 shoots) a mean biomass of non-flowering samples (e.g. 5 x 0.01 g/shoot = 0.05 g). Only after this, the biomasses were combined (0.06 g + 0.05 g = 0.11 g) and used in analyses.

Generally, we have revised the text in methods sections according to comments 1 a-d.

2. Since you are examining the spatial *variation* of methane fluxes within one study site and how plant biomass contributes to this variation (included in your research questions), I would suggest including **additional measures of spatial methane flux variability (e.g., daily or seasonal coefficient of variation or other spatial variation metrics**). This way you could quantify the spatial heterogeneity in methane fluxes which I think is currently lacking in this manuscript. Quantifying the spatial variation would be important background information for showing that there is indeed spatial methane flux variation in your peatland and then go to investigating the contribution of vegetation on it. You already touch on it a bit in 3.1 but only by talking about ranges in mean methane fluxes and visually showing plot-scale variability in Fig. 3 (which are good to show as well but do not really quantify the variation).

   Response:
   We have calculated a CV value for each day that measurements were recorded (19.10.2021-31.10.2022) and the daily CV ranged from 38.9 % to 300.4 % when looking at the whole year. When focusing on snow-free season, the range was 38.9 % - 85.4 %, and for snow season 39.3 % - 300.4 %. The largest differences were mostly from the time of the observed spring burst (1.4.-12.5.2022). We have revised the results section accordingly.

3. Did you measure methane fluxes from one plot once a day or multiple times a day? How did you decide which plots to measure each day when half of the plots were measured per day (n=18 out of n=36)? Please add more detail about this in the methods.

   Response: Only one measurement was conducted per plot per day. On most days only half of the plots were measured, using randomized plot selection. We have revised methods section.

4. 116: The accumulated flux: it is based on a “24-hour” accumulated flux, but, based on your methods, you measured only between 8 am and 6 pm. It would be good to discuss that these accumulated fluxes are based on daytime fluxes and do not include nighttime fluxes, which may lead to the annual accumulated fluxes being quite uncertain. You do mention that you assumed that the fluxes did not vary significantly in the diurnal scale but some justification may be needed here (can you refer to, e.g., the EC data to show this?).

Response: The assumption that fluxes did not vary remarkably diurnally was based on unpublished automatic chamber (AC) data from June 2022 measuring fluxes at the experimental site throughout the day. The AC data showed daily variation ranging from 0.27 mg/h/m^2 to 2.14 mg/h/m^2 during the month. These observations are also in line with the reference provided (Knox et al. 2021) in that way, that the site most similar to our study site (Lompolonjänkkä, FI-Lom) showed very little multiday or diel variation.

5. 118: you mention that you used the value from a previous measurement for days without flux measurements. What was the maximum number of consecutive days where there were no measurements? Methane fluxes (and some plant-mediated methane transport proxies) have been found to vary a lot in daily and multiday scales (see e.g. Knox et al. 2021: https://doi.org/10.1111/gcb.15661).

   Response: The longest gap between the measurements was 28 days between 14.12.2021 and 10.1.2022. Daily mean flux on 13.12.2021 was 2.59 mg/h/m2 and on 11.1.2022 0.27 mg/h/m2. In comparison, the flux on 13.1.2022 was 3.61 mg/h/m2. The value used for these 28 gap days (2.59 mg/h/m2) represents the mean winter fluxes quite well (mean flux between 1.11.2021-30.4.2022 was 2.21 mg/h/m2). Other longer gaps between the measurements were 7 days (n=1), 6 days (n=3) and 5 days (n=3). We have clarified this in the manuscript.

6. 161-162: How did you test the significance between BM variables and methane fluxes using LOESS? LOESS does not test hypotheses, it is used for exploring nonlinear trends (which I believe you did here). You could rephrase this to highlight that (exploring nonlinear trends between BM and methane fluxes between VP clusters).

   Response: We have rephrased the text accordingly.

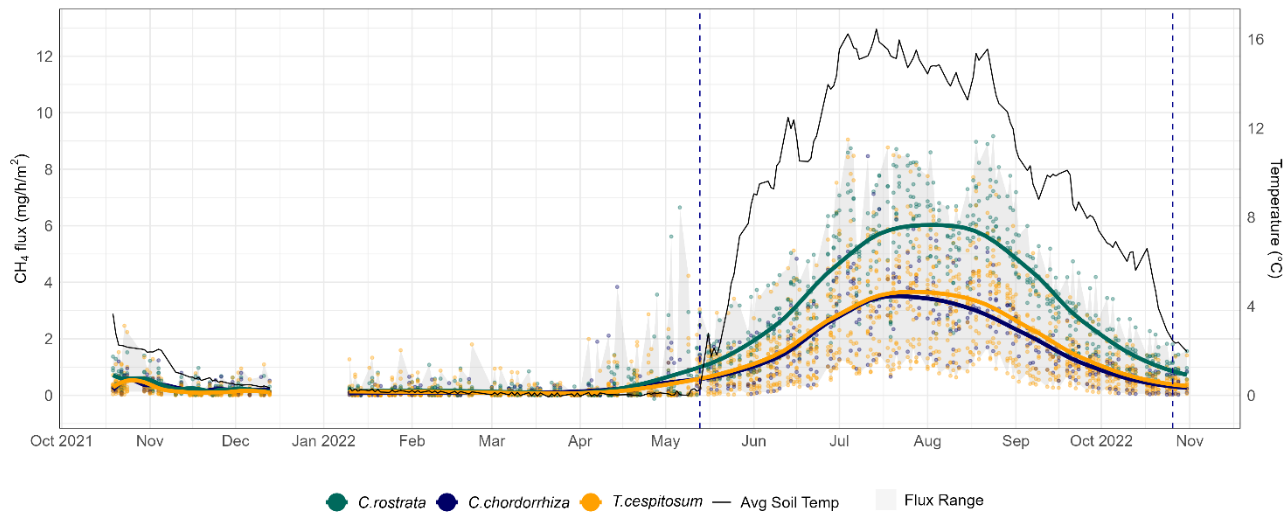
7. 185 (figure 3). It is hard to identify the individual plots based on color in this plot. If they just represent the different plots without considering the vegetation composition within the plots, I don't think the coloring here is needed. You could just replace it with black lines and remove the legend (but mention that the black lines represent the different plots in the caption), for example.

   Also, the plot numbers themselves do not really give any valuable information for the reader. If you want to show all the plot fluxes separately here, the plot numbers could be replaced with something simpler, such as 1-36, to improve the readability of this figure (the current numbering adds more complexity for the reader who is not familiar with your study site).

   On the other hand, if you want to keep the coloring, could you do it based on e.g. vegetation composition grouping (for example based on your vegetation clusters)? If you do this, I would also recommend changing the red color of the soil temperature to black, because the red and green are difficult to separate visually for some readers with color-blindness.

   The axis texts are also a bit small so please increase the font size.

   Response: We have modified Figure 3 as suggested by the reviewer. We chose to combine the content of original figures 3 and 4.



8. 260-263: Could you add a sentence or two about how you would estimate the methane fluxes to change if you had measured them the same way as Alm et al 1999 or similar studies?

Response: Alm et al. (1999) observed that methane concentrations declined linearly within a snow depth profile towards snow surface. This indicates free upward diffusion through the snow and therefore, we would not expect highly changing flux rates even if fluxes were measured on the ground. The measurement would have been more accurate, though, and we could have linked it with the studied plant communities with higher confidence. We have added discussion about this topic.

9. 267-268 (and forward in this section): the plot numbers are not informative for the reader. You could instead describe the dominant vegetation of these plots (e.g. based on the vegetation clusters, which you show in B7)

Response: Revised.

10. 289-291: I understand your reasoning for the uncertainties in the wintertime methane fluxes and plant contributions on them but I would like to see some more in-depth discussion about this based on other studies. How can you make this conclusion based on your data? Depending on the snowpack properties (e.g. porosity) you might have also measured lateral methane flux which did not originate from the actual collar, especially in windy conditions or due to pressure changes between the chamber enclosure and the surrounding atmosphere and snow. Also, I would like to see a better reasoning behind your statement of the plants contributing to the measured winter methane flux even through the snowpack. In theory, this might be possible if there were broken stems or culms that were exposed to the air above the snowpack, which could possibly contribute to the Venturi effect via pressure changes especially in windy conditions but otherwise I am not currently very convinced, especially since you did not find any significant differences between the vegetation clusters in snow cover seasons (Fig. 5).

Response: We understand the critique behind this comment and acknowledge the uncertainties related to our data. However, regardless of the uncertainties, we found a significant correlation between vascular plant clusters and wintertime methane fluxes in multivariate analyses (DCA and

CCA) and, therefore, we argue that vegetation may play a role, and this topic should be acknowledged and potentially studied further.

We further justify our reasoning by referring in the revised version to previous work from other sites and observations from our study site. Pirk et al. (2016) observed that CH_4 flux rates are, firstly, measurable, and secondly, do not decrease during the cold season, which indicates continuous CH_4 production instead of a release of gas reservoirs. They observed a large spatial flux variability in the cold season and discussed that plant species composition, by affecting substrate quality and quantity, could cause these observed differences. In addition, they also observed in some plots a higher gas concentration at snow layers, where plant shoots located. (https://onlinelibrary.wiley.com/doi/abs/10.1002/2016JG003486). In addition, approximately 40 % of *C. rostrata* and *C. lasiocarpa* shoots at our study site overwinter green (Cunow et al., *in prep*). Therefore, it is plausible that gases travel through the aerenchymatous tissues of overwintering shoots, although this topic would require further examination. We have revised discussion accordingly with applicable references.

11. 292-307: I would move a majority of this part to the results section and discuss only the general aspects. Based on your research questions which are about the relationship between vegetation and methane flux, it doesn't seem so relevant to me to discuss the species distribution in such length here. This information would also be more useful in the results section, because then the reader has more of an idea about what kind of vegetation the individual plots contain and where they were located (see my previous comments about plot numbering, possible grouping in figures and naming).

    Response: Text in second paragraph of section 4.2 partly moved to the results section 3.2.

12. 321: does it really provide labile carbon also in winter under the snowpack? Photosynthesis (and thus root exudation) is unlikely that efficient in those conditions, at least to the same extent as in the growing season, and especially so far up north as Puukkosuo where daylight hours are very few. Root decomposition could be one way too (but how efficient is microbial decomposition under the snow in cooler temperatures?) but I would like to see a bit more discussion based on more studies here.

    Response: The assumption that *C. rostrata* "supports methane production year-round" was not related to photosynthesis. Cunow et al. (*in prep*) have studied the belowground processes and phenology of sedges at our site and discovered that approximately 40 % of *C. rostrata* shoots overwinter green. It is also likely that the roots of *C. rostrata* grow deeper than soil freezing depth. These observations support the assumption that the shoots could potentially act as conduits for methane produced in unfrozen peat layers during the cold season, transporting it through the frozen peat layers and snowpack. In addition, methanogenic bacteria have substrates for methane production also during winter and the substrate sources - fresh root litter vs. carbon stocks provided by the perennial sedges - likely depend on plant species. As noted by Pirk et al. (2016), microbial processes continue in the soil throughout the year, even though flux rates in the cold season are much smaller compared to the growing season. (https://onlinelibrary.wiley.com/doi/abs/10.1002/2016JG003486. Given that the soil temperature at 5 cm depth at our site fluctuates around zero during the winter (Fig. 3) it is likely that methanogenic bacteria are also active.

We have revised the discussion section.

13. 342: it might be good to briefly discuss another explanation where vegetation would not be the main driver of the pH changes, and also add a bit more detail into how vegetation actually could have explained the pH variation.

    Response: We did not claim the pH variation to be due to vegetation, but rather the opposite; the variation of pH likely explains the distribution of plant communities through bryophyte appearance. In the text, we note that vegetation composition was the primary driver of spatial variability of methane fluxes, and the significant relationship between these fluxes and pH was potentially explained through vegetation.

    Based on the comments of the other reviewer, we have explored the relationship between methane fluxes and environmental factors a bit further and added some information about the role of pH in the text in section 4.4.

14. 344-346: This is an interesting finding and warrants a more detailed discussion. What do the other studies say and how could these theories apply to your study? Could the higher NO_3^- and NO_2^- concentrations contribute to pH or vegetation in some way that would enhance methanogenesis?

    Response: We have revised discussion, highlighting the debated role of nitrate and nitrite in methanogenesis. We also added a finding that the positive correlation was not found when analyzing the environmental variables and methane fluxes in plots with no *C. rostrata*.

15. 349: this could indeed be the case but, to support this argument, you could also add a number to represent the lack of strong temporal variation in WTD (e.g. standard deviation or coefficient of variation if you want to compare growing vs non-growing season variation for example).

    Response: We have WTD data only from the growing season, so comparing the values throughout the year is not possible. However, we have added the number of standard deviations in the revised version.

16. 350-353: Two points:

    a) The correlation between peat depth and plant biomass makes sense in the biological sense that, when there is more peat, there is also more space for roots especially for more deeply-rooting vascular plants. Since you did not find significant correlations between peat depth and methane fluxes, I would be careful drawing strong conclusions about the influence of peat depth on methane fluxes via vegetation (but see my next point).

    b) On the other hand, it is also possible that in the presence of deeply-rooted aerenchymatous vegetation, such as *C. rostrata*, the roots may provide labile carbon substrates in deep peat where methanogenesis increases despite the dominance of recalcitrant peat (i.e. indirect influences of peat depth on methane fluxes). The release of labile carbon compounds via root exudation could also trigger microbial carbon priming (see e.g. Waldo et al 2019: https://doi.org/10.1007/s10533-019-00600-6). However, be careful about your interpretations about the wintertime vegetation influences based on your data (see previous comment about wintertime fluxes), and keep in mind that the direct relationship between peat depth and methane flux was still nonsignificant.

Response: We wanted to state that there was a significant relationship between peat layer thickness and the first ordination axes of vascular plant data in CCA and a positive pairwise correlation between peat layer depth and the biomass of vascular plants, sedge and *C. rostrata*, and that these latter three were all proxies for higher methane fluxes. This suggests, firstly, that these vegetation parameters associate with deeper peat and, secondly, may support methanogenesis through indirect relationships. We have revised the manuscript for clarity.

17. 354: Please add more discussion about why soil temperature may not have correlated significantly with methane fluxes- soil temperature has been an important predictor of methane fluxes in multiple studies and discussing this opposing result would be warranted.

    Response: Olefeldt et al. (2017) have found that methane fluxes in boreal fens are associated with soil temperatures at greater depths rather than with surface temperatures. As we only recorded temperatures which reflect the surface temperature, our results are in line with the Olefeldt et al. (2017). We have discussed the lack of correlation between soil temperature and methane fluxes in discussion.

Technical comments:

- 30: Add “(CH4)” after “methane”. You could also replace the rest of the “methane”s with “CH4” if you want, especially since you use it in the flux units throughout the paper.
    - Response: Corrected. In text we prefer using the word “methane”, as it is commonly read out as a word instead of the chemical formula 'CH_4'.

- The word ”dynamics” is used quite a lot throughout the introduction. I would recommend changing it to something more specific, as in some cases (e.g. “methane dynamics”) it may sound a bit vague.
    - Response: We have replaced word “dynamics” with more specific terms throughout the text.

- Generally through the whole manuscript: the term “year-round” doesn’t sound very good to my ear. How about “full-year”?
    - Response: We have carefully chosen the phrase “year-round” because it clearly expresses the nature of our data – continuous, frequent measurements across an entire year. This term highlights that the data is not aggregated into an annual sum but rather presented as a time series of spatial variation. Therefore, we believe "year-round" is the most appropriate term and prefer to retain our choice of wording.

- 32: Saunois et al have a newer global methane budget paper (currently a preprint): https://doi.org/10.5194/essd-2024-115
    - Response: Reference updated.

- 34: instead of using the word “spatial and temporal dynamics”, maybe “spatiotemporal variation” or something similar would be better?
    - Response: Corrected with “spatiotemporal patterns”.

- 39: “ecosystem process” – maybe use another word, for example “These ecosystem-level processes…”
  - Response: Corrected.

- 42: remove “layers” after topsoil, and add why rising temperatures lead to increased topsoil oxidation?
  - Response: It is probable that increased temperatures enhance microbial activity and oxygen availability (Zhang et al. 2021). We have removed “layers” and added this clarification.

- 45: I would change the topic sentence to something shorter. Perhaps remove mention of hydrology and just start with “Vegetation type and its responses..”
  - Response: Corrected.

- 47-48: maybe change the words “deeper” and “upper” to “anoxic” and “oxic” (this way it would focus on methane being transported from anoxic soil through the oxic soil and into the atmosphere)
  - Response: Corrected.

- 49: I would be careful with the wording “better than any abiotic factor”- please add more references, or modify the sentence so that it doesn’t sound so definitive
  - Response: Edited the sentence to *“Indeed, plant species and their specific traits have been found to be **reliable** predictors of methane flux rates (Korrensalo et al., 2022).”*

- 59: the part “extensive, year-round, plot-scale flux data are, however, limited” sounds a bit complicated. Maybe something like “However, ... full-year methane flux data at the plot scale are limited”?
  - Response: We understand it is a complex sentence and will revise it.

- Methods: the model numbers could be written in parentheses after the instrument, e.g. at row 99 you could put the LI-COR model number “LI-7810” in parentheses after mentioning the instrument. You already do this in the 2.5 section so it would be good to keep it consistent.
  - Response: Corrected.

- 60: would “..spatial variability in methane fluxes..” work better?
  - Response: Corrected.

- 71: what is “normal period”?
  - Response: A "normal period" refers to a climatological standard normal. This is a period used to calculate average climate conditions, spanning over 30 years. The 30-year period used for calculations is stated in row 71.

- 73-74: please add a detail saying where the pH was measured (peat I assume?)
  - Response: pH was measured from peat pore-water, which is mentioned in the revised text.

- 76: was the variation standard deviation or other measure of variation? Or do you mean that 6.3 cm was the mean WTD during the study period? Please specify.
  - Response: The plot-scale variation in WTD during the snow-free season of 2022 was 3.8–9.1 cm with an average of 6.5 cm. This is corrected in the revised version.

- 77: graminoids are herbaceous plants so this sentence should be changed accordingly (you could, for example just call them "vascular plants typical of rich fens" and then give the species examples)
    - Response: Corrected.

- 91: The figure caption could be made even simpler, how about just starting from: "A map of Puukkosuo rich fen.."?
    - Response: Corrected.

- 95: You could remove the mention of "manual" here since you introduce it later in this paragraph.
    - Response: Corrected.

- 97: this sentence ("…, doing measurements from half (n=18) of the study plots per day") could be made smoother, for example just: ".. from half (n=18) of the study plots per day".
    - Response: Corrected.

- 105: the end of the sentence starting with "making the possible dilution.." is a bit hard to understand, could you make this a bit clearer? Do you mean leakage? Good that you mention this though.
    - Response: We purely meant any effects that the snowpack may have on the magnitude of the flux, without specifying exactly what they could be. We have revised the text accordingly. The uncertainties related to this measurement technique are further discussed in section 4.1

- 108: what exactly do you mean by "successful"? Visible linear increase in CH4 concentration?
    - Response: We have revised the text to more clearly state that we only 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).

- 115: it is a bit unclear now how you determined the snow cover- did you define it snow-free when there was snow but you were able to set the chamber on the collar? For transparency, it might be good to add this detail here.
    - Response: We defined the seasons by the ability to measure the fluxes of all the 36 plots on the collar. We have revised the text accordingly.

- 145: do you have more details of the pH analyzer, other than the brand?
    - Response: Details for the pH analyzer (913 pH/DO Meter, Metrohm) were added to the text.

- 146-147: write the numbers in the molecules in subscript (e.g. NH_4)
    - Response: Corrected throughout the manuscript.

- 148: please add that you estimated the litter cover as a separate percentage cover, if this is the case. This could also be actually mentioned already in the plant community data where you talk about moss percentage cover.
    - Response: We consider litter cover as a separate environmental variable, not included in the plant community data, and would rather keep the mention of it only under section 2.5. We have edited the text to clarify the estimation process.

- 151: "VP" abbreviation appears here for the first time but you don't introduce it before this. Please add the abbreviation to the appropriate spot in the text (maybe introduction?) so you can then start using it: "vascular plants (VP).."
    - Response: The abbreviation is introduced earlier in section 2.4. We will inspect the consistency of the use of abbreviations.

- 181: put the "4" in "CH4" in subscript
    - Response: Corrected.

- 204: add the name of the statistical test you used to obtain the F-values ("F=..") for the first occurrence of the letter.
    - Response: The name of the statistical test (ANOVA) added.

- 206 and forward: write the species names in italics and I would also write the complete names, e.g. "*C. rostrata*". It would improve the readability if you wrote them in full form (the genus does not have to be written out since you have already discussed the species before).
    - Response: The original idea was to separate individual species from the plant community clusters indicated by these species. We understand that this might have caused unclearness in the text and have therefore corrected the unclarity related to this issue starting from section 3.2, where the clusters are first described.

- 215 (figure 4): please increase the axis text font size, and consider writing out the species names in the legend. Caption: replace "dot in the graph" with "data point". Based on this plot, it also seems that there might be another plot group or cluster in T. ces where there are lower fluxes (the lower yellow point cloud which I would imagine could lead to a different smooth curve? Did you look into this? What might contribute to this trend? This is a bit extra but maybe worth discussing and/or looking into.
    - Response: We have updated the figure, and the caption based on the reviewer's comments. We chose to combine figures 3 and 4 into one graph. While the plot gives an impression of a possible fourth cluster showing lower fluxes during the peak season, the cluster analysis for vascular plants didn't imply this. Please, see cluster dendrogram shown in Figure B2.

- 220 (figure 5): 1. increase the font size for axis texts. 2. Even though you list them in the caption, I would still write out the whole species names instead of the abbreviations in the plot. 3. It is very hard to see the median line in the dark blue boxplots so changing the color to something lighter might help readability. Also, even though the colors look nice, are they really needed here since you also give the same information on the x axis as cluster names? Or, if you would prefer keeping the colors, you could also consider removing the legend and in the caption write something along the lines of "the colors represent the clusters and are shown for clearer visualization".
    - Response: Figure edited mostly as suggested. To keep the figure clear, we chose to use the abbreviations of the species names in the plot instead of full names.

- 225: to remind the reader what these are, please add the term before "DCA" and "CCA" abbreviations: e.g. "detrended correspondence analysis (DCA)".
    - Response: Corrected.

- 226 forward: you could write the species names in complete forms here.

- Response: Corrected. We want to emphasize that we have explained earlier in the text that these cluster names refer to plant communities indicated by these species, rather than purely the abundance of these individual species.

- 243: would something a bit more specific be better instead of calling the ratio “BM ratio”? For example, “VP:BRYO ratio”? The reader might forget what exactly “BM ratio” consists of and would need to come back to the definition of this term.
  - Response: We have revised the text and the abbreviation in Figure 5 (scatterplot).

- 245: add “p” to the second p-value: “and $p \leq 0.01$”.
  - Response: Corrected.

- 250 (figure 6): increase the font size of axis texts and write out the species names in italics.
  - Response: Figure edited partly as suggested. Additional scatterplots added to the figure to further show the significant biomass and environmental variables affecting snow-free season methane fluxes.

- 257: move the Jammet et al reference to the end of the sentence, and if possible, try to find another reference here since you mention multiple northern fens. Or you could just say “.. in a northern boreal rich fen (Jammet et al. 2017).”
  - Response: We chose to refer only to Jammet et al. (2017) as it was from a site most similar to ours.

- 256-260: I think these sentences should be in the results section and not in discussion. For example in the 3.1 section.
  - Response: Part of the text from beginning of section 4.1 moved to section 3.1.

- 264: is this percentage based on your results? If yes, please indicate so in this sentence, and if not, add a reference.
  - Response: The percentage is based on our findings (2.3-21.3 %) and the findings of Alm et al. (1999) (6-17 %) and this has been clarified in the revision text.

- 274-275: plant traits are part of vegetation, so you could rephrase this by for example: “…could not be explained by aboveground plant biomass..”. Also, give examples of these plant traits, as well as the “ecohydrological aspects” and microbiota, and how they might contribute to the spatial variability in CH_4 flux between the plots.
  - Response: We have corrected the first point as suggested (row 306). Also examples about plant traits, such as rooting characteristics, ecohydrology, such as peat water holding capacity, and microbial metabolic interactions, such as nutrient cycling, as well as their contribution to spatial variation in the fluxes (contribution to soil conditions, substrate availability, and microbial activity) have been added to the text.

- 315-316: replace the "organic matter“ with "carbon substrates", and replace "and providing pathways” for example with: “.. for methanogenesis through deep root systems throughout the year”.
  - Response: Corrected.

- 318: add “methane” in front of “transport”: “..may be due to the species’ high methane transport rate..”
    - Response: Corrected.

- 318-319: why would *C. rostrata* have low oxidation potential in your study? As you say next in this sentence, this species has high root porosity (so it could also oxidize the rhizosphere), so why would the methane transport exceed the effect of methane oxidation in your study? Clarify briefly.
    - Response: The assumption is based on a previous study by Ström et al. (2005) where *C. rostrata* was observed to have a much lower capacity (20-40 %) to oxidize the rhizosphere compared to two other species (*Eriophorum vaginatum* and *Juncus effusus*) (>90%). We have edited the text to highlight the contrast between the plant traits and low oxidation potential.

- 319: saying both “high porosity” and “large aerenchyma” is not needed as they refer to the same thing. You could instead just say “.. and high root porosity”.
    - Response: Corrected.

- 325: this is a bit vague sentence. How about: “Thus, VP:Bryophyte ratio could be used as a parameter in peatland methane flux models together with remotely-sensed data products.” (But see comment 1 c)
    - Response: Corrected.

- 330: add “gas” “high transport efficiency”: “high gas transport efficiency”
    - Response: Corrected.

- 332-333: This sentence is a bit unclear. Do you mean that *C. rostrata* had more shoots and therefore plots with more *C. rostrata* shoots transported and emitted more methane?
    - Response: Due to the species' high transport efficiency, even a few shoots can release the total flux magnitude from the ground. We observed saturation in the magnitude of the flux, rather than a linear increase with higher biomass of the species. This indicates that only a few shoots of *C. rostrata* are needed to release the methane stored in the soil. We have revised the text.

- 334: add “methane” to “transport efficiency”: “methane transport efficiency”
    - Response: Corrected.

- Figure 7: move this to the results? And increase the axis text font size and write out the species names in italics.
    - Response: Corrected.

- 359: remove the mention of “causality” because you did not use methods for estimating causal relationships in this study.
    - Response: Corrected.

- 361: remove “answer our first research question and”, and replace “affects the flux” with “affects methane flux”.
    - Response: Corrected.

- 363-364: remove “answer our second research question and”
    - Response: Corrected.

- 368: you didn’t really discuss plant traits in the discussion part, so I would remove the mention of plant traits here. Or, you could say for example: “Our findings suggest that, in addition to species-specific plant traits, the biomass ratio of vascular plants and bryophytes could potentially be used as a parameter for predicting peatland methane emissions” (but see comment 1 c).
    - Response: Corrected.

- 373: I don’t think you have to show the reference at the end. The closing sentence would be stronger without it.
    - Response: Corrected.