

Plant community composition controls spatial variation in year-round methane fluxes in a boreal rich fen

By Eeva Järvi-Laturi *et al.*

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.

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.

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”?

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?

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).”

Line 135 – Delete the comma, to read “ ... *for those species which were found flowering* ...”

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%.

Caption of Figure B7 – correct the spelling of segregated (from segragated).

References relating to CH₄ 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/10.1016/0045-6535(93)90430-D)

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

[https://doi.org/10.1016/0304-3770\(96\)01048-0](https://doi.org/10.1016/0304-3770(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>