Review of:

A full year of continuous net soil and ditch CO2, CH4, N2O fluxes, soil hydrology and meteorology for a drained fen in Denmark

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This data paper is a useful contribution to the study of soil greenhouse gas fluxes in peatlands, because it presents data from a novel system of automatic chamber greenhouse gas measurements covering a transect with a large number of collars, a relatively high data frequency and a full year coverage. However, I have two serious objections to this paper that should be addressed properly before final publication. Therefore I recommend publication, but with major revisions. My main objections are:

1. A lack of ancillary soil and vegetation data. There is practically no data on soil and vegetation included, except for a short mentioning of peat soil. Neither it is clear which of the cited references gives adequate information on soil conditions of the transect. If these data are to be be used by other researchers on ecosystem greenhouse gas fluxes, one would at least expect a basic description of a soil profiles at the site, or a borehole transect, and some basic soil and water chemistry data.

2. The measurement procedure, that entails removal of vegetation. The procedure of removal of vegetation has been common in the past but is increasingly abolished because of the intense intertwining of vegetation, microbial community and soil processes that generate greenhouse gas fluxes. Removal of vegetation (including the application of herbicide in this case) is a large disturbance of this system, with questionable results. It introduces artefacts that are poorly quantified for CO_2 fluxes since labile carbon pools in the soil are affected.
In the case of CH_4 fluxes, the main supply of labile carbon for methanogens is reduced, and the main transport pathway of CH_4 from soil to atmosphere (by plant aerenchyma) is destroyed. Therefore it likely leads to much lower fluxes of CH_4 compared to those in an undisturbed system, resulting in data that cannot be compared to those of other sites – if not simply flawed. For details, see the comments below.
The authors should state clearly in the abstract that vegetation removal has been applied. Furthermore, they should discuss properly what effects this may have had on the fluxes that they have measured.
Even if this is a data paper only, reflection on the complexity of the system that you have measured, and on the effects of your measurements on that system, is necessary.

Minor comments concern mostly the quality of figures and their captions, and questions on the operation of the automatic chamber system.

Detailed comments on the paper.

Section 2.1 Site description: This is disappointingly incomplete. Not any information is given on the soil profile and it lateral variation along the transect, while this could have been checked with a few hand augerings. What is the peat stratigraphy, are there any sand or clay layers in between or on top of the peat? What is the peat type, its decomposition grade, loss on ignition? Any information about soil water chemistry, for instance the presence of anaerobic electron acceptors that influence the redox potential and methanogenesis? What is the variation of the vegetation along the transect? *Juncus effussus* and most grasses differ strongly in the characteristics of their root system and methane transport characteristics. All this is information that any user of your data would want to know.

Line 94 – 101, caption Fig. 3: What are the instruments in the lower right corner of the figure?

Figure 4: The figure is not very informative (except on the surface topography and placement of the collars) and the caption is confusing. The vertical profile has two colours, brown and dark grey, which suggests some sort of stratigraphy. However, the brown colour is marked as 'transect surface', but apparently it indicates the soil above the minimum water table depth. The lower depth of the peat is not indicated. Information on the peat properties and its variability (e.g. the presence of clay/sand layers) is lacking. This would be very useful information for users of the data.

Line 170 – 173:
This procedure of killing vegetation by harvesting, and application of a herbicide, attempts to reduce the effects of vegetation respiration and to measure the 'true' or 'net' soil GHG flux. However, it introduces other artefacts that are poorly quantified, in particular for the CH_4 fluxes. For measuring of CO_2 fluxes from the soil it often has been done with the purpose of reducing CO_2 respiration/uptake by plants. Because of the artefacts it introduces, alternative approaches have been developed that leave vegetation intact and separate soil and vegetation components of the flux by modelling (e.g. Boonman et al., 2024). For the CH_4 fluxes it may have resulted in serious underestimation of the fluxes.
The statement that the fluxes after removal of the vegetation represent the 'net' soil greenhouse gas flux is invalid without specification what is actually meant by 'net flux', in particular when it is not explained which soil carbon pools are assumed to contribute to to this net flux. It may at best approach the soil CO_2 flux (with an unknown error or bias) and likely severely underestimates the CH_4 flux. In general, CO_2 from the decomposition of recently produced labile carbon, and that from older soil carbon (e.g. the peat carbon pool) is difficult to separate in surface flux measurements. Vegetation removal does a poor job in that, because most the root mass often remains behind, will be active, and also affects the microbial population.
Besides these caveats, I also wonder if vegetation removal is a specific requirement of this automatic chamber system. Does vegetation hamper a leak-free placement of the chamber on the collars with this system?

A motivation why this procedure has been applied, and a discussion of the caveats listed below is necessary. I suggest to do this in a separate Discussion section. Also, the vegetation removal procedure itself should be mentioned clearly in the abstract, for potential users to judge wether the data are suitable for use.
Drawbacks of vegetation removal:
1. The soil greenhouse gas flux in an undisturbed ecosystem is the sum of all peat and other organic matter decomposition. The 'other' being various forms of recently produced, usually labile carbon, produced by the vegetation root system and litter decomposition. Removal or decrease of one carbon pool may strongly affect the measured fluxes, in particular when it is not known quantitatively what has been removed. Furthermore, labile carbon interacts also with stabile carbon decomposition via the priming effect. This may enhance stable carbon decomposition (e.g. peat decomposition) in the presence of labile carbon. Therefore, it should be specified which carbon pools are considered to be included in the flux measurements (peat, older humic matter, recently produced organic matter, labile or stable?), and what effects the measurement procedure has on CO_2 emission from these pools. If actual data collection, e.g. root mass, is not available, the authors could at least consult literature from other sites on that.
2. Glyfosate is known to affect soil faunal and soil microbial respiration (e.g. Nguyen et al., 2016). The application of this herbicide will have influenced the measured fluxes to an unknown extent.
3. As the need for very frequent removal of living vegetation during the experiment testifies, the root system in the soil remained active, producing labile carbon and adding a vegetation and labile carbon respiration component to the fluxes. Therefore, vegetation removal still does not remove vegetation effects.

4. Since detection of CH_4 emissions is included, you are removing one of the main transport mechanisms of CH_4 from soil to atmosphere: the transport via plant aerenchyma (e.g. Vroom et al., 2022). Moreover, the main source of carbon for methanogens is labile carbon compounds produced by plant roots. The low CH_4 emissions therefore may be flawed and not represent normal ecosystem or soil CH_4 fluxes. On peat soils with approximately similar water table variation and vegetation, significant positive CH_4 fluxes were measured with manual and automated chambers (Hendriks et al., 2007; Lippmann et al., 2023).

Furthermore, you say that you removed vegetation with a minimum of 7 days. What was the (probably higher) vegetation removal frequency in the spring and summer period? This is highly important given the rapid vegetation regrowth in that part of the year.

Line 179: How does wind speed affect the operation of the system? How reliable is it at higher wind speeds?

Line 180: How certain can you be that rapid vegetation growth near the collar does not affect the airtight connection of chamber and collar? For instance, leaks may result from high grass getting between the chamber and the gasket during windy conditions.

Line 182: If a fan was not installed in the chamber, what is the air flow provided by the main pump, and is it sufficient to flush the chamber?

Figure 5: Can the upper photos be made sharper or larger, providing more detail on the chamber construction? Eventually, provide them in a supplement.

Line 232: Check the sentence starting with “If the relative SE…”, the part “than 100%” appears to be misplaced.

Line 238: During the measurement time the temperature in the transparent chamber can rise in a matter of a minute in sunny weather. Is there no temperature sensor inside the chamber to detect this effect and correct for it?

Caption Fig. 7. Good point, but how sensitive is the system to bubble fluxes induced by the chamber lowering? Was the collar anchored somehow in the subsoil of the ditch to prevent disturbance?

Line 383: variability in soil water content. Again, it is disappointing that so few soil data is included. Could there be differences in water seepage from higher ground (which is to be expected given the topography) or does soil cracking in dry periods occur, influencing the SWC?

Line 398 – 402. Again, the large spatial variability should be no surprise. Unfortunately, any information on soil variability is missing. For instance, I would have expected information on the soil carbon content, which is an important predicting variable in CO_2 fluxes from soils. Although the above-ground vegetation is removed, there is still root mass present that produces labile carbon; root density also adds to the spatial variability. This kind of data would be very useful for other users of the data.

Line 424. Interesting to see these bursts. As mentioned above, this could also be an artefact of your methodology. It excludes plant emissions, which is usually a major CH_4 emission pathway (Vroom et al., 2002). By artificially removing the plant flux, a buildup of CH_4 concentration in the soil could induce a burst-like emission pattern.

Figure 16 and Figure 19. This figure is difficult to understand, information in the caption is ambiguous.

You have only 5 measurement points per day, but there are more observation points. The caption suggests that the points are based on a grouping of all soil collars together. Is this over one day, if so, which days? Or over an entire month, as the reference to the figure legend suggests? In that case I would have expected way more data points in the figure, unless it is a monthly average per collar. In short, be clear how your data have been grouped. This is also important for understanding sources of variation in the data. Grouping of all collars in one day introduces also spatial variation, next to temporal variation.
Next, the colour scale of the legend is not very distinctive by choosing only shades of blue and red. Better include other colours as well, which makes the data from different months more distinctive.

Line 443-444. The lack of diurnal variation for CH_4 may also have been caused by removing the vegetation. Plant fluxes of CH_4 tend to have diurnal variation, driven by solar radiation (Vroom et al., 2022).

Line 540. ‘it cannot be ruled out that living roots inhabited the soil below the chambers’. You can be quite sure about that if you have to clip the vegetation frequently!

Line 547-550. There could be other electron acceptors inhibiting methanogenesis, for instance Fe^{3+}, sulfate. Here again, some information on basic soil and water chemistry could have been helpful for users of the data.

Table 3. Mention the source of the GWP factors used here.

Section 5 Conclusions: The causes for spatial variability of the GHG fluxes is unresolved – but that is not surprising given that any information on soil variability is lacking. Fig. 15 suggests that the spatial variability is larger than the temporal variability on these closely spaced collars, which would be an interesting conclusion.

Line 578-580: The low CH_4 emission is attributed to low water table and a cold wet winter. However, the huge elephant in the room here is the potential effect of vegetation removal on the CH_4 fluxes detailed above. At other peat sites with similar water table and vegetation that I have measured myself, persistent positive summer CH_4 fluxes occurred (Hendriks et al., 2007; Lippmann et al., 2023). Neither, alternative explanations for the low emissions are considered, such as the presence of other anaerobic electron acceptors (e.g. sulfate reduction) that maintain a too high redox potential for methanogenesis? This is mentioned elsewhere in the article for NO_3^-, but not considered here.

Supplement.

Missing: collar numbers at each graph. What do the ticks on the horizontal axis represent? First day of each month, midpoint? Day numbers would be more informative on the horizontal axis!

Data.
The data representation is largely correct. However, for greenhouse gas fluxes it would be useful include the standard error of the flux calculation method that is applied. This would allow data users to apply additional quality checks.

References.

Boonman, J., et al. (2024):Transparent automated CO2 flux chambers reveal spatial and temporal patterns of net carbon fluxes from managed peatlands. *Ecological Indicators*, *164*, 112121.
Hendriks et al., (2007): The full greenhouse gas balance of an abandoned peat meadow. Biogeosciences, 4(3), 411-424
Lippmann et al. (2023): Peatland-VU-NUCOM (PVN 1.0): using dynamic plant functional types to model peatland vegetation, CH_4, and CO_2 emissions. *Geoscientific Model Development*, *16*(22), 6773-6804.
Nguyen et al. (2016): Impact of glyphosate on soil microbial biomass and respiration: a meta-analysis. Soil Biology and Biochemistry, 92, 50-57.).
Vroom et al., (2022): Physiological processes affecting methane transport by wetland vegetation, Aquatic Botany 182:103547

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June 2025