

Second review of:

**A full year of continuous net soil and ditch CO<sub>2</sub>, CH<sub>4</sub>, N<sub>2</sub>O fluxes, soil hydrology and meteorology for a drained fen in Denmark**

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Author: We would once again extend our gratitude to all three reviewers for taking your time to further improve the manuscript with your insightful and constructive comments and suggestions. We have addressed each comment below and the second review have resulted in substantial changes to the structure of the manuscript. As per suggestion of Judith the Materials and Methods section has been rearranged to be less redundant. All time series figures have been edited to streamline time format and the conclusion has been restructured. Furthermore, where relevant and suggested we have made changes to the text.

**Report 1 by Ko van Huissteden**

After my first review of this paper, I see that the authors have done an excellent job in improving the paper. Important ancillary information on the soil profile and water chemistry of the site has been added, which improves the usefulness of the greenhouse gas measurements considerably. The drawbacks of the measurement system and the procedure of vegetation removal have been discussed properly, albeit that the effects of vegetation removal still cannot be quantified due to the lack of a control experiment, as is admitted by the authors. However, such a quantification was not the goal of their measurements. Representing greenhouse gas measurement data from a novel chamber measurement system is in itself a very useful contribution to the research field.

Author: Thank you for this comment. We appreciate that our efforts to improve the manuscript are acknowledged.

A few smaller matters remain, which could be tackled by minor revisions.

- Effect of vegetation removal on CH<sub>4</sub> emission (reply by authors, page 18, point 4; revised text, line 459 – 465). Here, the authors first state that the effect of vegetation removal on the CH<sub>4</sub> flux might not be very strong because of root growth from outside into the collars, and second, the lack of labile carbon supply would not have a large effect during periods of lower water table. However, the labile carbon supply to methanogens is hampered mainly by green vegetation removal, root mass effects are secondary. Photosynthesis is the actual source of labile carbon products, which may be transferred to the soil via the roots in a matter of hours to a couple of days. This has been proved by carbon labeling experiments, see e.g. King and Reeburgh, 2002 (King, J. Y., & Reeburgh, W. S. (2002). A pulse-labeling experiment to determine the contribution of recent plant photosynthates to net methane emission in arctic wet sedge tundra. *Soil Biology and Biochemistry*, 34(2), 173-180).

So, by removal of green vegetation inside the collars you will inevitably cut off an important labile carbon source. Neither does the argument of lower water table hold. Roots of wetland plants such as sedges and *Juncus* can penetrate quite deeply and still add labile carbon to completely saturated soil, fuelling methanogenesis. In addition roots and stems will continue to transport some of the CH<sub>4</sub> towards the atmosphere, bypassing oxidation within the unsaturated topsoil. Therefore, the effect of vegetation removal on the fluxes will be considerable, and may occur even in drier periods with lower water tables.

Author: Based on this comment we realize our argumentation may have been too simplistic and have modified this text to reflect the above mentioned processes (lines 458-461): “However, as we excluded plants from the collars we might have decreased the net emission of CH<sub>4</sub> directly by restricting gas transport in aerenchyma from deep peat layers potentially sustaining net CH<sub>4</sub>

emission even though the observed growing season WTD was 20-40 cm (Askaer et al. 2011; Vroom et al. 2022) and indirectly by potentially reducing plant carbon supply to methanogens.” Furthermore, we have deleted the text: “However, visible inspection at the site confirmed lateral root growth from vegetation adjacent to the collar. This could indicate that plant derived C and N was still available for microbes underneath the collars, but the impact on gas transport is uncertain. However, we did not excavate roots during the study to avoid excessive disturbance. Furthermore, considering that the WTD in the growing season was mostly 20-40 cm below terrain the potential for CH<sub>4</sub> production in the topsoil would be limited (Koch et al. 2023).” Finally, we have modified the text in lines 461-463: “The lack of consistent hot moments of CH<sub>4</sub> emissions and low cumulative emissions during periods of shallow WTD in the growing season (Fig. 6A-F), potentially conducive CH<sub>4</sub> production, could indicate that redox potential is elevated due to presence of other electron acceptors.”

- 7-day period of green vegetation removal (reply by authors, page 19, top paragraph). “Furthermore, we never observed net CO<sub>2</sub> uptake in the growing season indicating that the vegetation removal was effective.” This is flawed reasoning. To my experience there can be a significant regrowth of vegetation in a few days during the growing season, resulting in a measurable CO<sub>2</sub> uptake that reduces the measured net flux. The fact that you never observed net CO<sub>2</sub> uptake, does not mean that your CO<sub>2</sub> fluxes are not influenced by photosynthesis of the small amount of leaf and shoot regrowth that may occur within seven days. This photosynthesis might not be able to overcome the soil CO<sub>2</sub> flux, but still will result in a reduction of the measured total flux. This should be mentioned in the text.

Author: We agree that the net flux we measure may be positive even though there is photosynthesis. However, if there indeed was a significant regrowth of plants that would photosynthesize and hence decrease net CO<sub>2</sub> effluxes, we should expect to see a systematic decrease of net CO<sub>2</sub> effluxes over 7 day periods during the growing season. This was never observed. We do not have photos to document reemergence or other data to estimate plant abundance, so our conclusions here are based on the visual inspections done at the time of removal and the overall knowledge of the flux magnitudes and temporal variability in the growing season. We have modified the text in lines 126-131 to reflect the above: “Regrowth inside collars was manually removed at least weekly, minimizing photosynthetic CO<sub>2</sub> uptake. While regrowth abundance was not measured, stable net CO<sub>2</sub> efflux between removals suggests minimal impact. Aboveground plant removal is standard for isolating soil GHG fluxes, though belowground autotrophic respiration from adjacent roots remained, as trenching was avoided to reduce site disturbance. Without a control plot, the direct effect of disturbance on GHG fluxes remains uncertain.”

- Temperature measurements inside the chamber (reply by authors, last reply on page 19). I agree with the authors that in this case significant effects of a temperature rise in the chambers are unlikely. However, the conversion from ppm to moles will still be improved by adding a temperature sensor.

Agree: Yes, we agree with this and in our new deployments of the SkyLine2D system we are measuring the headspace temperature.

## **Report 2 by Daniel Epron**

The authors either addressed almost all of my minor comments or explained why they didn't. While they agree with my major comment, they did not consider it, and I understand their reasoning. In both cases, bias can result from maintaining falsely low values or suppressing truly low values. However, since the concentrations of three gases were measured, some values related to chamber malfunction could have been detected and not confused with a truly low flux, as it would have affected all three fluxes [I agree that it can happen that the three values are truly low].

At least, rather than deleting the result from the database, it would have been possible to flag the questionable flux values and leave future users free to use their own criteria.

Author: Thank you for your comment and understanding our reasoning. We fully agree that flagging questionable fluxes are paramount for the user, regardless of the purpose with using the data. As per request of one of the reviewers we added the standard error of the slope at time zero for each flux in the dataset. This provides the user with an added possibility of flagging fluxes where the fit is uncertain. As described in Section 2.7 the fluxes included already underwent quality control screening using the criteria published in Rheault et al. (2024) and in case of high SE (indicative of overfitting) which is more frequent at low fluxes the linear model was chosen. This approach we consider as the most conservative approach without penalizing the data set using the R<sup>2</sup> or flux value per se. However, with this addition of the SE of the slope we hope we have added even more transparency to the quality of the data set.

### Report 3 by Judith Vogt

I would like to thank the authors for their work on the manuscript. I am still struggling with the fact that this manuscript is meant to be a data paper. It seems that there is a lot to explore, but the authors refrain from doing so to stay within the scope of the journal. I do see that there is a lot of data in this product that could potentially be used by the research community. The authors could be clearer in convincing the reader in this regard. Therefore, I think this paper should be further improved before publication.

Below are more general and some specific comments.

#### Dataset:

The csv files need revision. The first row should include the header and there should not be any empty or redundant columns. Otherwise, potential users might refrain from using the data if they have to clean it up themselves first. The files still seem to have multiple TIMESTAMP columns and it remains unclear to me why. A Readme file clearly explaining the meaning of column names would be appreciated, e.g., what does Head\_05 mean?

Author: Thank for the recommendations. We have gone through all the files to remove redundant columns which was present in the file “VB groundwater depth Figure 6.csv” now “VB groundwater depth Figure 5.csv”. In this file we have reduced number of columns and included a “collar” column. This now means that there are five columns in total without the ambiguous numbering. The file “VB SkyLine2D transect Figures 3.csv” has also been revised to only include position data. In the data repository there is a file called “Data variable explanation.pdf” where all variables are explained in table format for each of the csv-files.

#### Abstract:

The second and third paragraph of the abstract showcase essentially a summary of what I would expect in a Results and discussion section. I think those could be merged and shortened. Afterwards, it would be more valuable to clarify why the presented dataset is useful to the research community. Which processes or relationships could be investigated with this dataset? E.g., those that the authors refrained from further investigating in this paper.

Author: We agree with the suggestion here and have reduced the summary of data to mostly summarizing average values for CO<sub>2</sub>, N<sub>2</sub>O and CH<sub>4</sub>. We have added a new paragraph outlining broadly the opportunities we see with this dataset. This is not an exhaustive list, as we hope that potential future users can find other innovative ways for the data.

1. 11: remove “here” and add “dataset of automated [measurements of] greenhouse gas...”

Author: done

l. 21: mean and SE should be given with the same precision Author: We are unsure how to implement this recommendation. What is meant by precision? We have strived to have equal number and significant digits for average and SE values and view that the numbers we show are detailed enough to provide the reader with a sense of the magnitude and variation of the mean, without being too precise in terms of more or less decimals.

l. 26: split up long sentence Author: This part of the abstract has been reformulated

l. 36: maybe rephrase the sentence too because it is currently confusing – how do CH<sub>4</sub> emission bursts have little seasonal variability? Due to their abrupt nature, variability is large both spatially and temporally. Author: Agree that this sentence was confusing and we deleted the last part of the sentence: “, confirming...”

## Introduction:

The short discussion on pros and cons of measurement techniques (l. 48-53) could be reconsidered: What is meant by extreme events? Please add an example. Also, I think it is possible to determine seasonal dynamics with chamber-based measurements. Their caveat is rather the spatial component and the required workforce when aiming at high temporal resolution (for manual chamber measurements). Regarding eddy covariance measurements, I think it might be a matter of perspective whether the spatial coverage (on a global scale?) can be called “poor” – it depends on the goal. The authors are aimed at introducing automated measurement techniques here, so adjusting the wording of this paragraph may resolve this controversy.

Author: Yes, we see that this line of argumentation may have been drawn up to sharply and have largely reformulated parts of the paragraph in line 52-63.

l. 44: add “net sources [to the atmosphere]” Author: done

l. 48: add “[manual] chamber-based measurements” Author: done

l. 65-75: seems irrelevant to the present study – consider removing Author: agree. This would be more relevant in a paper dealing with flux calculation procedures

l. 76: remove “uniquely” Author: done

l. 79: might be worthwhile mentioning your measurement system here, i.e. “with an automated GHG chamber system [(SkyLine 2D)]” Author: done

## Materials and Methods:

I still suggest some restructuring to avoid repetition and improve readability. First, the site is described, then the main part of the study should be described which are the flux measurements, and afterwards, soil, and other measurements can be described. Move subsections 2.2 and 2.1.4 before 2.1.2. And I would move section 2.4 either before or after 2.1.3. Furthermore, I suggest to merge sections 2.1.4 and 2.6.

Author: Thanks you for taking the time to consider this logical progression of the text. We agree with this division and have largely followed these recommendations. After reorganizing the order of the sections in M&M is:

2.1 Site description

2.1.1 Site preparation and disturbance

2.2 Overview of time series...

2.3 The SkyLine2D system at Vejrumbro

2.3.1 Greenhouse gas flux measurements with the SkyLine2D system

2.4 Peat and organic soil characteristics

2.5 Groundwater table level, depth and sampling

2.5.1 Groundwater water sampling and chemical analysis

## 2.6 Soil moisture and temperature measurements

## 2.7 Wireless data transfer

## 2.8 Calculation of diffusive fluxes

## 2.9 Calculation of ebullition fluxes in the ditch

Reorganizing like this also meant changing table 1 → 2 and table 2 → 1.

l. 94: change title to “Site description” only [Author: done](#)

l. 101: what is meant by “has primarily served as grassland in recent decades due to the wet conditions”? Was the site a wet grassland before drainage in 1950? Or why would the site be wet after drainage? Please clarify/reword. [Author: We can see this is an ambiguous formulation it has been changed to the following: “...was used to cut hay for fodder as the conditions were unfavourable for cereal production”](#)

l. 106: Suggest to remove “and disturbance” [Author: done](#)

l. 107: remove “Initially” [Author: done](#)

l. 115: Suggest to replace “avoiding” with “removing”, remove “also”, remove “and resolve spatiotemporal patterns to a higher degree than previous studies at this site have achieved and what other commercial platforms are capable of” [Author: done](#)

l. 120: Suggest to reword to: “Therefore, we harvested and removed aboveground plants...” [Author: done](#)

l. 120-139: there is a lot of repetition in this paragraph, please conflate. [Author: this paragraph has been shortened to provide a more concise summary of the harvest and glyphosate application \(line 118-130\).](#)

l. 145: a reference to current Fig. 3 would be helpful here [Author: done](#)

l. 148: Please clarify that effective pH is determined under suspension conditions. [Author: done](#)

l. 153-161 and Tab. 1: I would rather expect these in section 3 Data presentation [Author: this can also work and these data have been moved to now section 3.6 Peat soil characteristics. Table 1 → Table 3](#)

l. 165: I think by capping the sample, you don't avoid air bubbles, but contamination? [Author: done](#)

l. 173: remove “to a 10 mL sample” [Author: done](#)

l. 176/Fig. 2: I suggest to merge Fig. 1 and 2 [Author: done](#)

l. 181: replace “placed” with “located” [Author: done](#)

l. 182: remove “little” [Author: done](#)

l. 189 and 194: repetition of collar description [Author: text in original line 194 deleted](#)

l. 200: replace “normal” with “common” [Author: done](#)

l. 201/Fig. 3: please mark the minimum and maximum of the water depth as shaded areas rather than lines that are not easily distinguishable from the mean [Author: done](#)

l. 231: Start phrase with “To measure the depth of the water table, piezometers...” or similar [Author: done](#)

l. 241-247: Please make clear here which metric or maybe rather reference point you are using in the data presentation. The abbreviation WTD should also be introduced in this section – is it positive or negative and does it have the ground surface as the reference point? [Author: this has now been clarified \(line 237-244\)](#)

l. 309: “either” should be followed by “or”, but it is missing in the sentence. [Author: the parenthesis had been placed wrong. Now corrected](#)

l. 311: Is this already accounted for in the numbers given in l. 309? Also, what is meant by “visibly detect” – which criteria was used here to remove low fluxes, also RMSE? [Author: we agree with the reviewer that the formulation was ambiguous and we have now clarified that the total number of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O fluxes \(numbers in line 292\) were discarded based on two situations: 1\) chamber malfunction and 2\) insignificant \(p>0.05\) regression between concentration and time at in situ flux levels close to the minimum detectable flux of the system](#)



- l. 318: Do the annual cumulated fluxes here refer only to the diffusive fluxes? Please clarify. Author: we have now added that for the ditch in this section that it was only the diffusive fluxes. The annual upscaling for ebullition is described in line 327-329. For the peat soil we did not observe ebullition
- l. 338: refine “classified as ebullition events” Author: the classification we used is described in line 308-312 and example shown in Fig. S4. We now refer in the text to this classification
- l. 343: would it not be handier to convert the ebullition flux in the dataset to more common units (per second instead of per 5 min)? Author: This is a good suggestion and in the data file we have now converted the ebullition fluxes to s<sup>-1</sup> to be compatible with the rest of the fluxes. This change in units also result in that the text originally in line 341-346: “Furthermore, ebullition flux is calculated as...” has been deleted.
- l. 344: why not use precise numbers here – i.e. 19.3% and 365 days per year? Even if 360 days were measured, 365 days make the annual estimate. Author: there was a mistake in the number we initially added and the correct is now added (line 327-329).
- l. 347: shouldn’t it be 60 s instead of 30 s, or are you not referring to the delay time of 60 s? Author: Yes. This has now been corrected to 60 seconds. Was a leftover from earlier discussions on this.

#### Data presentation:

- l. 353: worth mentioning the time period here once more Author: done
- l. 359/Fig. 4: consider using a color palette that is easier to distinguish Author: done
- l. 365: Revise caption: Does collar 4 have soil temperature measurements or not? – Tab. 2 states yes, Methods states no, Fig. 3 states no; “along the measurement” probably means “along the transect”; add “blue dots are the raw 5 min measurements of air [and soil] temperature” Author: Thank you for spotting this error. We have now added in M&M section 2.6 (line 259-263) that we initially inserted soil moisture/temp sensors at collar 1, 4, 7, 9, 18, 23 and 27, but sensor failures limited this coverage. This has also been updated in the current Fig. 2 showing the transect.
- l. 369: reference to Tab. 2 does not seem correct, probably Tab. 3 is meant. Please double-check the referencing of figures and tables. Author: after rearranging the sections the reference to table 2 is now correct. We have double checked all references to tables and figures after the reorganization of sections and results etc.
- l. 388: Unit missing “below -40” and now the reference point seems to a different one because of the negative number. Please be consistent throughout. Author: Corrected to “40 cm” and yes negative representation is wrong and we have checked throughout.
- l. 389: add “the deepest groundwater [table]” and do you mean “transect” instead of “site”? Author: corrected
- l. 401: Please update the caption, there are no green lines. Author: done
- l. 411: What is meant by “drying properties of the soil”? Maybe reword. Author: we have now reformulated to indicate that the contrasting modes of decreasing soil moisture may be because of different water retention properties.
- l. 424: Please revise the sentence. Author: We have now reformulated the beginning of the sentence to “This temporal trend was also observed...”
- l. 438: remove “especially elements related to” Author: done
- l. 439: “dynamic” is a very vague word. Please be a bit more concise what you mean. Author: we have reformulated to: “...where the chemical composition of groundwater varied more over time than ditch water.”
- l. 445: I am unsure whether it is clear what you mean by site. I would assume you mean the whole transect. Ideally define this already in the Methods section. Author: we have gone through the entire manuscript and corrected where it is more appropriate to mention “transect” rather than “site”. Furthermore, we use “site” together with “Vejrumbro” to indicate a broader

geographical location rather than the specific transect.

l. 467-471: adding references here would be appreciated Author: this part of the text (line 464-470) has been changed and a couple of relevant papers have been cited.

l. 473: Why “drained”? Do you refer to the past drainage at the site? Author: drained has been deleted

l. 489: I think GWP might have been confused with GHG? Author: yes and no. We actually mean the global warming potential (GWP), but we have reformulated to “...GHG budget in relation to the global warming potential”

l. 502/Fig. 10: Some figures have month axes with abbreviations (Fig. 10), some with numbers (Fig. 12). Please unify. Author: all figures now have month in numbers on the x-axis with year of measurement also added where relevant

l. 504: correct ST to Tsoil Author: done

l. 554: “Net uptake” of what? Author: corrected to “Net uptake of CH<sub>4</sub>...”

l. 573: replace “diffusive” with “diffusion” and again in l. 606 Author: done

l. 575: I find “varies most throughout the measurement period” more precise than “is most dynamic” Author: done

l. 632-635 This section appears out of nowhere. It mentions some methodological hints that should be inserted in the Methods section. If annual estimates are calculated, please state the numbers here or in a table. Were the budgets converted to GWP? If so, which time frame was used? Author: section has been deleted and was by mistake left over from the previous revision. All cumulative fluxes is mentioned now in sections 3.8.1 and 3.8.3.1 (ditch). The simple upscaling to annual estimates is outlined in lines 304-309 (section 2.8) and 330-332 (section 2.9).

Data availability:

The link to the dataset directs to “Page not found”. Author: sorry for this confusion. It was an older link and the correct one - <https://doi.org/10.60612/DATADK/BZQ8JE> - has now been added to the Data Availability section

Conclusion:

I am having difficulties with some of the conclusions since they are – to my understanding – based on visual inspection and not statistical analyses or any in-depth analyses of the data.

Author: We are unsure which specific parts the reviewer refers to. As mentioned in the first review the purpose of this data descriptor paper is for objective and simple presentation of the data and not hypothesis driven research that would necessitate the use of specific and diverse types of statistical analysis depending on the hypothesis being tested.

We have rearranged the conclusion and attempted to be more cautious in the wording, so as not to suggest that we did statistical test. However, in the conclusion regarding we do write how that there is a link to thermal and hydrological drivers are linked to GHG, but in more general terms. These links are well established knowledge, based on decades of research in peatlands. We hardly see a need to make formal statistical tests demonstrating to prove this again. The very dynamic nature of the data also beckons the question which types of statistical analyses are then most suited to test this. Simple linear regression could be one, and we agree with the reviewer that this would have provided a quantitative basis for the conclusion, but not necessarily providing a deeper insight to unravel short term temporal trends that may be more non-linear.

Below we have highlighted the conclusions we have changed to reflect the observatory nature of data interpretation rather than the use of more statistics:

- 1) “However, spatial variation of cumulative fluxes for all GHG were not directly related to WTD levels, contradicting the general assumption that WTD is the primary driver of GHG

emissions.” We have changed the wording of this conclusion to “Specifically, the dataset demonstrates how temporal variation in soil hydrology and temperature is linked to the temporal variation of fluxes. Interestingly, the temporal variability of GHG fluxes across the transect appears to be lower than the spatial variation highlighting that spatial variability in hydrology and temperature may not necessarily be the best predictor of flux magnitudes across the transect.” (lines 639-642) and have deleted the word directly, so as not to suggest a tested relation, but rather communicate that it is based on observation.

- 2) “Cumulative soil N<sub>2</sub>O fluxes exceed what has been previously reported for temperate fens, but show similar seasonal regulation by ST” have been changed to “: Cumulative soil N<sub>2</sub>O fluxes exceed previously reported values for temperate fens at the Vejrumbro site and others. Unlike CO<sub>2</sub>, N<sub>2</sub>O is emitted largely in pulses related to rapid fluctuations of WTD, which increase in size with T<sub>soil</sub>, indicating a seasonal regulation of N<sub>2</sub>O production by temperature.” (lines 656-658)
- 3) “A likely cause for the high soil N<sub>2</sub>O emissions could be a combination of leaching of inorganic nitrogen from surrounding agricultural fields and release of organic N from the decomposing peat.” this has been deleted from the conclusion.
- 4) “The site was during the measurement period an insignificant source of soil CH<sub>4</sub>, which is likely due to the well-drained summer period, a cold wet winter and presence of the major electron acceptors (NO<sub>3</sub><sup>-</sup>, SO<sub>4</sub><sup>2-</sup> and Fe<sup>3+</sup>), providing suboptimal conditions for CH<sub>4</sub> production.” has been changed to “The peat soils across the transect were insignificant sources of soil CH<sub>4</sub> during the measurement period. This could be linked to deeper WTD (20 – 40 cm) during the summer period, a cold wet winter and presence of alternative electron acceptors (NO<sub>3</sub><sup>-</sup>, SO<sub>4</sub><sup>2-</sup> and Fe<sup>3+</sup>), which provide suboptimal conditions for CH<sub>4</sub> production.” (lines 662-664). We argue that keeping the mentioning of the possible impact of the alternative electron acceptors is warranted as we have now added a reference stating this in the discussion (Bridgham et al. line 467) and that this is so well known regarding CH<sub>4</sub> regulation in peatlands that it does not require a statistical test. It would be possible to make more in depth analysis of the CH<sub>4</sub> fluxes – geochemistry relation for the single piezometers and collars, but then again what we communicate here is more of an indirect effect of the electron acceptors, as the most direct effect of these in relation to CH<sub>4</sub> would be expected to be the redox potential.