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Biogeosciences Discussions

Interactive comment on "Spatial and temporal variation of methane emissions in drained eutrophicpeat agro-ecosystems: drainage ditches as emission hotspots" *by* A. P. Schrier-Uijl et al.

Anonymous Referee #1

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Review of A.P. Schrier-Uijl et al.: Spatial and temporal variation of methane emissions in drained eutrophic peat agro-ecosystems. Drainage ditches as emission hotspots, Biogeosciences Discuss., 5, 1237-1261, 2008

Schrier et al. present measurements and a regression analysis of methane emissions and their spatial and temporal variability of landscape elements in two fen grasslands of the Netherlands. They confirm previous findings that drainage ditches and their edges are CH4 emission hotspots. They try to explain the spatial and temporal CH4 emission patterns by environmental factors but do not find any consistent correlation with soil moisture and water table. Temperature turned out as most important factor.

However, its explanatory power was weak. They compare two methods to integrate the occasional field measurements to annual CH4 budgets and find a strong bias between the two methods but cannot conclude which of the two is more realistic.

General comments

The authors do not report how often and when the measurements were performed. Figure 2 suggests that the measurement frequency was about monthly.

The authors should describe the site management in more detail including dates of interventions.

In figure 2 soil moisture and air temperature are shown as a discontinuous records. Given that air temperature is used for the regression calculation of annual fluxes I suggest that continuous records from a nearby weather station are used for interpolation.

The correlation between temperature and CH4 emissions could be related to microbial activity and to the vegetation activity, which is likely to provide the labile carbon for methanogenesis. However, the authors have not addressed any biological parameters that could help understand the spatial and temporal patterns of the CH4 fluxes. Overall, the explained variation by the regressions is small and not convincing since obviously, important drivers of CH4 emissions have not been included in the observations. I therefore disagree that the regression results suit for the integration of the observed CH4 fluxes to an annual balance. (Hendriks et al., 2007) also show a poor fit of the regression between temperature and CH4 fluxes on a similar site, so obviously, temperature seems not to be sufficient for predicting CH4. A multi-factor model, e.g. via multiple regression, and the inclusion of additional factors, e.g. the activity of the vegetation, is recommended to improve the model fit.

I suggest that the authors include quantitative uncertainties in their results and discussion, in particular in the regressions (e.g. Fig. 4) and the annual CH4 emission estimates (Tab. 1). How significant is the regression shown in Fig. 4 if the spatial

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variability of the measurements at each point is accounted for?

Table 1 shows large discrepancies between the trapezoidal integration and the regression. This is not convincing. Please add uncertainties. In my opinion this is too low to derive annual fluxes by trapezoidal integration (Table 1), but could serve for calculating a longer-term average CH4 flux. Vice versa, the correlation between air temperature and CH4 emissions is too weak to justify the estimation of the annual CH4 emissions by regression. The CH4 emissions calculated by trapezoidal integration, in particular from the ditches and edges, appear very high and need further explanation. Can they be related to the productivity of the ecosystem? How much of the C uptake is released as CH4?

The authors have not measured a diurnal cycle of CH4 emissions. It seems that the diurnal cycle of CH4 emissions was calculated from the data set of seasonal data. However, the diurnal patterns of CH4 are not purely driven by temperature, and I disagree that the regression calculated for the annual average temperature dependence of CH4 emissions can be used at the diurnal time scale. The seasonal temperature dependence is driven by the activity status of the vegetation and microbes, while the diurnal cycle is mainly driven by the short-term cycle of substrate flow and plant activity at a given vegetation activity status. I suggest that the maximum reasonable time resolution derived from the available data is at the level of daily mean fluxes. Why should there be a diurnal cycle of this magnitude?

Can the moments with high versus low emissions be separated and analysed to determine when and why emissions were high? The CH4 emissions seem to be dominated by few events. This may improve the robustness of the interpolation to annual fluxes.

The methods and results are very similar to Hendriks et al. (2007) and are presented in a convincing manner. The novelty was CH4 fluxes were measured at two (poorly explained) new sites. Given the poor degree of explanation of the CH4 fluxes, the missing presentation of uncertainties and the missing discussion about robustness and

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credibility of results I do not think that the paper is scientifically new nor sound enough for publication.

Specific comments

Table 1: give uncertainties.

Table 3: Are the results of the quoted studies comparable as mean CH4 emissions? How were the different periods of the measurements and likely biases towards warm / high emissions accounted for?

Fig. 2: add the time series of the water table in field, (ditch), edge.

Fig. 3: add the site names.

Fig. 4: show statistics of regression. I would prefer a graph with the original CH4 data, showing the exponential function.

Fig. 5 does not help much if there is no logical explanation why this is the only month with significant regression. Delete.

Fig. 6: The data available in the study do not allow the calculation of a diurnal cycle. Delete.

The English needs some editing.

Reference

Hendriks DMD, Van Huissteden J, Dolman AJ, van der Molen MK (2007) The full greenhouse gas balance of an abandoned peat meadow. Biogeosciences, 4, 411–424.

Interactive comment on Biogeosciences Discuss., 5, 1237, 2008.