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Interactive comment on "Comparison of eddy covariance CO_2 and CH_4 fluxes from mined and recently rewetted sections in a NW German cutover bog" *by* David Holl et al.

Anonymous Referee #2

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General comments

This manuscript tested the use of single EC tower to estimate CO_2 and CH_4 fluxes from different land surface area (drained and rewetted) in a mined bog in Northwest Germany by partitioning the sources of signals using footprint statistics. It is an interesting paper from both technical perspective and management perspective.

The manuscript in general is well-written. The authors paid special attention to the footprint analysis, which is very good as here we bend the rules for applying eddy covariance technique. And the gap-filling procedure, the model input selection and the comparison of model performance are clearly explained, although it even seems a bit

too technical considering the main topic is the comparison of EC CO2 and CH4 fluxes from different surface types of a restored bog. But it is a matter of style.

One thing not much mentioned in the paper is the information about the processes and controls which I generally have interest in. How did the environmental variables affect the fluxes under different water regime? how important was temperature control, water table and photosynthesis at different time scales in these ecosystems?

I do like the comparison of TER between bare peat and vegetated strips as shown in fig.4. And I would also like to see similar comparison for CH4. Vegetated strips are in close proximity of EC tower from both rewetted and drained section. By merely looking at the frequency of wind directions, the most frequent wind direction are apparently from the vegetation stripes. "The vegetated strips in Himmelmoor cover around 10% of the surface and appear to be especially strong sources of CH4...", as stated in the paper, it further proved the importance of vegetation on CH4 flux. Thus it would be more interesting and useful to quantify the CH4 flux from vegetation and bare peat separately, rather than solely reporting the annual balance of the mixture. In addition, the section about the vegetation is currently very simple. It would be nice if the authors can provide more information on the vegetation as the EC tower is located just near by. For example , there are tree species like *Betula pubescens*. How tall are they? Can there be flow distortion since the EC mast is not very high (2m)?

"In summer of 2012 this area therefore was not yet permanently flooded ...From winter 2012/2013 on, inundation of the rewetted bare peat area progressively increased,..." It would be nice to show the time series of water table level during the study period. How was the dynamics and intensity of the inundation with time? I also wonder if the vegetated area was changing during the study period due to the progressive inundation. It was shown by a previous study that the fractal dimension of the vegetation area has the most importance in explaining the variation of fluxes in a restored wetland (Matthes et al., 2014).

The authors have done a nice job reporting the annual greenhouse gas balances and comparing them to other studies. But we should also be careful here, about the reasons behind those numbers. As I can see from the paper, vegetation and progressive inundation have substantial contribution to the results. Imagine if the EC tower is moved somewhere else with higher (or lower) fraction of vegetation in its footprint, or if the measurements are conducted one year before (or later), are we expecting to get similar results from the drained and rewetted sections? Some sensitivity tests would help to show the reliability of results.

In the end, I do like to see a bit of advices concerning the management of the peat-extraction fields. For examples, is it advisable to rewet the field in terms of climate impact? What are the pros and cons of having large patches of vegetation during rewetting? Should we aim to regulate the water level during the rewetting?

Specific comments

Abstract

Page 1, Line 18: The numbers in CO2 fluxes from rewetted and drained section are wrong. Rewetted section should have lower CO2 emission as stated in the manuscript.

Page 1, Line 20: It is not useful to compare the difference in CH4 to the difference in CO2 in an absolute term. Surely CO2 is larger in the magnitude.

Introduction

Page 2, Line 27-29: Please provide references.

Page 3, Line 1: What do you mean by “higher plants”?

Material and methods

Page 5, Line 19: So the drained section had also some area rewetted during the study period? Please specify what you meant here.

Page 7, Line 15: *Salix spp.*.

Page 11, Line 1: Maybe replace “and” with “or”

Page 11,Line 13:“...70 % at all flux gaps that resulted from data division”. What does

that mean?

Page 11, Line 15-17: Why not using median? Maybe a probability density function plot would justify your method for gapfilling CC_{veg}.

Results and discussion

Page 14, Line 12-20: Was the ANN model prediction compared with the testing subsets as it should be? As written in Appendix A, “70 % training and 30 % validation data”, it seems there is no testing subset of data.

Page 20, Line 20: There are many more recent studies on that topic. e.g. “Impact of water table level on annual carbon and greenhouse gas balances of a restored peat extraction area”, Jarveoja et al., 2016, Biogeosciences.

Conclusions

Page 21, Line 16-17: “The release of CH4 increases after rewetting and within the present two year data set also over time.” This sentence does not read very well.

Page 21, Line 17-18: This statement does not correspond to the current results. CO2 decreased from 887 to 567 g $m^{-2}yr^{-1}$ while CO2e of CH4 increased from 453 to 621 g $m^{-2}yr^{-1}$ in the rewetted section from year 1 to year 2. Otherwise, comparing the rewetted to the drained section, CO2 dropped from 974 to 567 g $m^{-2}yr^{-1}$ and CO2e of CH4 increased from 412 to 621 g $m^{-2}yr^{-1}$ in year 2. Either way it showed the reduction of CO2 emission was more prominent than the increase of CH4 emission during rewetting.

Technical comments

1) Maybe some of the figures D1-4 can be moved to the main text as they validated the modelling and the flux decomposition method.

2) The results on the cumulative fluxes were repeatedly presented in multiple units (g $m^{-2}a^{-1}$ in Table 2, mol m^{-2}, CO2-C g m^{-2} and CH4-C g m^{-2} in Figure 5). Maybe Table 2 and figure 5 can be combined instead.

References

Matthes, Jaclyn Hatala and Sturtevant, Cove and Verfaillie, Joseph and Knox, Sara and Baldocchi, Dennis: Parsing the variability in CH4 flux at a spatially heterogeneous wetland: Integrating multiple eddy covariance towers with high-resolution flux footprint analysis, JOURNAL OF GEOPHYSICAL RESEARCH-BIOGEOSCIENCES, 119, 7, 1322-1339, 2014.