

Reply to review #2 by Anonymous referee.

* R - Referee

* A - Authors

First of all the authors would like to thank the referee for the insightful comments that definitely lead to improvement of the manuscript. We admit that some of the statements in the present manuscript are not accurate and may lead to confusion/misguided conclusions. We will update the manuscript according to the referee's comments.

General comments

R: 1) *It is not fully clear, to which extent data and results presented in this study have been published before. This needs to be declared very clearly at the end of the introduction or at the beginning of the methods section.*

A: A paragraph clearly describing previously published data and used in the study will be added to the introduction section.

R: 2) *In my view the terms "ecosystem scale" and "plot scale", which are very important in the present study, need some initial consideration and a clear definition, how they are used here. Both terms are not well defined a priori and can potentially be used in different ways.*

A: We will add a paragraph with definitions of "ecosystem scale" and "plot scale" to the methods section.

In context of the present study:

"Plot scale" - is the footprint area of a chamber; covers a single plant community.

"Ecosystem scale" - is the 1 x 1 km square; covers a mixture of several plant communities.

R: 3) *It is misleading and not appropriate to mix the problems of upscaling and of EC footprint weighting, as it is done in Section 2.8 and in other parts of the manuscripts. Upscaling in the present context is a self-standing task to apply plot scale model simulations (or chamber measurements) on larger spatial scales. It has per se no direct relation to EC measurements. On the other hand, the footprint weighting distribution is a specific characteristic of EC measurements (to account for their limited and varying spatial representativeness) that is not directly related to ecosystem modelling and upscaling applications. Thus formulations like "3.3 Upscaling by FW average" are confusing and must not be used. The two issues should be clearly separated in the text. The term upscaling should be used to describe the combination of the peatland model with the satellite derived vegetation map to infer the spatially and temporally resolved methane emission in the study area. On the other hand, the terms "footprint weighted (FW) averaging" and "area averaging (AW)" should be used to describe the validation (of the upscaled methane emission) with EC measurements by two different procedures.*

A: The manuscript will be changed accordingly.

R: 4) *For illustration and interpretation of the two validation procedures (AW and FW average), it is necessary to show the average or typical footprint location/distribution within the 1 x 1 km map. The footprint information in Fig. 5 is definitely not enough. The difference between the AW and FW average obviously depends on the position of the footprint depending on the wind direction statistics. Therefore it is essential for the manuscript to show the specific distribution of wind directions and thus footprint locations in the validation period.*

A: It is not really clear what the referee is referring to. "Average or typical footprint location/distribution" or "specific distribution of wind directions and thus footprint locations". Fig. 5 shows the average (typical) footprint distributions binned by wind direction and averaged for the both measurement periods. We will replace this figure with a half-hourly fetch locations figure.

Specific comments

R: 5) *p3931, line 3: In my view this statement is not really true. The EC technique for methane cannot be*

generally considered as low in costs or low in power supply needs.

A: Corrected.

R: 6) p3933, line 17: *Since the angle of attack correction is not commonly applied in EC studies, the effect of this correction should be described.*

A: Angle of attack dependent correction proposed by Nakai et al., 2006 tends to increase calculated fluxes by 2.7 – 5% at short vegetation sites. This sentence will be added to the text.

R: 7) p3934, line 1: *"An energy balance analysis of the system..."*. *With the measurement system (instrumentation) described here, an energy balance analysis cannot be performed. Beside net radiation and ground heat flux, especially an EC measurement for water vapour is needed.*

A: The energy balance presented by van der Molen et al., 2007 does include latent heat fluxes, measured by LI-7500. They report 99.8% energy balance closure. Parmentier et al., 2011 also performed an energy balance analysis and found similar closure. The instrument description will be added to the methods section.

R: 8) p3934, line 3-8: *Cospectra for different wind speeds should not be averaged (only if wind speed normalised frequencies f^*z/u are used). In addition I cannot see a $-4/3$ slope for the wT cospectrum where it is most expected.*

A: The figure erroneously reports natural frequency, when in fact dimensionless frequency is used. This will be corrected in the manuscript. Regarding the slope of the wT cospectrum in the inertial subrange, it is indeed not $-4/3$, but seems to follow $-2/3$ slope instead. We cannot explain this behaviour at the moment as the slope doesn't change with the wind direction (calculation of cospectra was performed using EddyPro 5.1.1 and Alteddy 3.90, but showed similar results). However, when applying frequency response corrections, the correction factor is calculated using model cospectra defined by Moore et al. (1986), Kaimal et al. (1972) and Højstrup et al. (1981), therefore the effect of the observed elevated cospectral power in the inertial subrange is compensated for at the cost of increased uncertainty.

R: 9) p3934, line 9-12: *Due to the problems mentioned in 7) and 8), this statement is not justified.*

A: Fixed (see replies to specific comments #7 and 8).

R: 10) p3934, line 13: *What are the reasons for gaps in the EC measurements? Which quality control and rejection criteria were used?*

A: One 15 hour gap is due to loss of power supply to the instruments (Parmentier et al., 2011), which occurred on 28 July, 2008. Some data were discarded by stability parameter ζ . Only measurements within $-1 < \zeta < +1$ region were used for the study. This resulted in removal of 162 half-hourly measurements.

R: 11) p3934, line 18f.: *Where have these chamber measurements been performed (location on the map)?*

A: The chamber flux measurements were performed along a board-walk to the south of the EC tower on the terrace and along another board-walk on the floodplain. Locations will be added to the map.

R: 12) p3935, line 2: *Specify, what "GeoEye-1" is.*

A: "GeoEye-1" is a high-resolution earth observation satellite. This will be added to the vegetation map description.

R: 13) p3936, line 3: *What does "plot-scale model" mean here? Does it mean that the model was one-dimensional? (see also comment 2).*

A: "plot-scale model" in this context indeed means that the model is one-dimensional.

R: 14) p3937, line 1-3: *how many cases (stable and unstable) were affected by this criterion?*

A: 25 unstable, 37 stable in 2008 and 48 unstable, 52 stable in 2009.

R: 15) p3937, line 11: *This is not a useful flux footprint climatology of the EC tower! A real footprint distribution would be needed (see also comment 4).*

A: See reply to general comment # 4.

R: 16) p3938, line 24: *F_{FW} rather represents the "footprint integrated methane flux" than the "ecosystem scale methane flux". These two scales are not identical as illustrated by the results of the present study.*

A: Changed.

R: 17) p3946, line 16/17: *This is a misleading statement. It is not the area-weighted upscaled flux (which is the final quantity of interest!) that underestimated the EC flux. In contrast it is rather the EC flux that obviously overestimated the upscaled flux, due to non-representative footprint coverage! (see also comment 3).*

A: This will be changed in the revised manuscript.

R: 18) p3946, line 22-24: *This is a somewhat misleading formulation. It seems to imply that footprint weighted averaging should generally be used to upscale the model results to larger scales? This would be wrong because the footprint weighting is only necessary (and meaningful) for comparison to EC measurements, and nothing else! (see also comment 3).*

A: This will be changed in the revised manuscript.

R: 19) Fig. 5: *The different shades of blue are not well distinguishable here. Better use clearly different colors for the different vegetation classes as in Figs. 6 and 9.*

A: We will update the figure using different colors.

References

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Nakai, T., van der Molen, M. K., Gash, J. H. C., and Kodama, Y.: Correction of sonic anemometer angle of attack errors, *Agr. Forest Meteorol.*, 136, 19–30, 2006.

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