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Interactive comment on "Measurements of CO_2 exchange with an automated chamber system throughout the year: challenges in measuring nighttime respiration on porous peat soil" *by* M. Koskinen et al.

Anonymous Referee #2

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General comments

The article presents detailed data and analyses about a newly developed automated chamber system for measuring soil CO2 exchange in two boreal peatlands. The focus of the paper is clearly on the methodological aspects of automated chamber measurements, with little discussion of the data with respect to the magnitude of the measured flux rates in the context of peatland CO2 exchange. However, studies presenting data from automated chambers and comprehensive discussions of the challenges associated with automated chamber measurements are still rare, with many more studies

presenting data from manual chamber measurements. Therefore, this study is of great value to all interested in the advantages of using automated chambers for assessing in situ soil CO2 exchange with high temporal resolution. The subject is thus also of considerable interest to the biogeochemistry community and the readers of Biogeosciences Discussions. The manuscript is generally well-written and easy to follow. I particularly liked the thorough review in the introduction about the necessity for automated chamber measurements, the advantages & limitations of and the challenges associated with such measurement systems. However, the manuscript could be further improved by reducing redundancies between the methods and results/discussion section and addressing a few minor issues (see specific comments).

Specific comments

Above all, I would like the authors to point out very clearly in the manuscript which component of ecosystem CO2 exchange they have actually measured. Given that the automatic chambers are transparent, the reader could easily be lead to believe that NEE was measured. However, as the study was conducted in forested peatlands, in the daytime the chambers do not measure NEE as they neither capture aboveground tree respiration nor tree gross primary productivity (GPP). So, as I understand it, the chambers capture heterotrophic soil respiration, autotrophic respiration from the roots of trees and ground vegetation as well as the GPP of ground vegetation. Daytime opaque measurement (by shading with cloth; p. 14202 ll. 14-16) would thus capture soil CO2 efflux consisting of heterotrophic soil respiration and autotrophic respiration from the roots of trees and ground vegetation. This differentiation may also help to interpret and improve the rather diffuse respiration-temperature models in the manuscript, as the current variability could result from differences in tree root biomass below the chambers (unknown) and differences in ground vegetation type (known; data should not be pooled for different vegetation types; see below).

Flux calculation: Why was the CO2 flux rate calculation done using a 5 s average of raw data? While this removes some of the noise originating from the CO2 sensor itself,

it also reduces the numbers of data points and thus the regression fit particularly for very small flux rates. Moreover, this seemingly reduces the error of the regression parameters, providing a false error value for subsequent error calculation of periodic cumulative estimates of CO2 exchange.

Flux filtering: Generally removing all negative and zero CO2 fluxes (p. 14206 ll. 20-22) will create a bias the results. Particularly in winter, real fluxes may be very small and difficult to distinguish from very small positive fluxes due to the error (noise) of the CO2 sensor. In theory, when there is a real zero flux, the detected flux resulting from the sensor noise could be positive or negative with equal probability. Fluxes originating from the sensor noise should therefore average out to zero in the longer term. By removing exclusively the negative and zero fluxes, overall flux rates will be slightly overestimated. Please reconsider the sensor error (concerning the detectable CO2 concentration) in combination with your measurement time (chamber closure) to determine the range within which a small positive or negative flux could exclusively originate from sensor noise and include all of these fluxes. Negative fluxes outside of this range can then be discarded on the assumption that the chamber was not closed.

Respiration modelling 1: Why do you not include the nighttime respiration measurements in the nights close to the daytime campaigns (NEE = Reco when PAR=0) in the parameterization of the Lloyd and Taylor model? It would likely extend the temperature range for which your model is parameterized and would thus provide a more reliable model for gap-filling.

Respiration modelling 2: I do not understand why you combined data from all chambers at one site to parameterize the respiration model. In section 2.1 (p. 14201 ll.3ff.) you explicitly state that the six plots at each of your site were chosen to each represent different plant communities, ranging from empty patches (no vegetation at all) to mosses to small shrubs. The relative contribution of heterotrophic and autotrophic respiration at each of these plots is therefore very different among these plots – and thus likely also the temperature dependency of respiration! The scattering of your man-

ual measurements in Fig. 13 and the scattering of the automated daytime respiration data suggested by the diverging functions therefore not only includes temporal variability (the mentioned 2 seasons), but also variability originating from the different plant communities. I would prefer to see respiration models by plant community type.

Respiration modelling 3 (p. 14217): Why do you not present annual estimates for the 2 years x 2 sites based on the different Lloyd-Taylor models you fitted? It would be of great interest to see how much the annual respiration estimates differ among your 4 models and how much they differ between the plant communities (see comments above). Effect of fan speed (p. 14215 and Fig. 8): Why do you present the data meant to illustrate the effect of fan speed in the form of respiration vs. soil temperature scatter plots? I cannot see a clear fan effect in Fig. 8 as to me, the different variability in the scatter plots could also be due to inter-annual differences (2011-2012; row 1 vs. row 2) or differences in flux measurement and calculation (day vs. night or 30-90s vs. 120-240s). The fan effect comes out wonderfully in Fig. 9. I therefore suggest removing Fig. 8.

Sensitivity to u* (p. 14215-16 and Fig. 10): You state that the measured fluxes sensitive to increases in u*. First, Fig. 10 is not a very straightforward way of illustrating this fact, as the scatter is fairly large and linear trends do not seem appropriate for this data. How about reclassifying the data (similar to the high vs. low fan speed in Fig. 9) into u*-groups and present the results as notched boxplots in a new Fig. 10? It is also not clear to me why the complete removal of the sensitivity of respiration measurements to u* is aimed for (p. 14216 ll. 16-22). I suspect that real in situ fluxes are indeed sensitive to u*, so why should measurements not be? I do not think that unreliable nighttime measurements can be easily replace with modelled data based on daytime measurements exclusively.

Conclusions (p. 14218 ll. 5ff.): You conclude that you have enough measurements for linear interpolation and gap filling via modelling yet you did not show a single continuous annual model and/or annual estimate for any site or chamber. As you state in your

introduction, automatic chamber measurements are desirable for providing better data on long-term balances, but you did not show that your system can provide such data. It seems that it can . . . so why not show it?

Technical comments

p. 14201 l. 10 The peat density (0-22 cm depth) . . .

p.14202 l. 22 . . . a transparent polycarbonate chamber . . .

p. 14202 l. 11 Please report the actual fan speed.

p. 14202 ll.11-14 Please state here that you did not install any cooling system in the chamber, as this is often done in CO2 chambers.

p. 14205 l. 18 Remove comma after atmosphere.

p. 14208 ll.15f. Interesting to read that you did not have any problems with rising within-chamber air temperatures as commonly observed with transparent chambers. Do you have an explanation for this?

p. 14211 ll. 16-19 Please change the order of your enumeration to 1) starting point, 2) length of fit, 3) type of fit, 4) fan speed and 5) atmospheric turbulence, as this is the order in which you subsequently present and discuss the results. Also re-order accordingly in the abstract.

p. 14212 ll. 25-28 These two sentences are highly redundant. Remove either one and start the following paragraph with "Secondly, we tested . . ."

p. 14213 ll. 4ff. Consider rephrasing: "Although the mean flux did not . . ., the nonlinearity and random . . ."

p. 14213 ll. 7-9 Does this hold true for daytime and nighttime fluxes?

p. 14213 l. 27 to p- 14214 l.4 This paragraph is very general and seems a bit out of place. Remove here and consider integrating it into the introduction.

p. 14214 ll. 22f. Rephrase: " In this case, a nonlinear function is therefore . . ."

p. 14214 l. 27 Replace "suggest" with "suspect"

p. 14215 ll. 6-7 What do you mean by the similarity of the general dynamics of CH4, N2O and CO2? Explain!

p. 14216 l. 29 and p. 14217 l. 2 As there is no Table 3 I suspect that you are referring to Table 1.

p. 14218 ll. 3-8 Given the large number of figures and the relative low visual contribution of Fig. 12, I suggest to remove Fig. 12 and to report range of values (effect in %) and the correlation statistics in the text.

p. 14218 l. 23 to p. 14219 l. 6 Please combine these sentences into one paragraph.

Table 2. Incomplete sentence "The data used . . . measurements." Please rephrase.

Fig. 7. Please report the numbers of data points in each boxplot (n).

Fig. 8. Remove from manuscript and incorporate the data from Kalevansuo equivalent to the Lettosuo data shown in Fig. 11 into Fig. 11.

Fig. 9. Is the effect of fan speed also detectable in the daytime or only at night? Please report the numbers of data points in each boxplot (n).

Fig. 10. Remove and replace with a better figure to illustrate the effect of u* (see above)

Fig. 11. Please expand with the respective data for the Kalevansuo site. Did you try to fit a Lloyd-Taylor model rather than a linear function? Maybe the fit is better when splitting the data into seasons? Please report the numbers of data points used for each model (day/night) and chamber.

Fig. 12. Remove and integrate into text (see above).

Fig. 13. Please also show range of automated measurements underlying the respira-

tion models (maybe very small dots or shaded area). Please provide the parameters for the different Lloyd-Taylor models! What are the “manual” measurements referred to here . . . are these the measurements done during the daytime using cloth to shade the chambers? Please specify!

Interactive comment on Biogeosciences Discuss., 10, 14195, 2013.