Biogeosciences Discuss., 9, C530–C533, 2012
www.biogeosciences-discuss.net/9/C530/2012/

Biogeosciences
Discussions

Interactive comment on "The effect of atmospheric turbulence and chamber deployment period on autochamber CO_2 and CH_4 flux measurements in an ombrotrophic peatland" *by* D. Y. F. Lai et al.

Anonymous Referee #1

Received and published: 1 April 2012

GENERAL COMMENTS

The article of Lai et al. presents evidence for biases that are introduced into closed chamber measurements of CO2 and CH4 soil-atmosphere fluxes due to changes of atmospheric conditions (e.g. wind speed, turbulence intensity, concentration gradients) that are inherent to this flux measurement technique. The authors present a very valuable extensive flux dataset derived by an automatic closed chamber system installed at three sites in an ombrotrophic peatland. The dataset has a temporally high resolution which is of great advantage for detecting the artefacts that are introduced by the alteration of ambient turbulence conditions by the employment of the closed chambers.

Such artefacts were suspected by other researchers before; however, the number of measurements at a single site type was often not sufficient to unambiguously prove these effects. In this way, the study at hand is definitely a good step forward. An optimal understanding how gas fluxes between soils and the atmosphere are controlled is important for a correct interpretation of chamber measurements and unbiased flux estimation. Closed chamber measurements are commonly used for the estimation of soil-atmosphere carbon exchange fluxes, and the considerable potential measurement biases will propagate through simple up-scaling or biased parameterisation of mechanistic models into regional and global carbon budgets. The authors not only demonstrate the substantial potential bias due to turbulence changes by the chamber deployment but they also give recommendations how to minimise these errors, i.e. by waiting until the concentrations gradients have adjusted to the new conditions after chamber deployment which, according to their analysis, can last more than 10 min. These recommendations are not in line with other recent literature on the topic but were developed by the authors in a stringent and understandable way. Thus, I consider this article as a very important contribution for the scientific field which should be intensively discussed by researchers using the chamber methodology.

II.) The article is very clearly structured, well written and easy to follow. The abstract is concise and informative but could have used a bit more precise wording (see specific comments). The introduction nicely introduces into the scientific topic and the respective literature and leads straightforward to the scientific goals of the study. The methods section provides all information needed to understand and evaluate the presented results. Generally, the extensive results are well presented, and the figures and tables are clear and informative. The number of tables and figures are appropriate. I recommend adding another figure showing typical gas-concentration-over-time curves to illustrate the effect of slowly stabilizing fluxes on the raw data. The authors discuss their results in an interesting way and develop conclusions relevant for the community of soil-atmosphere carbon flux science.

III.) I recommend this well-written, interesting and highly relevant discussion article for publication in Biogeosciences after minor revisions.

SPECIFIC COMMENTS:

Page 1439, line 9: I suggest adding: “ambient” to stress the difference between ambient and chamber headspace conditions: “. . .correlated with ambient friction velocity. . .”

Page 1439, lines 10-13: “This suggests. . .conditions.”: Though I like most of the text very much, I think that this central sentence in the abstract should be improved. Now, it sounds as if ambient conditions with high wind speeds lead to an underestimation of the flux. However, the underestimation is due to the change of atmospheric conditions by the chamber deployment from highly turbulent-windy to less turbulent-less windy. At least, this is what I understood from your results and discussion. The problem is the method not the ambient wind.

Page 1439, lines 12-16: “underestimate”, “overestimation”: I think that it would be good to state already in these sentences what you used as reference values for under- or overestimation.

Page 1440, line 26: There are also some considerably larger estimates, e.g. Yu et al. 2010 (doi:10.1029/2010GL043584).

Page 1443, line 16: I suggest adding “observed” before “diel CH4 flux pattern”.

Page 1443, line 22: I suggest adding “observed” before “diel variability”.

Page 1443, line 23: I suggest rewording: “. . .associated with artificial and abrupt changes of atmospheric turbulence by chamber deployment.”

Page 1443, lines 26-29: I think that it should be made clearer that the low transient flux is due to the reduction in turbulence by the chamber deployment. Under the highly turbulent ambient conditions, the flux is probably not low. The soil gases are rapidly flushed out of the upper soil pore space. If turbulence is then abruptly lowered by

the chamber deployment, first this upper soil pore space can be enriched with the gases. Only after the gradients have adjusted to the new transport resistances, the flux stabilises.

Page 1449, line 3: How was the water dilution corrected for? Equation?

Page 1449, lines 13-19: Why not using the RMSE of the regression as quality control criterion? What is the advantage of using R2 (which systematically assigns lower fluxes a lower quality than higher fluxes) in comparison to RMSE?

Page 1456, line 29: I think that “re-establish” does not fit. The concentration gradient has to adjust to the new turbulence conditions and associated transport resistances.

Page 1458, lines 11-13: This experiment would be difficult. To quantify the flux, one would have to measure a concentration profile within the chamber headspace and integrate this over the headspace height.

Page 1463, lines 10-13: Again, I think that not the windy conditions lead to the low flux, but the artificial and sudden decrease in turbulence by the chamber deployment.

Page 1464, lines 18-21: I think that here some discussion is still open. After 13 minutes, the gas concentration in the headspace is already increased compared to the ambient conditions. Shouldn’t this have an effect on the diffusive fluxes compared to ambient conditions? Furthermore, I think that this “quasi-equilibrium between the net rates of gas production and gas exchange across the peat surface” might be over-simplified. It is to be expected that with peat depth CH4 and CO2 concentration increase. Thus, there is a rather large pool of these gases in lower parts of the peat soil which is only slowly released due to high diffusion resistances. This pool and its properties (concentration, temperature, pressure) should play also a role in controlling the surface exchange rates, not only the gas production and top soil transport processes.

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