Anonymous Referee #1

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.

>> Authors' response: We thank the reviewer for the thorough review of our manuscript. We will add a figure under Section 3.3 to depict the change in headspace gas concentration over time during chamber deployment as shown below.

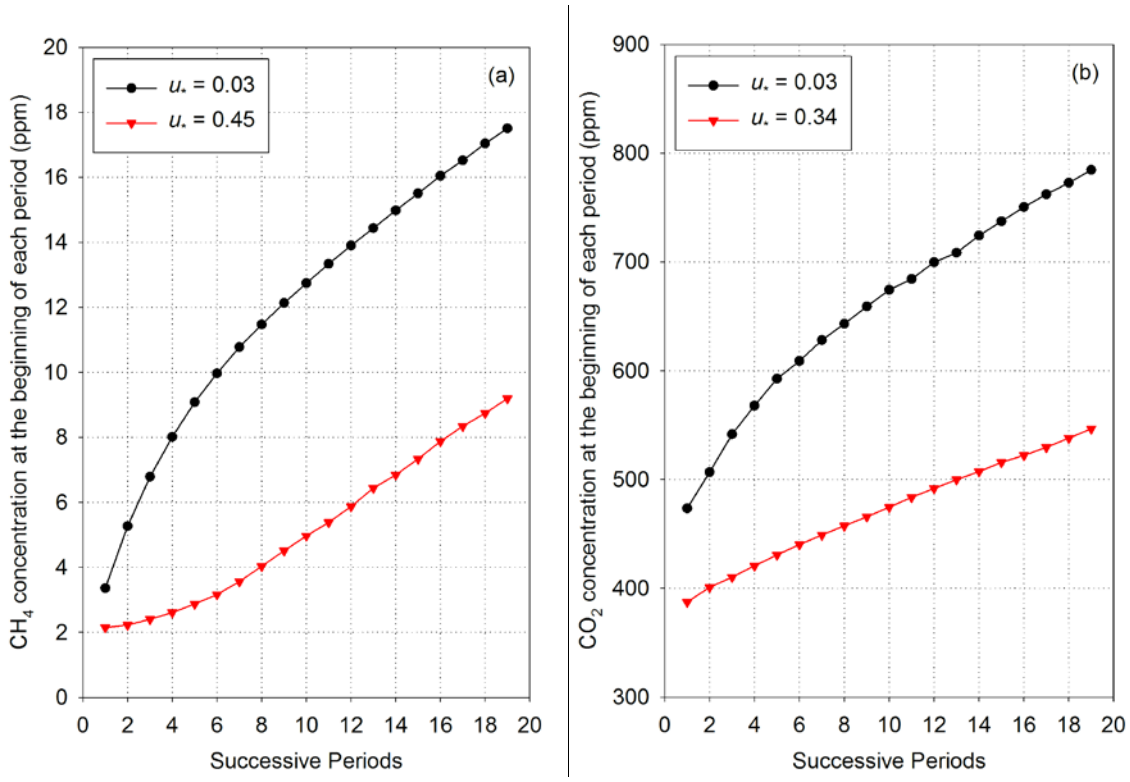

Fig. 7. Examples of the concentration trace in the headspace of a sedge-dominated chamber over 19 successive 1.5-min periods in calm and highly turbulent conditions for (a) CH_4, and (b) CO_2.

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…"

>> Authors' response: We will modify the sentence as follows: "…both CH_4 and nighttime CO_2 effluxes were highly and negatively 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.

>> Authors' response: We agree and will modify the sentence as follows: "This suggests winds were flushing the very porous and relatively dry near surface peat layers and reducing the belowground gas concentration gradient, which then led to flux underestimations owing to a decrease in turbulence inside the headspace during chamber deployment compared to the ambient windy conditions."

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.

>> Authors' response: We will use the term "net biological fluxes" in these sentences, which implies reference values to be fluxes unaffected by turbulence that are measured after 13 minutes of chamber closure, as described in the abstract. To maintain the logical flow of the abstract, we will revise these sentences as: "We found a 9 to 57% underestimate of the net biological CH_4 flux at any time of day and a 13 to 21% underestimate of nighttime CO_2 effluxes in highly turbulent conditions. Conversely, there was evidence of an overestimation of ~100% of net biological CH4 and nighttime CO_2 fluxes in calm atmospheric conditions possibly due to enhanced near-surface gas concentration gradient by mixing of chamber headspace air by fans." We will also do a global edit to ensure the use of this terminology is consistent throughout the manuscript.

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

>> Authors' response: We will cite the Yu et al. paper and change the upper limit of C storage to 547 Pg.

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

>> Authors' response: We will modify the sentence as suggested.

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

>> Authors' response: We will modify the sentence as suggested.

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

>> Authors' response: We will reword the sentence as suggested.

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.

>> Authors' response: We agree and will add the following sentence to clarify: "The abrupt decrease in headspace turbulence following chamber deployment will cause an increase in gas storage in the peat pore space until sufficient time is given for the concentration gradient to adjust to the new transport resistances."

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

>> Authors' response: This is simply a conversion of gas concentration from wet to dry basis using the H_2O concentration measured by LI-6262 according to the following equation (taking CH_4 as an example):
CH_4 conc. (in umol mol^{-1} dry air)
= CH_4 conc. (in umol mol^{-1} wet air) / (1 – H_2O conc. (in mol mol^{-1} wet air))

To make this clear in the manuscript without the need of an equation, we will modify the sentence as follows: "Mole fractions of CO_2 and CH_4 measured in the autochamber headspace were converted to mixing ratios using the concurrent LI-6262 measurement of water vapour concentration to account for water vapour dilution effects."

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?

>> Authors' response: We have tried to use the standard deviation of residuals of the regression (which is a similar measure as RMSE) as a quality control criterion. However it is not effective in identifying bad quality fluxes, which were mostly collected when there were issues with leakage or broken tubing. These fluxes typically have a small magnitude as well as a small RMSE – they could not be filtered out satisfactorily using an RMSE threshold.

We will add a sentence to address this as follows: "The poor quality fluxes could not be filtered out satisfactorily using a root mean square error (RMSE) threshold, since these fluxes were mostly collected when there were issues with leakage or broken tubing, and typically had a small magnitude as well as a small 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.

>> Authors' response: We agree and will revise the sentence as: "…there was insufficient time for the diffusive concentration gradient to adjust to the new turbulence conditions and associated transport resistances, and much of the gases produced were stored in peat rather than released into the chamber headspace."

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.

>> Authors' response: We will revise the sentence as follows: "A test of this hypothesis would be to remove the fan and determine fluxes over calm nights by measuring and integrating the concentration profile over the height of the chamber headspace…"

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.

>> Authors' response: We agree and will revise the sentence as follows: "In highly turbulent conditions, underestimation of transient fluxes was caused by a combination of an artificial and sudden decrease in headspace turbulence by chamber deployment, as well as a reduction of diffusive concentration gradient from wind flushing and/or pressure pumping, as shown by our pore space CO_2 concentration data in near surface peat."

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.

>> Authors' response: We acknowledge the effects of gas accumulation in the chamber headspace on the concentration gradient and hence measured flux. Yet, as discussed in the paper, this chamber feedback effect is much less significant than the effect of changing turbulence by lid closure on initial fluxes, at least over the deployment period of 30 minutes in our study, since the flux rates remain constant after 13 minutes rather than decreasing continuously over time.

The reviewer points out that there is a large pool of gases in the deeper peat layers. If the gas production rate is not in equilibrium with the gas exchange rate across the surface, there should be a change in the amount of gas stored belowground. This deserves further investigation with profile measurement, but is technically challenging especially for CH4. Meanwhile, the constant flux obtained suggests that a quasi-equilibrium is likely attained on the time scale of minutes during chamber deployment.

To address the above, we will add a sentence as follows: "The pore space gas dynamics in the peat profile deserves further attention for assessing the influence of a large pool of gases in the deeper peat layers on the rate of gas exchange across the peat surface."