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Interactive comment on "Overestimation of closed chamber soil CO_2 effluxes at low atmospheric turbulence" *by* Andreas Brændholt et al.

Andreas Brændholt et al.

andbr@env.dtu.dk

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We thank reviewer 2 for taking the time to review the manuscript and for providing helpful suggestions. We provide the following response to the reviewer's questions, suggestions and comments.

"Specific points: For personal experience, one important issue in chamber measurements is the soil collar insertion, not only regarding the depth and disturbances to different soil components of but also the collar height outside the soil surface. The 8100/8150 is designed to achieve a good mixing inside the chamber without using a fan. But without a fan inside the chambers, the collar should be set low to few cm (offet âĹij5 cm), since too high collars could make the air mixing inside the chamber difficult during nighttime. The authors should specify in the manuscript not only the insertion

depth but also the collar height and offset, and see if improvements could possibly be made by lowering the collar."

We will include information of soil collar height in section 2.2. The average collar height for the 8 soil chambers was 5.2 cm with the individual collar heights ranging from 4.5 to 5.8 cm. This seems to fall well in line with low collar height required from the reviewer's experience.

"Related to the first point, a good solution for solve the night-time overestimation could be acting on the deadband in post processing. Indeed during nighttime the air mixing could be poor shortly after the chamber closes, but then a good mixing could be kept by the flow between the LI-8100 and chamber. Even if at lines 22-25 (pg 14) the authors explain that they checked different deadbands, they did not show results of this analysis. It could be interesting to see how a raw gradient appears during one daytime well-mixed measurement compared to one nighttime calm measurement and test if a larger deadband may in part improve the gradient fitting. Did the author try to recompute the whole dataset with a larger deadband (e.g. 50s) and see if the diurnal cycle is at least reduced? One limit could be the short measurement time used (90s). However, this could be a very important way, because if it is true that u* acts on the overestimation of chamber CO2 nighttime efflux, we would love to find a way not to reject a big amount of data as for EC. Depending on the air mixing conditions both linear and non-linear fit could perform better, for this reason the use of the best between the two methods for each single measure could be a solution to be tested."

Based on results by Lai et al. (2012), we decided to look at different dead bands up to 40 s, to see if dead band had an influence on the overestimation. The overestimation of fluxes at low u*, however, persisted independent of dead band. We therefore decided just to mention these results in the paper, instead of showing them, which would add considerable length to the manuscript. We also inspected the increase in CO2 concentration during chamber closure both during high and low turbulence. However, the increase seemed to follow a pattern expected from flux theory during both high and low

turbulence. We also tried to see if removing fluxes at higher r2 values of the fit to the increase in CO2 concentration, during chamber closure, could remove overestimated fluxes at low atmospheric turbulence (see line 30, page 13 to line 4, page 14). However, a similar response of u* was still seen, indicating that the increase in chamber CO2 concentration followed an expected pattern that let to high r2 values, even though the measurement resulted in an overestimated flux. The 90 s chamber closure time was indeed a limit to investigating flux overestimation at low turbulence at much longer dead bands, and a longer chamber closure time could beneficially be used for future studies.

“The authors present an approach to keep an adequate mixing of air around the chambers by using table fans. Even if the use of fans represent an interesting and efficient test in this study, I’m wondering if instead the use of a “channelled” artificial wind in a very stratified atmosphere could instead drain out CO2 from the near-chamber air volume, even if the authors mention this point, I think that the use or not of fan in chamber measurements should be better investigated before promoting its use.”

We agree that it is possible that using fans can drain out CO2 from the near-chamber air volume. We also agree that using fans for chamber measurements need to be better investigated before this approach can be promoted. This is in line with our statement on line 15, page 15 where we write that “Additional studies are needed to further explore this approach”.

“Minor points - I find the paragraphs at lines 23-31 and 32-34 pg. 3 too long: I suggest to focus only on the aims of the study and not on the many details concerning how the study is carried on that should be placed in the material and method section”

We agree. We will shorten the paragraphs to focus only on the aims of the study.

“-pg 5 – line 4: remove “which yielded a total of 52131...”

We will remove it.

“-pg 5 – line 22: “the current manuscript focuses on the potential error introduced by low turbulent...”

We will change “in” to “on”.

“- pg 5 – line 25-27 this part could be better placed in section 2.2”

We agree to move the lines. We will suggest moving the first sentence, concerning the one year campaign, to section 2.2., and the second sentence, concerning the fan experiment, to section 2.3.

“- Fig. 1 probably a scatter plot of nighttime Rs versus u* values will better present the inverse relationship”

Thank you for the suggestion. We initially tried a scatter plot. However, the large amount of data resulted in a plot overloaded with data points. Instead, we provide a boxplot of mean hourly soil CO2 effluxes plotted against binned groups of u* in figure 4, which we think better presented the inverse relationship.

References: Lai, D. Y. F., Roulet, N. T., Humphreys, E. R., Moore, T. R., and Dalva, M.: The effect of atmospheric turbulence and chamber deployment period on autochamber CO2 and CH4 flux measurements in an ombrotrophic peatland, Biogeosciences, 9(8), 3305–3322, doi:10.5194/bg-9-3305-2012, 2012.

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