

## ***Interactive comment on “Overestimation of closed chamber soil CO<sub>2</sub> effluxes at low atmospheric turbulence” by Andreas Brændholt et al.***

### **Anonymous Referee #2**

Received and published: 12 December 2016

#### General Comments:

The manuscript from Brændholt et al. presents a very interesting study about the overestimation of night-time soil respiration (Rs) measured by chambers, when low turbulence mixing occurs. The main finding of the study is that night-time Rs is inversely related to friction velocity, and filtering out data measured under a certain threshold of  $u^*$  removes the differences between night-time and daytime Rs, underlying that the observed overestimation is due to poorly mixed air at the chamber level. This is a very important topic in the CO<sub>2</sub> flux community also because chambers are often used to be compared with the underestimated nighttime CO<sub>2</sub> flux by the eddy covariance method, but a gap still exists between the highly standardized procedures to process, check and filter EC data and the lack of common guidelines for checking the quality of chamber measurements. The manuscript is very well written and argued. The text is well struc-

[Printer-friendly version](#)

[Discussion paper](#)



tured and fluent, and the figures very clear. I think that there are some controversial points, e.g. the covariation of  $u^*$  with some biological process in the diurnal cycle, such as translocation of assimilates and lag-effect in the root flux component (e.g. Heinemeyer al. 2011), the covariation of  $u^*$  with temperature, the hysteresis between soil CO<sub>2</sub> efflux and temperature. However, the authors well discussed many of them in the manuscript.

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 (offset  $\sim 5$  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.

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 CO<sub>2</sub> nighttime efflux, we would love to find a way not to

Printer-friendly version

Discussion paper



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.

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 CO<sub>2</sub> 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.

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

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

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

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

- Fig. 1 probably a scatter plot of nighttime  $R_s$  versus  $u^*$  values will better present the inverse relationship

---

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-490, 2016.

Printer-friendly version

Discussion paper

