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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 3 for the taking the time to review the manuscript and for providing helpful comments and suggestions. We provide the following response to the reviewer.

"Major comments: 1. Page 3 line 32-33 – Maybe I understood wrongly, but from my point of view the hypotheses is stated wrongly or not precisely enough. There is written that overestimation of the CO2 fluxes during stable atmospheric conditions was due to insufficient mixing of the air above the soil surface. – do you mind the air inside the chamber headspace or in the open air? This should be clarified. If chamber headspace is considered I would avoid such hypotheses as it is discussed already in several papers that the effect of overestimation of nighttime fluxes is due to broken down the highly stratified layer of air inside the chamber headspace due to chamber movement

at closure (Gorres et al. 2015) or air mixing in the chamber headspace (Schneider et al. 2009, Juszczak et al. 2012), which lead to change of predeployment steady state steep CO2 concertation gradient above the soil due to air mixing in the chamber. However, if insufficient air mixing in the atmosphere is considered then I would avoid to promote any disruptions of this natural condition occurring during calm nights (by excessive artificial air mixing, as authors has suggested, as a possible solution to overcome the overestimation of night-time fluxes) as this again will artificially change the concentration gradients between the soil and the atmosphere and enhance emission of gases, which does not occur during nights with stable atmospheric conditions when the air is highly stratified and when the only process driving emission of CO2 from the soil is diffusion."

We agree that the hypothesis on Page 3 line 32-33 could have been stated clearer. We will correct this, so that it is clear that we mean that overestimation of the CO2 fluxes during stable atmospheric conditions is due to insufficient mixing of open air above the soil surface. We don't fully follow the distinction made by the reviewer between insufficient mixing of air in the chamber headspace and in the free air above the soil surface. Both the effect seen by (Görres et al. 2016) and (Schneider et al. 2009) as well as in our study is due to insufficient mixing of air prior to the measurements and not due to insufficient mixing in the chamber headspace. We discuss how to get unbiased closed chamber measurements during low turbulence (section 4.5, page 13). We argue that mixing of chamber air is a requirement for closed chamber measurements to work, i.e. we want to measure diffusion from the soil to the chamber atmosphere and not between layers within the chamber atmosphere. This makes it difficult to get a reliable flux estimate, as shown in our study, whenever the free air above the soil surface is not well mixed. We agree with the reviewer that using a fan, as well as performing a chamber measurement during low turbulence, will change the concentration gradient and enhance the emission. We think it is important to state that this is without a chamber on the soil. The undisturbed soil CO2 efflux is thus lower during calm nights. Soil respiration may, however, be identical to a period with well mixed condition. By providing mixing of free air with a fan, there is a closer link between soil respiration and soil CO2 efflux, and maybe also between the apparent soil CO2 efflux measured by the chamber and soil respiration.

“2. Page 4 line 9 – could you please specify how dense the canopy is? This seems to be not so important for the paper but it helps to imagine how far a density of the forest canopy may impact the turbulences in the canopy, especially that the sonic anemometer was installed 43 meters above the soil surface. In the ecosystems with a short vegetation (e.g. grasslands), the u* filtering procedure can be applied to separate periods with calm and turbulent atmospheric conditions near the surface, but in a forest canopy with nearly 30 meters height of tree stands this might be more difficult, as under certain conditions the air might not be well mixed under canopy, although there are turbulences in the air above the canopy. Can you be sure that during turbulent conditions (at height of 43 m) there are still turbulences near the soil surface? Question is how dense the forest is? this will help to interpret the data you have. Maybe, it would help if you look for CO2 storage and relate this amount to measured FCO2 from EC? If storage is relatively high the air is not well mixed and may be stratified in the forest canopy”

The annual duration of canopy cover is 180 days with a peak LAI of 5.0. The number of tree stems per hectare is 266. We will include this information on page 4. We cannot be sure that there at all times also are turbulence near the soil surface whenever there are turbulent conditions above the canopy. Earlier unpublished studies show that there is some degree of correlation between turbulence above the canopy and at the soil surface. We appreciate the suggestion regarding CO2 storage. That might have provided some information

“I am not sure but considering the results you got from a fan experiment, which indicated that fluxes measured over the day and night were smaller when artificial mixing was applied (of course it introduce other changes in environment as is discussed in the paper), one may conclude that the near surface air in the forest floor is not well

mixed even during the day when u* calculated based on measurements on 43 meters above this surface is above 1.2. Maybe this explain why there is no any diurnal cycle of CO2 fluxes detected by chamber measurements, especially that there is a weak diurnal pattern of soil temperature which drive respiration processes. This might be also the effect of the time lag between inputs of C via photosynthesis and Rs, as discussed in the paper, but maybe also factors indicated above may impact the measured fluxes for these conditions?”

We agree that it is possible that the near surface air in the forest floor is not well mixed even during day-time. We did detect a slight diurnal cycle in soil CO2 effluxes when a fan was applied, with peak CO2 effluxes during day-time (Fig. 9 page 24), which was in contrast to the diurnal cycle seen during summer of the one year campaign where the highest fluxes were seen during night-time (Fig. 5a page 22).

“3. Page 5 line 25-30 regarding closure time (line5 of page 5) there is written that closure time was 90 and 150 sec in automated and manual chambers respectively, while for a fan experiment you extended the closure time to 5 minutes. Why the closure time was different? In case of manual measurements, the first 20 sec of data points were discarded (due to initial disturbances), while in case of a fan campaign you discarded first 60 seconds: : : this was due to time-leg the air came from the chamber to the analyzer? What was the length of tubes ? Was it the same for all chambers? Regarding fluxes please specify how the fluxes were calculated for automated chambers. Did you calculate fluxes with your R script or you relied on fluxes calculated by LICOR soft? Were the same quality criteria taken into account for all chambers (page 5 line 28-30)?. If not, is that mean that fluxes which does not pass the goodness-of-fit criteria were also taken for analyses? (in manual chambers number of fluxes is small hence the question is what kind of quality criteria were applied in this case). Another point is that the linear fitting was applied to calculate fluxes. This is absolutely correct if closure time is short, but in the case of a fan experiment your closure time was 2-3 times longer than in manual and automated chambers – as described above. I assume that if you used the same small/short LICOR chambers there is for sure non-linear development of CO2 concertation in the chamber headspace. In this case we know that if linear fitting is applied to calculate fluxes then this will lead to significant flux underestimation. Did you consider this effect?"

We used a short closure time of 90 s for the automated chambers measurements during the one year campaign. We wanted to keep the closure time short to get a higher number of flux measurements. The 90 s was found to be sufficient to ensure a high enough increase in CO2 concentration during chamber closure time to provide a solid flux calculation. For the manual measurements with the 10 cm survey chamber we used a longer closure time of 150 s. Experiences from previous years of measurements have shown that it can be difficult to achieve a high flux coefficient of variance (as provided by the LI-COR software immediately following a measurement) during winter when fluxes are low. We have therefore found that a longer chamber closure time is required. We used a longer enclosure time and dead band for the automated measurements during the fan experiment because an external gas analyzer was connected to the LI-8100/LI-8150 system in relation to another experiment. The external gas analyzer required a bigger difference in CO2 concentration during a chamber measurement for precise measurements. This is why a longer chamber closure was used. We found that due to the extra volume of the external gas analyzer, a longer period for the air to be mixed and to stabilize at the beginning of a chamber measurement was required. A longer dead band was therefore used. The tubes between the LI-8150 multiplexer and the chambers were 10-15 m long. The different tube lengths were accounted for in the flux calculation. We calculated fluxes using R. The linear fluxes were calculated using the lm function and the non-linear fluxes were calculated by fitting the non-linear equation suggested by Hutchinson and Mosier (1981) using the nlsLM function. The same goodness-of-fit criteria were used for all flux measurements from the automated chambers. I.e. fluxes with an r2 < 0.95 of the linear regression were removed from further analysis (page 5, line 28). For the manual measurements the quality control was done in the field directly following a measurement. If the flux CV

was higher than 1.4, the measurement was discarded and an extra measurement was performed on the soil collar. We will specify this in the manuscript. We are fully aware of, and agree with you on the general risk of flux underestimation when applying linear regressions to observations (e.g. see Line 20-25, page 2). However, non-linear curve fitting also introduces new potential errors and biases and potentially more variable output. As we focus the current paper on the potential overestimation of fluxes during low turbulence we decided to use linear regression to get robust flux estimates.

"4. Please explain why different approaches were used to deliver annual CO2 effluxes for manual and automated chambers (Page 6 lines 7-23)? I do understand that the number of fluxes measured by automatic system is much higher than from manual one but still data coverage was 76% as you wrote in page 5 (272 days), besides there were for sure also gaps in daily data series. While, what you calculated for 12 subsets was just average daily flux. The missing data for period between 20 May and 22 June were estimated based on linear interpolation between hourly values, although you may use Lloyed and Taylor (1994) model to estimate missing fluxes much more accurate and with less uncertainty (from fig. 6 we know that there was clear seasonal pattern of soil temperature change). I am afraid that the approach used in the paper may bias estimates of annual fluxes. I am not sure if it is not too late now, but I would suggest to first model (with Lloyed and Taylor 1994 equation) the missing effluxes (for each automated chamber) to have a continuous data series of CO2 fluxes (looking for relationships between daily fluxes and T) and then by using u*, fluxes can be divided to 12 different sub-sets. This approach would be more accurate I assume, if you found any relationship between measured effluxes and soil temperature. Or, having so much data you may parameterize Lloyed and Taylor model for such datasets to calculate effluxes for the whole year based on the measured soil temperature. And then, Rreference (at 10oC) or annual fluxes for such subsets might be compared for different u* classes."

We used the empirical model by Lloyd and Taylor (1994) that was parameterized by the manual measurements to deliver the annual soil CO2 effluxes as this is a common way of getting a continuous set of data based on soil CO2 effluxes measured at a low temporal scale. However, the high data coverage of automated measurements allowed us to calculate an annual soil CO2 effluxes based on these measurements directly. We could thus compare two very different approaches. We don't see how calculating an annual flux directly from the automated measurement and filling the data gap with linear interpolation can lead to any directional bias in the annual flux estimate. However we agree that it is possible that a parameterized Lloyd and Taylor could have been used to estimated missing flux values better than linear interpolation. However, we don't see that it could have influenced the difference in the annual calculated fluxes estimates between the different data sets with different u* filters.

"Considering above, please clarify how the annual fluxes were calculated. If the measured fluxes were divided to 12 different subsets (depending on u*), then for sure you had different numbers of fluxes for each class (please specify this information e.g. in Fig. 4). Please specify how the annual fluxes were calculated then? I understood that first data were filtered based on u* and 12 subsets were selected. That means that you had for each day different number of fluxes for each chamber – these might be daytime and night-time fluxes or only daytime or only night time fluxesâĔŸAĔĞThow the daily fluxes were calculated then? Here, I would suggest to look for Rs vs T relationship for each subset and use e.g. Lloyed and Taylor function to model Rs for the whole year. If it is done in other way, I consider this incorrect, especially that we do not know how many fluxes were in each group and from which part of the day. This may impact results described in section 3.2 and 3.3., the whole discussion of results and might be critical point for the analyses presented in the paper"

The different subsets with different u* filters did indeed have a different number of fluxes. Increasing the u* filter led to a lower number of fluxes. We will specify the number of fluxes in each of the binned groups of u* in figure 4. It is true that u* filtering removed fluxes, such that a single day might contain day-time and night-time fluxes or only day-time or night-time fluxes as you describe. However, we accounted for

this when calculating the annual fluxes, such that a calculated annual flux were not potentially biased by days containing e.g. only night-time values. The annual soil CO2 efflux was for each of the 12 subsets calculated as described in line 7-9, page 6. First, a mean flux was calculated for each of the 24 hours of the day. This was done on a monthly basis. This ensured equal weight to all times of the day, even when the u* filtering had removed e.g. night-time fluxes only for specific days of that month. From the diurnal pattern, the mean daily flux on a monthly basis was calculated as the average of the 24 flux values across the day. From the mean daily flux on a monthly basis, the flux for the entire month was calculated by summation. From this the sum of the monthly fluxes gave the annual flux. We consider the method used to give a good calculation of annual soil CO2 efflux for each of the 12 u* subsets. Thus we don't see that any other method (ours including) than an Rs vs T relationship function would be incorrect as stated by the reviewer.

"5. From data you presented is clear that CO2 effluxes are not following soil temperature changes over the day and they are inversely dependent on u*. Of course there is a weak diurnal change of soil temperature at 5 cm depth and mainly it appears during summer, but still I would expect higher fluxes in the afternoon when soil temperature reaches maximum. The filtering procedure you applied lead to significant reduction of the fluxes during nights and slight reduction of daytime fluxes (which also indicates that there were stable atmospheric conditions over the day, as also was indicated by a fan experiment), hence still Rs was the smallest in the afternoon (besides autumn Fig 5j). Considering above I am wondering whether the temperature sensors are installed correctly? Maybe they are too deep? Have you measured soil temperature at 2 cm depth? Is there any relationship between air temperature (near the surface) and soil temperature? If not, then I assume the trees canopy might be so dense that the soil surface is homogenously shadowed, but this may also mean that next to the surface (where chamber measurements are conducted) there might be not much turbulences (this should also be critically discussed in the paper). Please specify in methods how many temperature sensors were installed and how they were distributed over the site

– were they installed in soil collars, next to, or few meters from? – this information is missing although it may help to understand why there is no correlation between measured fluxes and T (on daily basis). I assume this might be autotrophic respiration of tree roots which may dominate your Rs and may not be depend on temperature and if yes, the data analyses would be even more complicated."

We will specify the number of temperature sensors and how they were distributed in the methods section. Soil temperature in figure 3 is the average of 6 soil probes. They are distributed close to the soil chambers such that no soil chamber is further away than 10 m from an individual soil probe. We inserted each sensor 5 cm into the soil and we believe that they are inserted correctly, and not deeper than 5 cm. We did not measure soil temperature at 2 cm depth, but we agree that this could have been interesting to see if this would closer match soil CO2 effluxes. Air temperature at the site is generally highest in the early afternoon and lowest in the morning just around sunrise. The peak in soil temperature peaks a few hours later, which is to be expected from the heat capacity of the soil. We agree that lag times for autotrophic respiration can complicate matters and make the soil respiration look independent from soil temperature.

"6. The fan experiment described in the paper indicated that by mixing of the air during stable atmospheric conditions the near surface air is not so stratified and mixing of the air in the chamber does not lead to overestimation of the fluxes (but only if it is not too strong). I found this experiment interesting but from my point of view this should not be promoted as solution to overcome problems with nighttime chamber flux measurements. It is well known that during calm nights the only process driving emission from the soil is only diffusion, hence we should avoid to increase turbulence by excessive artificial mixing of the air nearby the chamber, as this change the emission of gases from the soil and will lead to increase fluxes which are much smaller when smaller gradients of CO2 occur during calm nights. Another point is how to measure fluxes with chamber over calm conditions. Maybe the application of short chamber is a good solution (as proposed by Gorres et al 2016 – although with this short chambers other problems

appear (as discussed in e.g. Pihlatie et al. 2013), but for sure one of the solution might be to reduce or eliminate air mixing in the chamber headspace to minimize disruption of stratified air in the chamber headspace. The artificial wind may cause also other problems, by changing air pressure inside the chamber by e.g. Venturi effect, or cause excessive latter fluxes which can impact measured chamber fluxes significantly."

We don't promote the use of fans. But we think the method has potential, why we in line 15, page 15 write that "Additional studies are needed to further explore this approach". How to get reliable flux measurements during low turbulence is indeed an interesting question that we also discuss in section 4.5, page 15. Regarding the Venturi effect, the pressure vent on the LICOR chambers is designed exactly to overcome and quickly release any pressure changes caused by changes in wind outside the closed chamber (Xu et al. 2006).

"Other minor comments and suggestions: page 2 Line 18 – there was also a paper of Pumpanen et al. (2004) where rates of over- or underestimation of CO2 fluxes measured by different chamebers are presented in the controlled conditions, it is worth to cite it here page 3 line 14-15, I would not agree that the mechanisms leading for flux overestimation are uncertain. They are well discussed in the cited papers and also in your paper (page 9-10) hence I would remove this sentence. Page 3 line 27 uâ′L°U was measured continuously above the tree canopy – I am afraid that conditions under the tree canopy might be different than those above the canopy, especially that EC system is installed well above the forest canopy (43 m above the surface). See a comment above"

We will cite Pumpanen et al. (2004) on page page 2 Line 18. We will delete the sentence on page 3, line 14, concerning the uncertainty of the mechanisms leading to flux overestimation.

"Page 3, line 12 – please consider also whether to cite the paper of Juszczak et al. (2012) in Polish Journal of Environmental Studies, who compared daytime and night-time Reco fluxes measured manually by chambers and filtered fluxes based on u*, and proved that when proper flux filtering (based on u*) is applied then there is no difference between day and night-time REco fluxes measured by chambers, while they are significantly overestimated when no filtering is applied. This is in agreement with your statements (page 11, lines 19-24)."

Thank you for suggesting the Juszczak et al. (2012) paper. We see that it contains relevant information and we will cite it on page 3, line 12. We will also refer to the paper following our discussion on page 11, lines 19-23.

"Page 4 line 24-26 I am afraid that even if collars are close (for manual and automated chamber measurements) the fluxes are not comparable due to a high spatial heterogeneity of soil respiration flux in the forest floor (due to many factors related to soil itself and distribution of roots, and hence different Ra and Ra/Rh ration)"

We agree that even soil collars very close to each other are not completely comparable due to the high spatial heterogeneity of soil respiration.

"Page 4 line 29 – if any plants appeared in the collar then they were cut or just removed with roots? What about surface layer then?"

The litter on the surface was kept intact, including new litter in the autumn. Tree branches that fell on the soil collars were removed. Otherwise these could physically prevent the soil chambers from closing. New plant shoots were removed by hand. The soil collars were checked at least every two weeks and new plant shoots never reached a height of more than a few cm. The roots (or the biggest part of the root) were most often removed as well when the plant was pulled out.

"Page 5 line 15-20 please specify the height the fans are installed. The chamber you used are rather small/short and I assume fans were just above the soil surface? But this need to be written here"

The fans were installed such that the middle of the fan was 30 cm above the soil

surface. We will include this information on page 5.

"Page 5 line 10-20 Can you please clearly write in paragraph 2.3 what kind of chambers were used and why you extend the closure time in case of a fan experiment."

We will specify the specific chamber models used and why the extended closure time was used. See also our response to "Major comment 3".

"Page 5 line 31, write covariance instead of co-variance"

We will change "co-variance" to "covariance"

"Page 7 lines 25-30 are the rates of fluxes restricted to turbulent conditions, or average of all fluxes is considered? If yes, then it may explain differences you describe (automated measurements combine data measured over the day and night, while manual measurements were conducted over the day (till 3 pm). If you compare fluxes which were filtered using u* then difference between manual and automated fluxes is not so big (Fig. 6b, c). In order to compare fluxes you should rather calculate average flux for fluxes measured in the same period from 9am to 15."

The automated fluxes on page 7, line 25-30 are without a u* filter applied. We compare fluxes measured for the entire day (section 3.2, page 8) and at day-time only (section 3.3, page 8).

"Page 12 lines 34 page 13 lines 1-5 – this was already suggested if I well remember in Rochette and Hutchinson (2005),. They suggested that to avoid overmixing of the air in the chamber headspace the fan speed should be adjusted to outside wind speed."

Thank you for the suggestion. We checked Rochette and Hutchinson (2005) and they indeed suggest matching chamber head space mixing intensities with pre-deployment conditions to avoid biased flux estimates. We will include a reference to Rochette and Hutchinson (2005) on page 12.

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