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

Anonymous Referee #3

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The paper presents the results of one year of automatic and manual chamber measurements of soil respiration conducted in a Danish beech forest and highlights the biases and uncertainties issues related to these measurements. Although I have some critical comments related to applied methodology and data processing I found this paper and dataset very valuable and important for chamber flux community and have to admit that it adds a new hints to a never-ending discussion about the quality of chamber fluxes, measurement protocols, data processing and data filtering. The paper is very well written and structured. However, I have some concerns and a number of suggestions, that I believe will improve this manuscript once addressed.

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

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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.

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

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canopy

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 CO_2 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?

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

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used the same small/short LICOR chambers there is for sure non-linear development of CO_2 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?

4. Please explain why different approaches were used to deliver annual CO_2 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 sub-sets 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 CO_2 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.

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

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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 fluxesâĂŤhow 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

5. From data you presented is clear that CO_2 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.

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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 CO_2 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.

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 CO_2 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 uâĹŮ 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

Page 3, line 12 – please consider also whether to cite the paper of Juszczak et al.

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(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).

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)

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?

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

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.

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

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.

Page 12 lines 34 page 13 lines 1-5 – this was already suggested if I well remember in

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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.

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