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Discussions

Interactive comment on "Carbon balance of surfaces vs. ecosystems: advantages of measuring eddy covariance and soil respiration simultaneously in dry grassland ecosystems" *by* Z. Nagy et al.

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Thank you for your comments on our manuscript. As with the language problems (for I am sorry) we are to ask help of a native speaker to improve the grammar. In the droughted grassland the litter layer was rather shallow, the plant debris average particle size was ca 2-5mm (there are no woody plant species in this grassland) at time of installation (early summer) of the system at the site. The $\sim$5mm insertion depth is a compromise between constraints of avoiding leakiness in one hand and disturbance, on the other.. The design of the system allows to achieve the required low flow rates at

small pressure differences (typically less than 0.12 Pa) between the headspace of the chambers and the ambient air. The litterfall exclusion problem, however, can be important in the longer term. This could be addressed, perhaps, by relocating the chambers at some time intervals, but obvious drawbacks arising from relocation frequency may persist. As with the data in the ms, the period reported is outside of the main litterfall period and it was not necessary to remove the litter prior to chamber deployment. The calibration issue: The basic option was not to calibrate the chambers, and assume that they should be the same as follows from the same design (shape, size). The insertion depths (and also the tilts) are controlled by three supporting rods attached to each chamber. However, minor differences may (and according to the calibration coefficients, do) exist between the chambers, and this was the reason to carry out the calibration procedure. The description of the eddy covariance data processing unfortunately included all the procedures applied since the station's setup in 2002. Until 2005 we have used a Gill sonic when it was replaced to a CSAT3. Prior to replacement crosswind correction and 3D rotation was applied to the data. The CSAT3 data, however were not crosswind corrected. Also we changed from 3D rotation to the planar fit method when post-processing (but are not applying both to the data). We will correct the methods section, accordingly. The authors were (and are) not at all to "attack" the closed system in general. Our aim was to develop a system, designed for automated measurements, suitable for unattended use at a remote location and adapted to the characteristic gap size of the grassland community (also to minimize disturbances). No commercial system exist which would be usable for measuring real soil CO2 effluxes without clipping of above ground biomass, for measuring the small vegetation gaps between grass tufts. When performing the calibration procedure, we assumed that the efflux from the calibration tank is the standard. The calibration therefore yielded not just the coefficients for the chambers, but also shows that the agreement between the "true" efflux and the flux measured by this system is reasonably good. In other terms, the coefficients (the slopes of "true" vs measured fluxes) are close to unity, also showing that, there were only very small differences between chambers of the new system.

Had we compared this system to another one (and not to the expected flux from a standard) on an inherently heterogenous media as soils usually are, we probably would have received less reliable results. The comment, "From the data presented in the manuscript, the authors' implied assertion that these two approaches (small open-top vs larger closed chambers) to soil CO2 flux measurements are different is completely unsupported." suggest that the referee perhaps consider our soil respiration system as a closed system. It is not a closed, but an open steady state system, i.e. we are measuring the CO2 concentration difference between the reference air, entering the chamber and the air sample, leaving the chamber and not the rate of CO2 concentration increase within the chamber. We think that the two approaches (steady state open vs non steady state closed systems) are both expected to yield reliable data. We agree with referee that both approaches are good and comparable in "standard conditions" on bare soil. Our approach (open, steady state system) should perform better in grasslands with small gaps, because building a small chamber to be operated in a closed (non steady state) system with automatic lid opening and closing is complicated, mechanically unstable and expensive. Finally, I thank for the useful comments, especially for realizing the mistake in the eddy method description.

GödöllÅŚ, 2011-03-17.

in behalf of the authors Zoltan Nagy

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