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Biogeosciences 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.

Anonymous Referee #1

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This is a review of the paper "Carbon balance of surfaces vs. ecosystems: advantages of measuring eddy covariance and soil respiration simultaneously in dry grassland ecosystems" by Nagy, Pinter, Pavelka, Darenova, and Balogh.

I found this a difficult manuscript to read. The organization was inconsistent and often, the grammar was such that the author's intention was not clear. While I understand the difficulties of writing in a language that is not native to the author, correct and consistent grammar is essential to getting the intended point across in an English language journal. I suggest that the authors seek an outside reviewer who can help correct this

aspect of the manuscript.

Scientifically, this is an interesting paper; however it is not clear exactly how much value the small diameter open chambers represent. While it is true that they can be placed more accurately and without some of the problems associated with larger, closed chambers, they do introduce their own unique set of problems. My biggest concern is how the surface litter is treated in the installation and maintenance of these devices. Several groups have noted that surface litter can be one of the largest contributions to surface CO_2 flux. If, we strictly define soil surface flux to exclude the litter, then the chambers are probably adequate to measure the local values. If, however the litter is included, there are several potential problems. First, the shallow installation depth seems to require the removal of litter. Installation of 3 to 5 mm would not penetrate the litter layer in many ecosystems, and could be problematic on rough ground or where larger forbs and woody plants grow. This would result in a very leaky seal. Secondly, the design of the chambers seems to prevent the accumulation of fresh litter in the measurement area. This will drastically affect the carbon balance within the chamber. Anecdotal studies have shown that fresh litter can decay very rapidly, especially in semiarid environments during episodic wet periods (i.e. after precipitation). One can't tell from the manuscript how the authors treated the litter in their study, but since they make direct comparisons of their chamber fluxes to eddy covariance fluxes, one must assume that litter is somehow included in the chamber. Is it possible that the unique character of their site allows a simple installation without disturbing the litter? Again it is impossible to tell from the manuscript.

Regarding the calibration of the chambers, this is an interesting aspect to the work. I do, however have reservation about their use of "calibration coefficients", that are unique to each chamber. In table 2, the authors list the slope of a regression line for each chamber, and state that these factors were used to correct the measured fluxes from these chambers. The question that arises is how unique are these numbers to the particular chambers? Could these small factors be due to slight differences in placing the chambers on the calibration surface (different tilts, different insertion depths, other factors)? Are these slopes repeatable for each chamber? It would seem more appropriate to average the 4 slopes.

I also have a few concerns about the reported processing of the author's eddy covariance data. They state that they corrected the sonic temperature from their Campbell Scientific CSAT-3 for cross-wind contamination according to Liu et al. This reviewer has researched numerous sonic anemometers and in conversations with the manufacturers, has found that only the Gill WindMaster Pro sonic anemometer does NOT correct for this effect internally. Apparently, they have done a double accounting for the cross-wind effect. Additionally, they state that they have used the planar fit method of Wilczak to remove apparent mean vertical wind components. They then go on to state that they have done a 3-dimensional coordinate rotation. These two processes are different approaches to a common problem. The planar fit takes a long-term approach and requires months of data while the coordinate rotation takes a short-term approach and only requires 30 minutes of data. I have not done the calculations necessary to prove this, but by doing the coordinate rotation, they probably have effectively removed any benefit of the planar fit procedure. Only one of these treatments should be used to process wind velocity data. Further, it has been shown that by completing all three of the possible coordinate rotations, one only adds noise to the resulting fluxes. If coordinate rotations are to be applied, it is most appropriate to first rotate the coordinate system into the wind (i.e. zero the mean cross-wind component), then apply a second rotation to zero the mean vertical wind speed. Either I have misunderstood the procedure that the authors attempted to describe or they have over corrected their eddy covariance data. In either case, this must be corrected.

Finally, although it is not explicitly stated in the title, the authors give the design of their small, open-top chambers a prominent place in the abstract and in the body of the manuscript. They explicitly state the desirable characteristics of this design relative to traditional larger closed chambers. Unfortunately, there is no comparison of their de-

vices to traditional chambers. From the data presented in this manuscript, we have no basis to evaluate how well this design performs relative to more traditional systems like the LiCor LI-8100, the PP-systems SRC-1 or the ADC ACE. 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.

Interactive comment on Biogeosciences Discuss., 8, 941, 2011.