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Biogeosciences Discussions

Interactive comment on "Ventilation of subterranean CO_2 and Eddy covariance incongruities over carbonaceous ecosystems" *by* A. Were et al.

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The authors would like to thank the enriching and encouraging comments of both the referees and the editor. We are convinced that with their suggestions the manuscript will improve and be suitable for its submission to Biogeosciences.

Here are in detail our analysis and replies to the different comments made by the referees and the editor. In a pdf document accompanying this response are listed the changes done in the manuscript according to each of the referees' and editor's comments.

REPLY TO THE COMMENTS OF REFEREE # 1:

1): Equation (2) on page 10919 uses |F_1| as the metric or normalization for the calculation of E_r (epsilon_r). But I wonder if a better normalization factor would be |F_1| + |F_2| for CO2 fluxes and (F_1 + F_2) for the ET fluxes?

The authors agree with the referee that the use of the average of F_1 (or |F_1| in the case of the CO2 fluxes) may not be the most appropriate factor for the normalisation or standardisation of epsilon_r. However, we think that the use of F_1 + F_2 (or the absolute values in the case of CO2 fluxes) as a standardisation factor may lead to an erroneous interpretation. The values of F_1 and F_2 represent an average of the flux of mass (either CO2 or water vapour) per time, over a 30 minute period. Therefore, being averaged values, their addition with a simple sum has no physical sense. As an example, in the case of evapotranspiration, if for a certain 30 minute period F_1 = 100 Wm-2 and F_2= 80 W m-2, than F_1 + F_2 = 180 W m-2, meaning an averaged flux of 180 J s-1 m-2 over a 30 minute period, which is not a real value, leading to a lower epsilon_r, but not realistic.

The root mean square error (epsilon) is used to compare the measured fluxes of the two Eddy covariance (EC) systems. However, as there is no evidence of which of both EC systems is measuring the "true" value of the fluxes, we should be talking of a root mean square difference, instead of error. The equation for calculating it (Eq.1 in page 10919) does not change, however the concept is slightly different, and we consider it more correct, and so we have changed it in the corrected manuscript (see changes at the end of this reply). Therefore, for calculating the relative root mean square difference (epsilon_r), we think that a more appropriate factor would be the average of the values measured by both EC systems, in order to have an idea of the magnitude of the values being measured in the area, and therefore, an idea of the magnitude of the differences found between the measurements of both EC systems.

Equation 2 has changed to (see list of changes):

epsilon_r = {epsilon/((F_1+F_2)/2)}*100 where F_1 and F_2 are averages for the period studied.

As before, in the case of the CO2 fluxes, the average of F1 and F2 has been done with the absolute values, in order to avoid the effect of the presence of positive and negative values.

The recalculated values of epsilon_r have been introduced in Table 2 (see list of changes).

REPLY TO THE COMMENTS OF REFEREE #2:

1) p. 10914, line 12. I do not like “caves” as a synonym for “macropores.” I would replace“caves” throughout the text with “cavities,” which is a more reasonable term. A cave implies an opening big enough for a person to enter.

The authors thank the referee for this suggestion and we have replaced “caves” by “cavities” throughout the text.

2) p. 10916, line 12. Does LE represent latent heat or evapotranspiration or both? Clarify.

In this manuscript, we considered the evapotranspiration measured by the Eddy covariance systems in its energy form, and therefore denoted it as LE. To avoid any confusion with these concepts we have changed page 10916, line 12 to: “Measurements of CO2 flux (Fc), evapotranspiration (also referred to as latent heat flux, LE) and sensible heat flux were carried out using. . ..”.

3) p. 10916, line 25. After “mid-October,” mention the year that these systems were installed.

We have added the year, which was 2007.

4) p. 10917, line 7. The address should be presented as “Hayward, CA, USA.”

We corrected this error in the manuscript.

5) p. 10917, line 16-17. “Within or outside the 50 % source area boundaries” would cover the Earth. So it is not clear what you are saying with this statement. It is also not clear what maximum source weight means.

The footprint theory in which is based the Flux source area model (FSAM) by Schmid (1994, 1997), considers that a sensor at a given height, measures an upwind source area of a certain dimension. However, the relative weight of the different points within the source area on the sensor measurements is variable, and follows the footprint distribution function (see Figure 1 in Schmid, 1994 and 2002). According to this function, the relative source weight rises to a maximum with increasing distance from the sensor and then falls off again to all sides as the separation further increases (Schmid 1994, 2002). The FSAM model calculates the footprint function in the horizontal plane, calculating the minimum area responsible for a given % of the total source weight. According to the footprint function, there is a source point that has the maximum relative source weight of all the source area of the sensor and, therefore, this point is the point of maximum source weight (the horizontal projection of the maximum point of the footprint function, fmax). If we select the source area responsible for the 50% of the total source weight (that we call the 50% source area), we ensure that the point of maximum source weight is within it, and any point on or outside the boundary of this source area should be 5 to 10 times stronger (in terms of influence on the sensor measurements) than a point at the maximum source weight location (in the centre of the source area) (Schmid, 1997). Therefore, for the purpose of this manuscript, where we want to analyse the differences in the dimensions and location of the source areas of each EC system, it is more relevant to use the 50% source area, than the 90% source area, even though the latter is much larger.

We are aware that the paragraph from lines 10 to 20 in page 10917 can be confusing for readers not familiar with the footprint analysisand so we have changed it in the revised manuscript (see list of changes).

References

Schmid, H.P.: Source areas for scalars and scalar fluxes. Bound.-Lay. Meteorol., 67: 293-318, 1994.

Schmid, H.P.: Experimental design for flux measurements: matching scales of observations and fluxes, Agr. Forest Meteorol., 87: 179-200, 1997.

Schmid, H.P.: Footprint modeling for vegetation atmosphere exchange studies: a review and perspective, Agr. Forest Meteorol., 113: 159-183, 2002.

6) p. 10920 (text) and p. 10933 (Fig. 5). I have a lot of trouble understanding Figure 5. In the Fig. 5 caption, you refer to 50% source areas of EC1 and EC2, but both figures have four circles, not two. Are these boundaries for 50 and 100%? Please clarify.

Figure 5 is a schematic representation of the approximate footprint of each EC1 and EC2 according to the dimensions of the source areas calculated with the FSAM model. These dimensions include the distance from the sensor of the near-end and the far-end boundaries of the source area. At a given wind direction, the dimensions of the source area are mainly dependent on the atmospheric stability conditions, represented by the stability factor $(zr-d)/L$. According to this, as the stability conditions get more unstable (corresponding to smaller values of $(zr-d)/L$), the size of the source area gets smaller, and it gets closer to the sensor, meaning that both the near- and far-end boundaries of the source area get closer to the sensor. The opposite occurs when the conditions are more stable (higher values of $(zr-d)/L$).

In this work, we differentiated between data with unstable conditions ($(zr-d)/L<0.01$) and data with near neutral conditions ($-0.01<(zr-d)/L<0.01$) for each EC system. In each group of data we calculated the minimum and maximum value of $(zr-d)/L$, and with them we calculated the dimensions of the 50% source area. This ensures that we calculate the dimensions of the smaller and larger 50% source areas possible for each group of data. Therefore, Figure 5 shows the minimum near-end (smaller circles around the sensor) and maximum far-end (larger circles around the sensor) of the 50% source areas for every wind direction (hence the circle), for each group of data

(unstable and neutral conditions), and for each tower. Thus, the area comprised within both circles is the area that includes all the 50% source areas of each Eddy covariance system (white for EC1, and white with black stripes for EC2), for every wind direction and stability condition found during the period studied.

We have made several changes in the manuscript to make this item more clear.

7) p. 10921, lines 3 and 6. No idea how to visualize the “up to 30 m” and the “less than 9 m” images in Fig. 5.

As it was already pointed out in the previous reply, the footprints of each EC system are overlapped, however for some wind directions the far-end boundaries of the footprints do not coincide (external circles in Fig. 5). For the unstable atmospheric conditions, the distance between the far-end boundaries of each EC system can be up to 30 m (see the 50 m scale in Fig. 5). In the case of periods with neutral atmospheric conditions, the separation between the far-end boundaries is less than 9 m.

Changes in the manuscript and in Figure 5 caption have been made to clarify this.

REPLY TO THE COMMENTS OF THE EDITOR:

MAJOR COMMENTS:

1) I would like to see an additional statistical analysis of the data, in particular tests of whether slopes and y-intercepts of linear regressions are statistically different from unity and zero, respectively, and whether slopes and y-intercepts of linear regressions differ statistically significantly between EC1 and EC2 - currently this analysis is relatively vague.

We have completed the statistical analysis, following the editor’s recommendation, by testing how significantly different from one and zero were the slopes and y-intercepts obtained from the linear regressions obtained comparing the CO_2 and evapotranspiration fluxes of EC1 and EC2. To do this, we used the R programme (R development core team, 2008) to test if there were significant differences between the simple regression obtained and: i) a linear model where the y-intercept was 0, and ii) a linear model where the slope was 1. For the first test, we used a t-test performed in the simple regression model, where the significance (p-value<0.05) indicates that there are significant differences between both models and, therefore, the y-intercept is significantly different from 0, and a non significant (p-value>0.5) result indicates that for the data used we can assume that the y-intercept is not significantly different from 0. For the second test, we performed an ANOVA test, comparing the regression model and the linear model with slope = 1, where significant differences between both models indicate that the slope of the regression model is significantly different from 1. The significance of the p-values obtained in each test, as well as the significance of the R2 of the regression, was added in Table 2(see list of changes).

These new results corroborate the results showed in the manuscript. According to the values of the slopes of the regressions, these are not significantly different from 1, when comparing LE from EC1 and EC2, and when comparing Fc for the biological period. This indicates that the measurements of EC1 and EC2 are very similar, being significantly close to a 1:1 relation. Although the y-intercepts are significantly different from 0, their magnitude is small (2.5 W m-2 in the case of LE and 0.3 umol m-2 s-1 in the case of Fc). However, when comparing Fc for the whole period studied and for the abiotic period only, the slopes of the regressions are significantly different from 1, and the y-intercepts are significantly different from 0. These results indicate that there are important differences in the measurements of Fc between both Eddy covariance systems, corroborated by the root mean square errors.

These new results have been added to the manuscript.

2) I would like to see a more thorough discussion of the assumptions and limitations of the used approach; in particular regarding (i) the use of CO2 and H2O as tracers, (ii) differences in above-ground footprint heterogeneity between biotic and abiotic periods, and (iii) the authors might also think about what their conclusions would have been in case their analysis had revealed that both EC systems measured the same fluxes

during abiotic periods, which might theoretically occur in case below-ground sources of CO2 of varying strength compensated each other within the footprint.

i) In this work we address the importance of the CO2 stored in the pores and cavities of the sub-surface of carbonate ecosystems to the net exchange of CO2 between this surface and the atmosphere. Through the ventilation of the pores and cavities located underneath the surface, the CO2 stored can outflow to the atmosphere. During periods of little or no biological activity, due to low water availability and high temperatures, this CO2 outflow can be responsible for a high CO2 release flux to the atmosphere, making the ecosystem behave as an important source of CO2 during these periods. In the case of the water vapour flux from the ecosystem to the atmosphere, also referred to as evapotranspiration, we have considered it as a control value to compare to the CO2 flux, because the evapotranspiration originates at the surface of the ecosystem through the evaporation of the soil and vegetation surface, and the transpiration of vegetation. If there is an outflow of water vapour from the underlying pores and cavities accompanying the CO2 release during the ventilation processes, it has not been referred to in the literature and we have not detected it in our measurements with the Eddy covariance systems.

We have included these comments in the discussion of the revised manuscript.

ii) The footprint analysis performed in this work considered the whole data set, including both the biotic and abiotic periods. However, an analysis of the wind directions and the atmospheric stability conditions indicates that there were no important differences in the footprint of the EC towers between the biotic and abiotic periods. During the biotic period 86.3% of the data corresponded to unstable atmospheric conditions, 53.9% of the wind directions came from the North to East directions and 23% came from the South. In the case of the abiotic period, 95% of the data corresponded to unstable atmospheric conditions, 76% of the wind directions came from the North to East directions and 15% came from the South. If we observe Figure 5 in the case of the unstable conditions, more than 50% of the data for the abiotic and biotic periods came from the wind directions where more discrepancy between the footprints of both towers can be found. Although there is an important percentage of data during the biotic periods with wind directions coming from the South, where the differences between the footprints of both towers are lower, we do not think that this can have a significant importance in the overall results and conclusions obtained in the manuscript. However, we have included these results in the manuscript.

iii) An absence of differences in the CO2 fluxes measured between the EC towers during the abiotic period (and therefore, during the whole studied period, as the differences found are mainly due to what happens in the abiotic period) could be explained by a compensating effect between different outflow points with different source weights within the footprints of both towers. However, what we think is interesting in the findings of this manuscript is that there are differences during the abiotic period, highlighting the importance of the sub-surface source of CO2 in the measurements of the CO2 fluxes at ecosystem level. If no differences would have been found between the CO2 fluxes measured by the two towers, even though an outflow of CO2 from the sub-surface would be happening, there would be no evidences of this outflow.

MINOR COMMENTS:

1) p. 10914, l. 24: or more generally "the underlying surface"

We agree with this comment, and we have replaced “underlying vegetation” by “underlying surface”.

2) p. 10914, l. 26: all EC towers?

Effectively, the word “towers” is missing after “EC” and it has been added.

3) p. 10917, l. 23-26: aren’t this 6 original 3 composite input parameters?

Indeed the input ratios of the FSAM model are composite of 6 parameters. Therefore, to avoid the confusion, we have refered to the input parameters of the FSAM model as “three dimensionless ratios”.

4) Fig. 1: if possible use a color image - the b&w version does not read very well (note that color figs are free of charge with BG!)

This figure has been downloaded from the black & white ortoimage of Andalusia, supplied by the Government of Andalusia. Unfortunately, there are no available ortoimages in colour with enough resolution to be suitable to show the position of the two EC towers, as it is shown in Figure 1. However, we have increased the contrast of the image, and we have changed the colour of the lines and text of the figure in order to make it clearer. Moreover, we have inserted an image where both EC can be seen, as well as the area studied. We hope that these changes will be suitable for the publication in Biogeosciences.

5) Fig. 2: use different colors and line types for EC1 and EC2 - the grey and black lines are hard to distinguish.

We have changed this in Fig. 2. See list of changes.

Apart from the changes proposed by the editor and referees, we have also replaced in the revised manuscript the word “carbonaceous” by “carbonate” throughout the text, including the title, as this term is more correct in reference to ecosystems, soils and substrates with a carbonate rock origin.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/6/C4421/2010/bgd-6-C4421-2010-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 6, 10911, 2009.

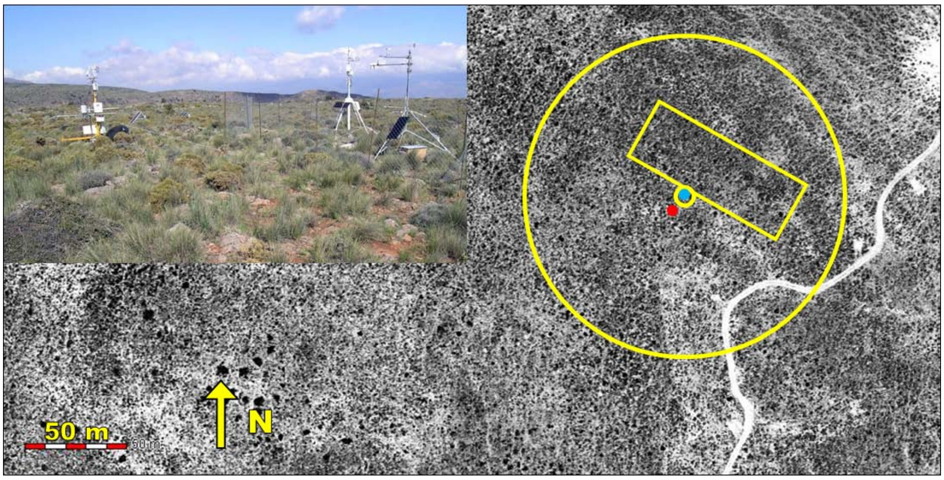
50 m
N

Fig. 1.

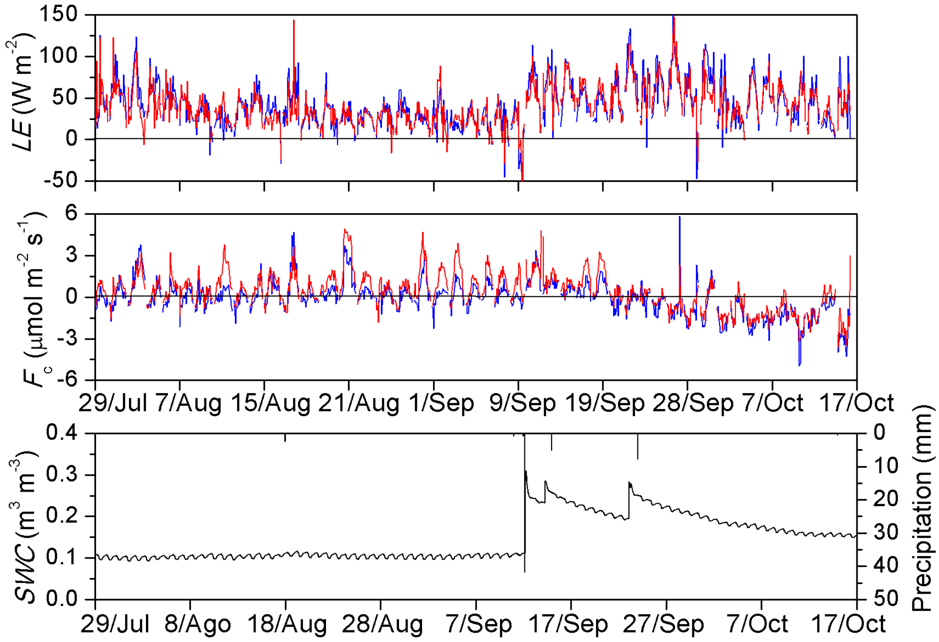
LE (W m-2)
Fc (µmol m-2 s-1)
29/Jul 7/Aug 15/Aug 21/Aug 1/Sep 9/Sep 19/Sep 28/Sep 7/Oct 17/Oct
SWC (m3 m-3)
Precipitation (mm)
29/Jul 8/Ago 18/Aug 28/Aug 7/Sep 17/Sep 27/Sep 7/Oct 17/Oct

Fig. 2.