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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Partial energy balance closure of eddy covariance evaporation measurements using concurrent lysimeter observations over grassland" *by* Peter Widmoser and Dominik Michel

Anonymous Referee #2

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This topic is very relevant, as surface energy non-closure is still one of the outstanding problems in micrometeorology. Lysimetry may indeed be one of the techniques that can help to shed more light on this fundamental problem. Interestingly, and as a byproduct this technique brings together the worlds of hydrology and micro-meteorology, which is very welcome.

Here follows my main critics, which then will be more detailed in specific points below. At the end more textual comments and suggestions are given.

1) A lot of results are presented in this paper, but often without much comments by the authors. In that respect the paper looks more like a technical report which may form the basis of peer-reviewed paper. I urge the authors to take the reader along the circa eight tables and describe in text what the main message of each table is.

2) Although the authors focus on the relation between LY and EC measurements, they also use the other observations of the surface energy balance, net radiation and surface soil heat flux in essential parts of their analysis. These observations should be described aswell in section 2.

3) A rational for the used method is lacking, given that the authors state that they are mainly interested in the relation of LY and EC evaporation observations.

4) Section 4 is more a summary of results then a discussion. For example the part on standard deviations needs a discussion on what these comparison of SDV means and what can be learned of it. Now there are so many nice statistical results and apparently so little conclusions can be drawn. The question is whether these statistical techniques alone are sufficient to grasp the mechanisms behind the differences observed. Perhaps these should be accompanied by detailed case to case studies.

5) The text is not always as precise at it could be, some examples are given below. But there are more of these occasions. Please copy edit the text carefully on this aspect.

Specific comments

6) L45: How is the evaporation fraction used to correct? Is that different from Bowen ratio preservation?

7) Table 1: It would be nice to have the other information (measurement time interval, vegetation type, period of the day used) also in the table. This may require to turn the table by 90 degrees.

8) What would be the influence of the oak trees at station Majadas on the flux observations.

9) S2.2: Here general error characteristics are given. Are the authors sure that these can applied to the various sites used here. Are there any specific circumstances which may have an influence on the error characteristics. For example, how well are the conditions in the lysimeter kept comparable to the surrounding fields. Are there infrared surface temperature observations to judge possible inhomogeneities between lysimeter and surroundings?

9) S2.3: Here I have the same questions. I find these error estimates to general. It is always good to look at specifics of datasets/sites. EC measurements require all kinds of corrections. I miss a statement on the applied methods, and any differences in treatment per site.

10) L113: Wohlfart and Widmoser (2013) apply the out-of-bound concept for corrected EC observations to judge whether this corrections lead to physical realistic values. Here you apply to the uncorrected EC observations, which may be physically unrealistic as this is the reason that you want to correct. Please clarify.

11) S2.5: To calculate the energy imbalance (epsilon) the authors also needs the available energy which is built from net radiation and the heat stored into the vegetation-soil system. I miss in section 2 a description of these observation for each site including error characteristics. It would greatly help when for each site a characteristic diurnal cycles are displayed of the components of the energy balance and the resulting imbalance (epsilon). This then should include a discussion if any peculiarities show up in these observations.

12) S2.5: The authors state that they are not interested in analyzing the full energy budget, but only the evaporative component. Alternatively the authors could have chosen to analyze the relation between the lysimeter and EC measurements. It would be nice if the authors could discuss the arguments for choosing not to follow that line.

13) Table 2 – 5: Only very little comments are given by the authors to these eight tables. Some more wording to guide the reader towards important points to learn from each

table would be very helpful.

14) Figure 1a and 2a: The larger differences in the morning in fig 1 have disappeared in figure 2a. This must be related to the diurnal characteristics of epsilon. Addition of epsilon in fig 1 and discussion would be helpful.

15) S3.7 In section 2.5 the authors describe a binning procedure of the LE data for regressing and obtain wL. How does this relate to figure 5 where averages of wL per hour are given. Some extra wording would be helpful.

16) L267: Bins ranging from 6 to 14. Is this the number of observations in each bin. Please be precise.

17) L273: Standard deviation in wL will among others depend on the statistical noise in the EC measurements. These can be large under convective low wind conditions during day time, and lower under the less convective conditions around sun rise and sunset.

18) Figure 6b: what is the meaning of _s11 in the labels?

19) Figure 6b: there is a remarkable drop in wL observed in the figure, but not mentioned in the text.

20) S3.9: Please explain what the value of these correlations are. One question that comes to my mind is: the authors use the result of the regression (wL and cLE) and look at their correlation. What can we learn from this?

21) L315-319: What conclusions can be drawn from the summary of these results on standard deviations?

22) L345: See my comment #17

23) L347: “one might conclude that the high standard variations are rather related to weather conditions”. Where is this conclusion based on?

24) S5, L352: I would say that the best adjustment of EC to LY would be a direct regression of without the complications of epsilon and the full energy balance. And if this is the aim, why not use LY and refrain from EC?

25) L358: Note that also the statistics of EC observations will be come progressively worse when going to smaller time intervals. But combining scintillometry and EC-observations might be a way forward.

Textual comments:

26) L14: “At the overall average” -> “Overall” 27) L15: “which were partially closed with” -> “after applying “ 28) L16: “remain high differences” -> “remain large differences” 29) L18: “correction evaporation weights”. This looks like a defining term, but is never used in the main text, please be concise on terminology. 30) L19: “correcting evaporation weights”. Yet another formulation never used in the main text. 31) L29: How is the energy balance gap defined? I would expect a value of 22-27 % for the magnitude of the gap. 32) L30: A comparison alone cannot lead to any reduced difference. I guess it is the adjustment of EC measurements with LY measurements that leads to this reduced difference. 33) L31: How do this percentages relate to the values of 73.2 and 78% on line L29. 34) L35: “with” -> “of” 35) L36: “an influence of the increasing plant height as against constant measurement height is suspected.” Unprecise wording, please correct. 36) L38: is -17% to -19% on a daily basis? Please be precise in formulation 37) L65: Textually it would be nicer to start with some of the general information given below the table 1, and then introduce table 1. 38) Table 2b: Some numbers are out of place in the last column, it seems. 39) L281: 212 weeks?

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