

## ***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 #1**

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General comments.

The manuscript is relevant for hydrological studies. The energy balance gap in eddy correlation (EC) measurement is an ongoing topic that deserves attention. The manuscript is well written. It is however rather brief and seems to be primarily readable for insiders. I am personally in favor of these short and concise manuscripts. It has the characteristics of a technical note. The editor could consider to publish it as such.

Major comment.

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Throughout the manuscript  $d$  is considered to describe a systematic difference between LY and LE. Lysimeter measurements are systematically larger than EC measurements. However  $d$  can be negative or positive for different sites. Stating that  $d$  describes a systematic difference is confusing and not correct.  $d$  is simply the intercept of the linear regression model having the energy balance gap (epsilon) at the x-axis and LY – LE on the y-axis. If the energy balance gap (epsilon) is zero  $d$  will remain. We could even argue that we could drop  $d$  in the model since cLE does already quite a good job in correcting LE (table 3a in the manuscript). The description of  $d$  and the conclusions based on  $d$  should be corrected throughout the manuscript.

Specific comments.

1. The formulation of line 30 to 33 is unclear to me. Was just the difference between LE measured with EC and LE measured with lysimeters smaller than the energy balance gap? I would suggest to reformulate this part and explain “reduced the differences”.
2. In line 37 partial evaporation closure is mentioned. Shouldn't this be partial energy balance closure? It is not clear what is meant.
3. In the equations the dimensionless weights are in the form  $w_A$ ,  $w_H$ ,  $w_L$ . I find this confusing. I would suggest to use subscripts for A, H and L. Otherwise I could interpret  $w_H$  as a weight times sensible heat, which is not the case.
4. LY is used for lysimeter LE in the equations. This is confusing. I think the notations should be reconsidered.
5. In line 312 the difference between LE and LY for humid climates is described as surprisingly little. I think this is not correct. The difference is large. 10 to 30 W/m<sup>2</sup> is similar to 0.35 to 1 mm/d which is equal to 128 to 365 mm/year. On a water balance in most regions of the world including Europe these differences are large.
6. The formulation of line 320 to 325 is unclear. “The adjustments reached in this paper are higher”. Did the corrections/adjustments lead to better results? How come? If I

C2

am correct the literature citations in these lines have used full energy balance closure techniques with still large differences with lysimeter measurements right? (I haven't checked) This is something different.

7. Line 352. To my opinion better to reformulate this line. The presented manuscript is basically fitting a certain model, but that doesn't tell anything about what is best.

8. In line 359 the authors suggest to use 5 to 10 min resolution lysimeter data. I think this is unrealistic. There are no lysimeters other than dead weight compensated lysimeters that can measure accurately at such a fine resolution. Even presenting data on hourly intervals is to my opinion debatable. I would rather suggest to do the analysis on daily data or the sum of daytime data. The analysis would then be much less affected by lysimeter measurement errors and as proved in manuscript the correction weights for most situations are constant during the day.

Technical corrections.

9. Typo: At the end of line 247 the word "und" should be "and".

10. Figure 7a. Legend item "zero line" should be brown.

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