

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

**Peter Widmoser and Dominik Michel**

dominik.michel@env.ethz.ch

Received and published: 5 August 2020

We thank referee #1 for the comments. We respond to them below the referee's comment (*italic*).

General Comment:

*The manuscript is relevant for hydrological studies. The energy balance gap in eddy correlation (EC) measurement is an ongoing topic that deserves attention. The*

C1

*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.*

The positive feedback on the importance of the topic as well as on the writing is much appreciated. We leave it to the Editor to publish the paper as a technical note.

Major Comment:

*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.*

We changed in line 145 from “the y-intercept ( $d$ ) can be interpreted” to “might be interpreted”.

We will, however, not correct the description of  $d$  and conclusions drawn from it. We have the following arguments:

- Our interpretation is correct from a statistical point of view
- Even if the interpretation of  $d$  may be irritating in the case of negative  $d$  as e.g. for Fendt 2013, we feel this irritation is helpful for a more detailed analysis of the data
- The new method proposed has been applied so far only to four stations. Further experience with data evaluations from more sites may decide about the  $d$ -interpretation

C2

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".*

This refers to the difference between EC and lysimeter data. The formulation was indeed unclear, as the numbers referred to the case without forced closure, which is confusing with the previous sentence. We reformulated lines 30 to 32 accordingly: After forced closure of the energy balance, the difference between daytime LY and EC data on two fields could be reduced from -28.8% to 6.2%, respectively from -26% to -12.3%, with an accuracy. . .

2) *In line 37 partial evaporation closure is mentioned. Shouldn't this be partial energy balance closure? It is not clear what is meant.*

We corrected in line 56 (not line 37) to "partial energy balance closure".

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 interpreted  $w_H$  as a weight times sensible heat, which is not the case.*

We feel it might be useful to apply the weight-notation in agreement with two previous publications in AGRFORMET. We leave it to the Editor to decide whether to change the weight-notations.

4) *LY is used for lysimeter LE in the equations. This is confusing. I think the notations*

C3

*should be reconsidered.*

We change the notation for lysimeter- and EC-values to  $LE_{LY}$  and  $LE_{EC}$

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.*

We changed the sentence on line 312 to: "They differ much less for humid climate with around 10 to 30 Wm<sup>-2</sup> (0.35 to 1.0 mm d<sup>-1</sup>) than at Majadas-Station with around 30 to 60 Wm<sup>-2</sup> (1.0 to 2.1 mm d<sup>-1</sup>)".

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 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.*

We replace line 320 by: "The effectiveness of our method is demonstrated by comparing our results given in Table 5 with the following results achieved by former authors:"

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.*

We changed the sentence on line 352 to: ". . .gives, according to our knowledge, for

C4

the first time a fully rational method to partially close the energy gap and a more detailed description of the correlations between LY and EC-observations."

*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.*

We can understand the reasoning of the referee. But we still feel it is justifiable to at least propose reduced time intervals. Higher time resolution of LY-data could be easily retrieved. However, thorough filtering of the data is required for high resolution data, e.g. using the AWAT filter (Peters 2017, <https://doi.org/10.1016/j.jhydrol.2017.04.015>). As far as our method is concerned, Eq. 3 relies on statistics and its reliability is as such dependent on a high input number.

Technical corrections

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

We corrected the error in line 247.

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

C5

We changed the color of the zero line in Fig. 7a.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-299>, 2020.

C6