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# *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

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We thank for the careful, profound and challenging review of reviewer #2, which underlines our intentions (1) to draw attention to the subject and (2) demonstrate that the method proposed gives plausible results under various local conditions. We acknowledge their input on the discussion on the standard deviations, which made the manuscript more concise.

Some comments suggest adding additional figures and tables. We are, however,



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concerned about the length of the manuscript. Nevertheless, we will do our best to respond to reviewer 2 (italic). In case we cannot fulfill all expectations, we suggest to publish the paper as a technical note. We respond to the referee's comments (italic) below.
Note that the new nomenclature for the variables (e.g. $LE_{LY}$ instead of $LY$, as changed upon input from Reviewer #1) is not yet used here for simplicity.

*Main critics:*

*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.*

We changed the text accordingly at several places, see responses below.

*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.*

At the end of section 2.2 we add the information on instrumentation along the lines of: EC fluxes were measured with CSAT-3 ultrasonic anemometers (Campbell Scientific Inc., USA) and LI-7500 infrared gaz analysers (LI-COR Biosciences, USA) at Fendt, Graswang and Rietholzbach. . ..
We are still collecting the necessary details.

*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.*

We want to demonstrate the applicability of approach described by Widmoser et al (2018) to several stations as mentioned in the abstract. In our opinion, the rationale for this study is made very clear.

*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.*

It is beyond the scope of this article to grasp all mechanisms behind the differences observed. We explain the meaning of SD-differences. See comment 21.

*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.*

We will consider all comments below.

*Specific comments*

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

This comment refers to our citation of Gebler et al (2015). We refer to p. 2151, 3 lines above Eq. 10 in Gebler. Their considerations are rather complex and we prefer not to explain them in detail in our article.

*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.*

We incorporate period of day, time interval and vegetation into Table 1 – or we add one table (1b) below Table 1 listing the requested information for all stations.

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

It is not the scope of this manuscript to investigate these influences and we refer to authors involved in the measurements at the site. However, since the oak trees are rather widely dispersed, we assume a mild influence on turbulence that would be significant to differences between lysimeter and EC measurements.

*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?*

We are in contact with the PIs of the stations used in this study and trust that all instruments are maintained with the care needed. This includes keeping the lysimeter vegetation comparable to the surrounding area. Investigating differences between LY and EC originating from such conditions is not the scope of this article. We are citing articles dealing with this problem (Evett et al. 2012) and other sources of uncertainty of lysimeters.

*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.*

We are aware of the extensive correction procedure that is required to obtain accurate flux measurements when using the eddy-covariance method. However, we are in contact with the data providers of every single station presented and trust their capacity of correctly processing theses eddy-covariance data. Describing the flux corrections in detail would take up an unnecessarily large part of the article.

*10) L113: Wohlfahrt 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.*

Our screening according to the Out-of-Bound-concept avoids data combinations which correspond to case 2 and case 3 in Fig. 1 of Wohlfahrt and Widmoser (2013).

We add at L113: According to this concept, the ratio $r_1 = (r_a + r_c)/_r a$, where $r_a$ and $r_c$ denote aerodynamic and canopy resistance, must numerically be within the range of 1 to infinity (see Fig. 1 in Wohlfahrt and Widmoser, 2013). Case 2 represents $r_1 < 0$ and case 3 represents $0 < r_1 < 1$. Data corresponding to case 2 and 3, are thus omitted.

*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.*

We add figures of the diurnal cycles for observed $A, H, LE, LY$, and epsilon for all four stations in section 2 (see figures at the end).

*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.*

Certainly, one can achieve good correlations between $LY$ and $EC$. But omitting epsilon-values would exclude any information about the contribution of $LE$ ($EC$-measurements) to the energy closure. This is, however, the main objective of our paper.

*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.*

We add to L173 (Tables 2): They highlight the substantial difference between the humid and dry stations in terms of the mean magnitude of evapotranspiration. Under moist conditions, in contrast, the dry station Majadas (M4) ranges around the same magnitude as the humid stations.We re-formulate L199 (Tables 3): Tables 3a and 3b show the absolute differences and their standard deviation between the $EC$-data presented in Tables 2a and 2b and $LY$-measurements. They indicate how the differences between $EC$- and $LY$-measurements mostly (except for F1) get smaller from observed ($DLoL$) to adjusted values of $LY$ ($DLaL$).

For Tables 4, L228 is moved to L219 (beginning of the corresponding section).

In general, the findings from the tables are presented in the discussion.

*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.*

The change from the original data ($oLE$) to corrected ($cLE$) and adjusted ($aLE$) data involves the interactions of $oLE, LY, epsilon, d$ and the regression of binned data. Especially the latter smoothes down the morning data.

*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.*

In line 266 we re-formulated the first sentence to: Figures 5a and 5b show the mean course of $wL$ during daytime-hours using the average of all $wL$ values at a specific hour.

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

This is the numbers of bins. The bins were selected such that the number of data within all bins remained more or less the same. Depending on the sample size we had 90 to ca. 120 observations within a bin. This is described in section 2.5 (see pp. 148-150). For clarity, we re-formulated L267 to: The number of bins used in Fig. 5a per station varies from 6 (F1), 8 (F2, G2, RHB) to 14 (G1). The number of bins used for Majadas in Fig. 5b varies from 5 to 12, depending on the used period.

*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.*

We are grateful for this hint. However, it's rather during low turbulence conditions (evening through morning), that the signal to noise ratio of the $EC$ system is small and $EC$ measurements inaccurate, since the fluxes are very small. Eddy correlation in low turbulence conditions can then result in flux overestimation. We rather believe that the variability is lower in the morning/evening due to the mentioned smallness of the absolute values during stable/less convective conditions. We add in L275: ..., which relates to the fact that the absolute differences between $LY$ and $EC$ observations are comparably small during stable to weakly unstable conditions in the morning and evening. Additionally, in the discussion L343 is re-formulated accordingly, basically dropping the surprising character and deleting the vague statement about weather conditions (see also comments 22 and 23).

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

$s_11$ stands for data smoothed with a moving median filter with a window length of 11 timesteps. We add this information in the legend: A moving median filter ($s11$) with a window length of 11 hours was used for smoothing.

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

We add a comment on this at L347: The correlation of $wL$ to the magnitude of evaporation is also indicated in Fig. 6b, where a drop in $wL$ follows $cLE$. This correlation is indicated in Fig. 6b, where a drop in $wL$ follows $cLE$.

*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?*

The authors rather use the regression of $wL$ and $LE$ and then derive $cLE$. The tables reveal, that wL and the magnitude of evaporation are positively correlated. But indeed, these tables present auto-correlated values, and their contribution to the study is small.

We thus drop the whole section 3.9 and mention the correlation $wL$ to magnitude evaporation in the discussion as the statement from comment 19).

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

We add the following statement on L319: The difference between $SD(LE)$ and $SD(oLE)$ is getting bigger since $SD(oLE)$ gets smaller after correction, whereas $SD(LE)$ remains the same.

*22) L345: See my comment #17*

See our comment under 17.

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

We will drop this statement in view of our new statement on $SD$ under comment 17.

*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?*

This is true, but without including epsilon one cannot get the contribution of $EC$ to the energy gap, which is a strong part of the current study. And as the energy gap problem is characteristic to involving $EC$ measurements, using lysimeters for evaporation instead of EC would obviously solve a part of the problem. It's a good point and we thus add in L364: If a high-precision lysimeter capable of resolving evapotranspiration as well as condensation is available complementary to an $EC$ set-up, $LE$ can directly be obtained from the lysimeter.

*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.*

The actual used time interval of $EC$ measurements would not change, only the averaging window on the rawdata could be shifted, as mentioned in L360, so that an $EC$ average would better correspond to a e.g. 5 min lysimeter value. The sentence is unclear and is re-forumalted to: In a first step we recommend to perform the comparison of $LY$ and $EC$ based on 5 to 10 minutes lysimeter intervals, and center the one/half-hourly averaging window on $EC$ raw data accordingly. The problem we see with scintillometry we see in regard of reducing the energy gap is the even larger discrepancy of footprints between $EC$ and scintillometer.

*Textual comments:*

*26) L14: "At the overall average" –> "Overall"*

Good suggestion, we use only 'overall'.

*27) L15: "which were partially closed with" –> "after applying "*

Good suggestion, as the term closed in regard of the measurements is misleading. Chnaged accordingly.

*28) L16: "remain high differences" –> "remain large differences"*

Will be changed.

*29) L18: "correction evaporation weights". This looks like a defining term, but is never used in the main text, please be concise on terminology.*

True, we drop the 'correction'.

*30) L19: "correcting evaporation weights". Yet another formulation never used in the main text.*

True, we drop the 'correcting'.

*31) L29: How is the energy balance gap defined? I would expect a value of 22-27 % for the magnitude of the gap.*

Good catch. This is true and we are aware of this. The numbers used refer to the closure, the gap thus is 22.0 to 27, as the R2 correctly suggests.

*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.*

This has also been critized by R1 and has been changed accordingly (see also response to R1): 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. . ..

*33) L31: How do this percentages relate to the values of 73.2 and 78% on line L29.*

This is now answered with the re-formulation in comment 32).

*34) L35: "with" –> "of"*

Changed.

*35) L36: "an influence of the increasing plant height as against constant measurement height is suspected." Unprecise wording, please correct.*

We re-formulate to: They reported substantially larger $LY$ evapotranspiration rates compared to the EC measurements due to differences in plant growth in the $LY$ and the $EC$ footprint.

*36) L38: is -17% to -19% on a daily basis? Please be precise in formulation*

In L34 we add that Evett used daytime values. Additionally, we re-formulate the sentence to: ...mean differences from -17 to -19 % were found between the two measurements methods after...

*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.*

Good suggestion: We move the first paragraph (In addition to...) to the end of the section, and start the section with the second paragraph (Data were obtained from the following Institutions), foolowed by: Table 1 gives an overview on the location of the sites and time periods used.

*38) Table 2b: Some numbers are out of place in the last column, it seems.*

Those are line numbers being somehow jumbled into the table.

*39) L281: 212 weeks?*

Yes. Is changed to days.

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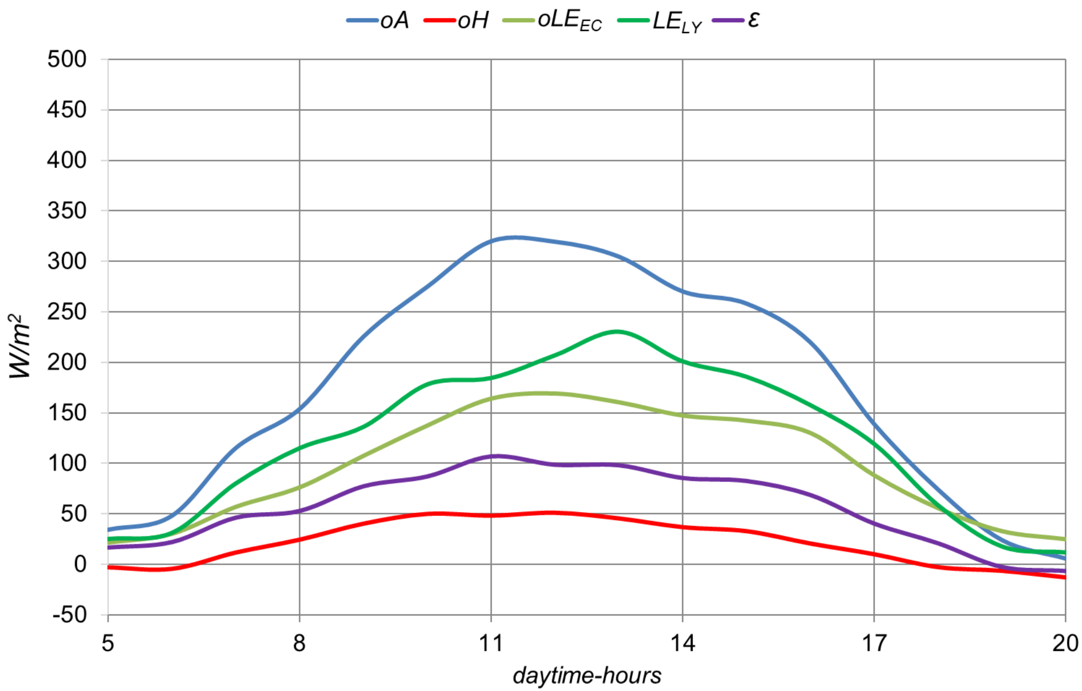


**Fig. 1.** Average daytime course of available energy oA, sensible heat flux oH, EC-based (oLEEC) and lysimeter-based (LELY) latent heat flux and the energy gap epsilon at Fendt.



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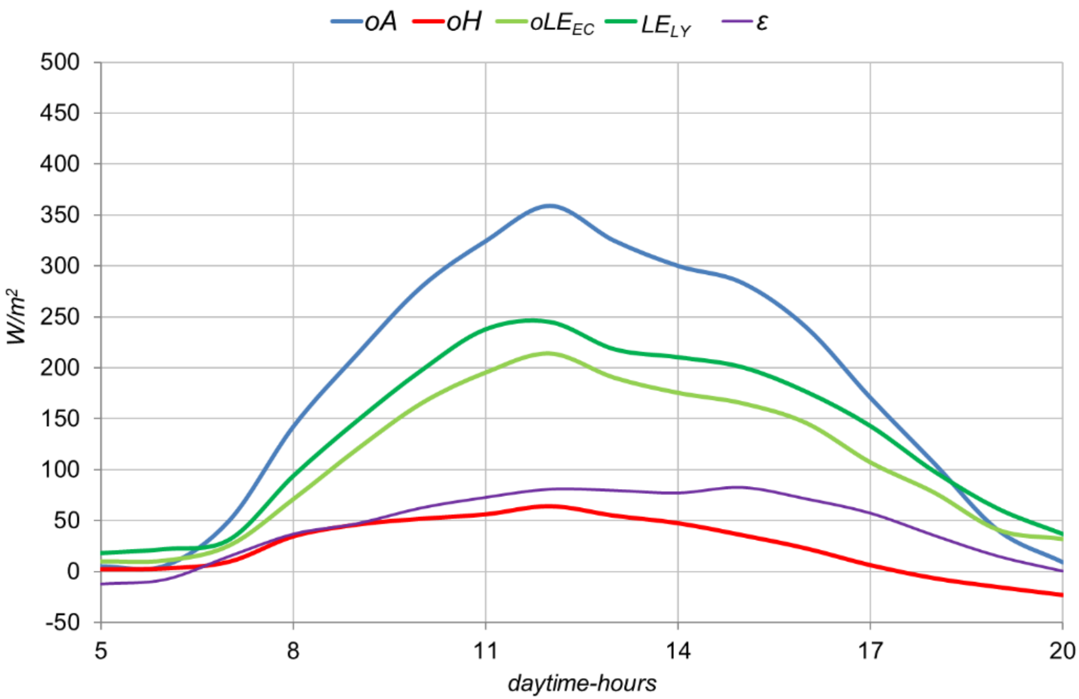


**Fig. 2.** Average daytime course of available energy oA, sensible heat flux oH, EC-based (oLEEC) and lysimeter-based (LELY) latent heat flux and the energy gap epsilon at Graswang.



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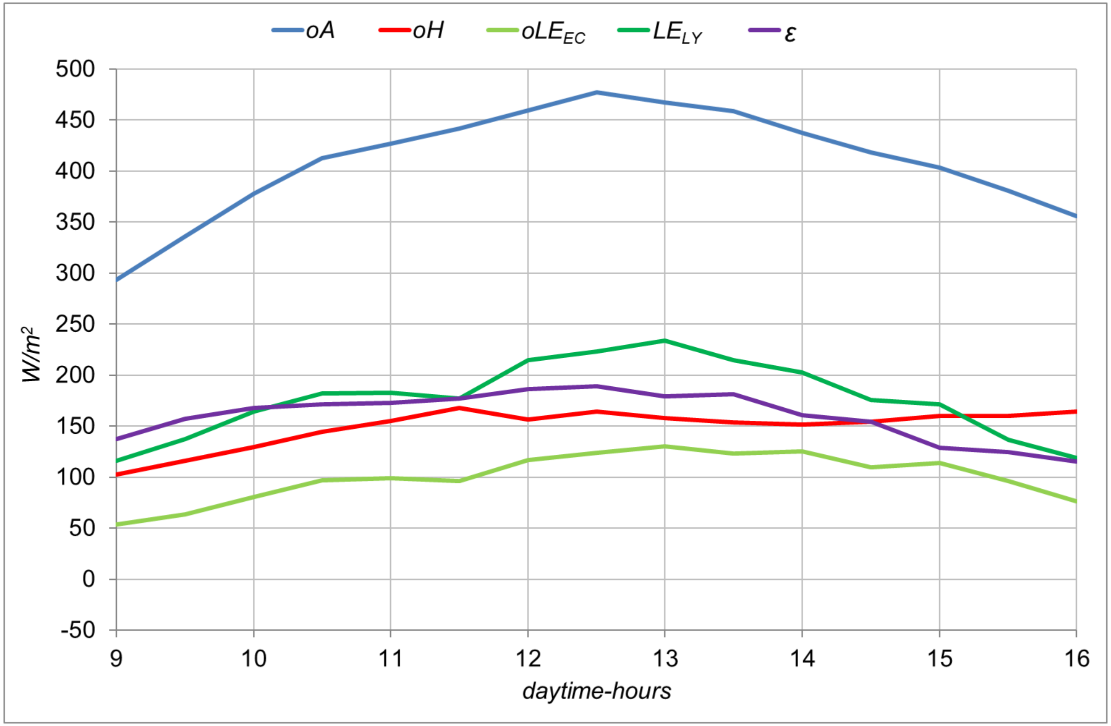


**Fig. 3.** Average daytime course of available energy oA, sensible heat flux oH, EC-based (oLEEC) and lysimeter-based (LELY) latent heat flux and the energy gap epsilon at Majadas.



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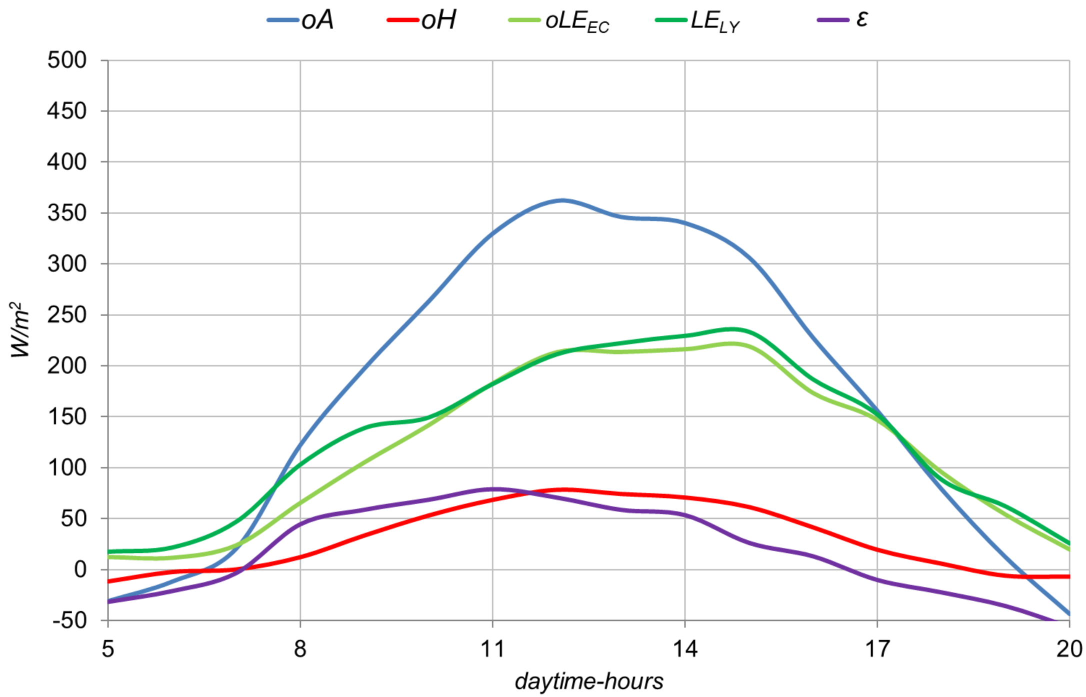


**Fig. 4.** Average daytime course of available energy oA, sensible heat flux oH, EC-based (oLEEC) and lysimeter-based (LELY) latent heat flux and the energy gap epsilon at Rietholzbach.



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