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Interpretability of negative latent heat fluxes from Eddy Covariance measurements during dry conditions	
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Response to Report from Reviewer #2

Reviewer comments are printed in black.
Answers are printed in blue below the respective comment.

My congratulations and thanks to the authors for putting together a clear and compelling case for detecting soil water vapor adsorption (SVA) using eddy covariance measurements. I found the paper easy to follow with interesting results and conclusions that were well supported.

Thank you very much for this kind assessment of our work

I have only some paper specific edits to suggest and comments that need clarification (minor revisions to address before publication).

L16-17. Wouldn't this claim about the prevalence of SVA be better supported by using the lysimeter results themselves? If so, change the text and move this above the results on how well EC does at detecting these events.

Thank you for your suggestion on the abstract structure. Since our study focuses on the interpretability of the negative latent heat flux (through the comparison to lysimeters), and to a lesser extent to the SVA measurement of the lysimeters, we decided that keeping the original structure of the abstract better reflects the manuscript's content.

L 20. Here and in the main body I found this result on using RF to explain mismatches between the two techniques to be confusing. In the text, I thought you were using RF mainly to explain mismatches between lysimeters themselves rather than differences between Fin estimates between EC and lysimeter?

Thank you for pointing out the ambiguity in the explanation of the conclusions of our model.

The model was designed to predict the mismatch between the difference between the lysimeter and the Eddy Covariance observations - which is why in Line 20 (abstract) we wrote "Based on a random Forest feature selection we found the mismatch between the EC vs lysimeter results".

We, therefore, addressed this comment in the respective results section 4.4 by revising the respective paragraph and clarifying the sentence, which was potentially misleading (line 468):

"We investigated the potential reasons for the mismatch between the two measurement methods by means of a predictor variable selection procedure followed by a random forest model analysis with the deviation between EC and lysimeter as the dependent variable (Jung & Zscheischler, 2013). "

However, we think that generally the use of the RF is clearly explained since we state right at the beginning of the relevant paragraph (line 476): "*The primary factor influencing the variation between instruments is SWC within the lysimeters. The deviation between instruments decreases at lower SWC (Fig 7c) and higher Ts (Fig 7d)."*

However, it is of course very important to us that readers can understand the result of the model. In case that also after a second read this section is still unclear to the reviewer, we would revise the text again.

L30. Is there a citation for this being an underrepresented component of the research?

Yes, you are right that having a citation here is appropriate. We added the respective citation:

Saaltink, M. W., Kohfahl, C., & Molano-Leno, L. (2020). Analysis of water vapor adsorption in soils by means of a lysimeter and numerical modeling. *Vadose Zone Journal*, *19*(1), e20012. https://doi.org/10.1002/vzj2.20012

L91. "distance between the sampling height and the ground"

We received a very similar suggestion from reviewer 1 and decided to add his/her suggestion: "the vertical distance between the EC sensors and the adsorbing soil surface"

L100. Suggest using "forces" instead of force fields.

We adapted the wording.

L108. "escape the liquid phase"

We adapted the wording.

Figure 1. Great summary figure!

Thank you very much, we are pleased to see that the figure is well received.

L71. "…integral turbulence characteristics were removed" - This wasn't mentioned for the other site. Also, is this based on measurements of turbulent mixing strength like u*?

Thank you for pointing out the differences in the depth of the description of the Eddy Covariance data processing. The filtering for time periods based on the test on integral turbulence characteristics was performed at both sites, following the method described in

Foken and Wichura (1996). We added the respective information to the processing description in ES-LMa:

"Standard integral turbulence characteristics were identified and most problematic records removed (Foken and Wichura, 1996)."

Apart from this detail, we decided to add the information that the two softwares used (EddyPro in ES-LMa and TK3 in DE-RuS) show close agreement (Fratini and Mauder 2014) to make more clear that there is no substantial difference in the processing of the EC flux data between the two sites:

"The two softwares used to process the raw data at the two sites (EddyPro and TK3) have been shown to be in good agreement (Fratini and Mauder, 2014)."

Foken, Th. and Wichura, B.: Tools for quality assessment of surface-based flux measurements, Agric. Forest Meteorol., 78, 83-105, https://doi.org/10.1016/0168-1923(95)02248-1

Fratini, G. and Mauder, M.: Towards a consistent eddy-covariance processing: an intercomparison of EddyPro and TK3, Atmos. Meas. Tech., 7, 2273–2281, https://doi.org/10.5194/amt-7-2273-2014, 2014.

L277. U* = 0.01 m/s is extremely low. Is this a typo, maybe 0.1?

This is not a typo, but an important point that the reviewer raises here. After discussion with our co-authors, we decided to re-run our entire analysis based on a standard EC post-processing u* - threshold that is site-specific and seasonally dynamic.

We changed the (former) line 277 accordingly to:

"However, to be conservative we determined periods with low turbulent mixing based on u thresholds for each season of a site year. The effect of the u* on the agreements between the two measurement methods was evaluated by removing measurements below the established threshold. To take into account the uncertainty of the u* threshold estimate, this was repeated for the 5th and 95th percentile of the u* threshold estimate (Papale et al. 2006; Wutzler et al. 2018, thresholds given in table F1)."*

The u* thresholds (5th, 50th, and 95th percentile) vary between 0.050 and 0.103 m s^{-1} in ES-LMa* and 0.054 and 0.177 m s^{-1} in DE-RuS. The change in the u* threshold affected the results reported in Section 4.2 and 4.3 including the respective tables and figures (Table 2, Table 3, Fig. 5, Fig. 6). In summary, the threshold confirmed that the flux direction between the two measurement methods is nearly constant independent of the selected u* threshold. The comparison of the mean nighttime flux magnitude revealed that there is a 25 % reduction in the error (MAE) during conditions of good turbulence, compared to including the low turbulence conditions (in the preprint). The correlation between instruments increased from 0.66 to 0.79 in ES-LMa*. However, with this threshold an even lower number of observations in DE-RuS remains (only 4 nights when both methods measure F_{IN} under turbulent conditions). Therefore, we refrain from interpreting the statistics at the temperate

site but keep the (updated) results in Appendix H for completeness. The model analysis, was kept as it was in the preprint with an u* threshold of only 0.01 m s^{-1}: This way we test the dependence of the half-hourly mismatch between the two measurement methods of the u* magnitude in a data-driven way. We added a sentence about the possible reason of the u* being less important than the rest of the variables:

"It is therefore possible that soil heterogeneity conceals the effect of variables associated with EC uncertainty on the mismatch, which should be checked in a more homogeneous ecosystem. This is supported by the detectable effect of the u that shows that the discrepancy between instruments decreases with higher u* (see Figure J1), but its effect on the mismatch is one order of magnitude smaller than the effect of lysimeter SWC and Ts."*

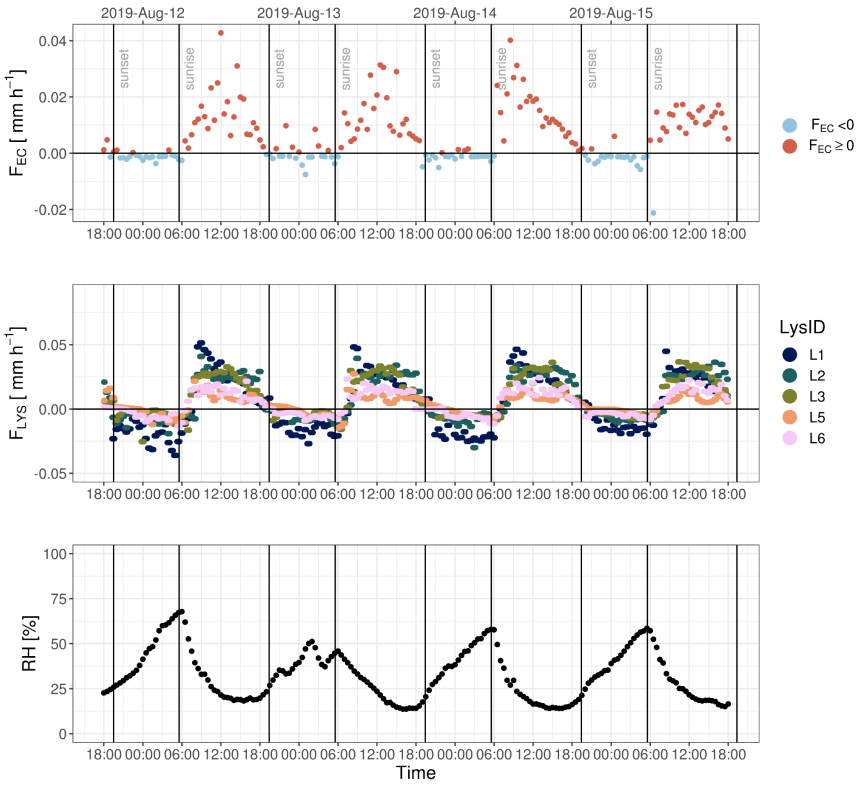
In addition to the changes in the results section and the respective figures and tables, the following new tables and figures were added to the appendix section:

- Table F1: reporting the different u* thresholds at both sites,
- Table F2: showing the effect of different u* thresholds on the flux direction agreement
- Table F3: reporting the statistics for the comparison between $F_{IN,EC}$ and $F_{IN,\ LYS}$ across the range of u* thresholds (5%; 95%)
- Figure J1: influence of single observations on the mismatch between EC and lysimeters (SHAP marginal plot; similar to the ones shown in Figure 8 (c) and (d))

Figure 3. This is very compelling evidence of EC detection of negative fluxes indicating SVA. Would it also be possible to select a few multiday periods with substantial Ein and show mean diurnal plots of E for both the lysimeters and the flux tower? Just 2 or 3 of these would show whether there is strong evidence of SVA during dry periods and its magnitude relative to the daytime values. I know this is quantified later in the manuscript but this would be a way to show it visually.

Thank you for your suggestion. We agree with this idea.

We have decided to illustrate the diel measurements using four days of data in August 2019. Although the $F_{IN,EC}$ cannot really be called substantial, we think it illustrates well that almost all F_{EC} values were negative during the nights, all lysimeters record weight increases ($F_{IN,LYS}$) and the RH never rises above 75%, showing that there is no formation of dew or fog. We added the figure as an Appendix Figure.



Diurnal measurements of water vapor flux with (a) eddy-covariance (F_{EC}) and (b) the five lysimeters (F_{LYS}); the color code shows the respective station L1, L2, L3, L5, and L6) at ES-LMa from 11.08.2019 18:00 h until 15.08.2019 18:00. Panel (c) illustrates the course of relative humidity (RH) at 2 m height above the soil surface. Black vertical lines illustrate sunset and sunrise (determined by the geographic coordinates of the field site).*

L360. I don't understand "the process is independent of climate". It seems highly dependent on climate conditions. Please clarify.

The reviewer is right that the frequency of the SVA occurrence depends on climate conditions, as it clearly depends on meteorological conditions.

In paragraph proceeding l.360, however, we showed that SVA occurs also in the temperate climate during conditions of climate extremes. Therefore, here we wanted to point out that the process itself can happen in different climatic areas (as long as soil water content is low and RH high) because it is a soil intrinsic physical property (while climate is defined only by the mean conditions and not constant over time). We changed the sentence accordingly and hope that this point becomes more clear now from the text:

"It underscores that while the probability of occurrence of SVA is influenced by climate (i.e. more common in semi-arid and arid regions), it can also occur in more humid regions. This is because it depends on soil-intrinsic physical properties, such as texture (clay content, clay mineralogy, and organic carbon content) (Orchiston, 1956; Arthur et al. 2019, Yukselen Akoy, 2010), soil structure that affects vapor transport characteristics (i.e. soil diffusion coefficient), and can happen anywhere if the dynamic requirements like temperature and moisture gradients are met."

Table 2. Could you please add the number of periods/days that the lysimeters recorded these events?

This number is given in the first line of the table (n nights) - since as described in the manuscript, the process occurs nearly exclusively at night in our ecosystem.

L434. Why better? Couldn't it be that the lysimeter spatial mean is biased high compared to the broader EC scale?

In the respective paragraph, we don't state that the lysimeter measurements are more accurate but state that the statistical metrics for the comparison between the median across lysimeter columns and the Eddy covariance instrument are better than between EC and individual columns. We hypothesize in sentence L.434, that this might be because the median across lysimeters better represents the spatial mean (since each lysimeter only covers a small spatial scale of 1 m^2).

We hope that we could clarify this in the revised version of the text and by adding the footprint climatology following the suggestion of reviewer 1. It now states:

"However, we find higher agreement between EC and the median across the lysimeters (Table 3) than between EC and individual lysimeters (Table H1). One interpretation of this result could be that each lysimeter covers a smaller spatial scale (1 m^2 each) compared to the EC (illustrated in Figure 2 as footprint climatology) but the average across lysimeters is a better representation of the spatial mean and is therefore more in line with the EC observations."

L484-486. I had trouble understanding this conclusion. I thought the sentences above were talking about what explains the differences between lysimeters and not between EC and lysimeters. Can you please explain and maybe rewrite this sentence?

This comment was already addressed in our answer to the comment related to line 20, (which refers to the summary of our model results in the abstract).

Figure 9 . Both axes in (c) are labeled as IN.

Thank you very much for spotting this. We changed the respective label.

L519-523. I think this text applies to bare soil evaporation. Evaporation from mixed plant/soil conditions as you have for the lysimeters and EC fluxes occurs differently than this in that stage 2 isn't just about diffusion-limited processes through the soil as plant transpiration is also a substantial element for this period (or at least for all but the driest conditions).

The reviewer makes a valid point for ecosystems with active vegetation. In our ecosystem, the grasses and hence - the vegetation in the EC footprint and on the lysimeters, have withered and don't transpire since they are dead during the focus period. This condition is described in the section site description (3.1) and again in the first paragraph of the results section (4.1). The soil is not bare since there is still plant residuals left, but transpiration is not part of the soil-atmosphere vapor exchange in this period (until it rains, which is also when SVA ceases due to rising SWC).

Therefore, we argue that the conditions that the discussion is based on are met but we've added the valid constraint that the reviewer pointed out ("in the absence of transpiration") to sentence line 518.