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Lysimeter based evaporation and condensation dynamics in a Mediterranean ecosystem	
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Author's response to Referee #3 Werner Eugster

(Referee comments in black, *author's response in blue italic*)

General comments

Non-rainfall water inputs (NRWI) can be an important hydrological water source to plants in arid and semi-arid ecosystems, but also elsewhere during dry spells and drought periods. The authors argue that so far most measurements were done with microlysimeters that may overestimate NRWI if their construction is of a simple type that does not attempt to bring microlysimeter soil temperatures in good agreement with the surrounding soil. Contrastingly, a normal size lysimeter---of which the authors have 6 on site, where one was excluded from the analysis---have the advantage that a temperature control of the soil slab is possible and hence less of the problems reported for microlysimeters should result from using standard lysimeters with highly resolving weight measurement.

The authors present a full year of data from a Mediterranean site in Spain, but the focus of the manuscript is more on the method, the real measurements are more used in a proof-of-concept mode without independent and reliable (and established) validation data as obtained from blotting paper water collection and analysis. Thus, the manuscript could actually be classified as a "technical note". My suggestion is to suggest moderate revisions before accepting the paper. There are a few scientific errors that can be easily rectified in a thorough revision round (wrong physical units, mostly) and with some information the context and wording can be quite misleading and should be corrected. Moreover, the main shortcoming of the manuscript is the

(1) the absence of a (at least simple) visibility sensor to be able to scientifically correctly separate fog from dew conditions, and

(2) lack of robust and independent validation data for supporting the claim that the presented method---which seems to be a further development of Zhang et al.'s (2019) method---is more accurate than other approaches.

Nevertheless, I recommend proceeding with the manuscript, there is indeed a need for better and more accurate quantification of NRWI in all ecosystems where rainfall can be absent for prolonged periods.

We thank the reviewer for the constructive evaluation of our work and the detailed feedback. We do our best to improve our manuscript accordingly.

We would like to respond directly to the identified main shortcomings in the General comments. All other points will be addressed individually within the next section.

The reviewer identifies that the written manuscript has a strong focus on the methodological approach and suggests that the manuscript could also be classified as a ‘technical note’. In our opinion, a technical note would require a complete validation including campaign-based measurements which we were not able to perform. This was criticized by reviewers 1 and 2, Giora Kidron and Werner Eugster, and we completely agree with both on this point. Unfortunately, however, we couldn't conduct such a campaign during the central measurement period, also due to restrictions related to the pandemic. Therefore, we alternatively tried to evaluate the lysimeter rainfall magnitude and coherence across stations and evapotranspiration measurement totals with independent observations and to benchmark NRWI as well as possible with models.

In the manuscript, we are nevertheless primarily interested in presenting the diel and seasonal dynamics of NRW at our site. Therefore, we prefer to keep the manuscript as a research article as it goes beyond the technical aspect. We leave it to the editor to decide whether he would classify this manuscript as a technical note.

Concerning (1) *based on the concerns of the reviewer we extended the analysis and tried to scientifically benchmark fog occurrence based on radiation equilibrium measured at 1.6 m sensor height. Suspended droplets during fog cause an optically thick layer that obstructs surface radiation to the sky and creates a radiation equilibrium. Under such conditions, visibility sensors would also record a decrease in visibility which has been used in other studies additionally to rH sensors to separate fog from dew (e.g. Feigenwinter et al. 2020, Riedl et al. 2022).*

Methods: *(1) We visually identified mornings of fog and mornings where no fog occurred from images collected by a digital camera installed at the site from 01.10.2020 to 31.12.2020. (2) We hypothesize that fog creates a radiation equilibrium at our sensor at 1.6 m measurement height. We tested this hypothesis by comparing the ratio LW↑ to LW↓ between foggy and non-foggy conditions with the non-parametric Mann-Whitney U test.*

(2) Then, we compare the ratio of LW↑ and LW↓ during fluxes classified as fog based on rH and the lysimeter weight changes to periods where dew or none of the two fluxes are assigned. Statistical significance between the conditions was again tested with the non-parametric Mann-Whitney U test.

A direct comparison between the camera images and the lysimeter measured fog deposition was not possible because fog could only be visually identified after sunrise. The deposition recorded with the lysimeters, however, stopped with sunrise.

Results1: *During fog conditions that were visually identified from the digital camera a radiation equilibrium is nearly reached with a median ratio of 0.98, whereas during non-foggy conditions the median ratio is lower (0.88). The difference is statistically significant ($p < 0.001$).*

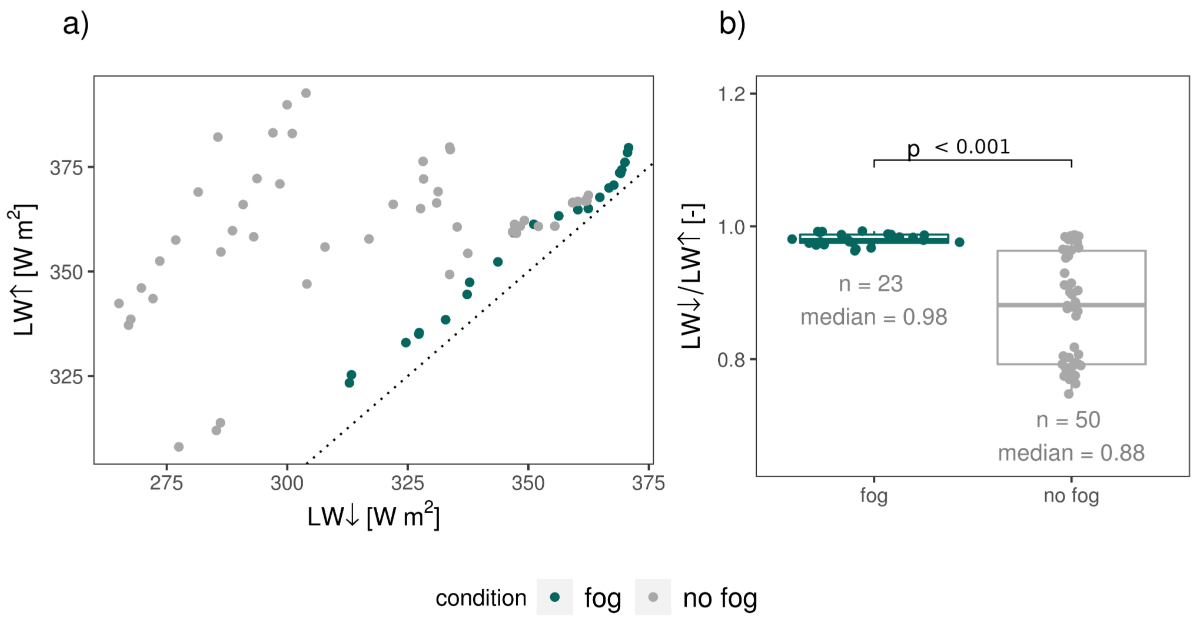

Figure 2. Comparison of upward (↑) and downward (↓) longwave radiation (LW) during the conditions classified as fog and without fog from a digital camera. The black dotted line in panel a) displays the identity line. Panel b) illustrates the radiation ratios during fog and no-fog conditions. The differences between the ratios are statistically significant with a significance level $p < 0.001$.

Results2: *Lysimeter weight increases assigned to fog based on a rH threshold of 97 % are also closer to radiation equilibrium (median = 0.9) compared to dew (median = 0.84). The distribution of the ratios has a greater overlap at lower ratios. The difference between the two categories is statistically significant ($p < 0.001$).*

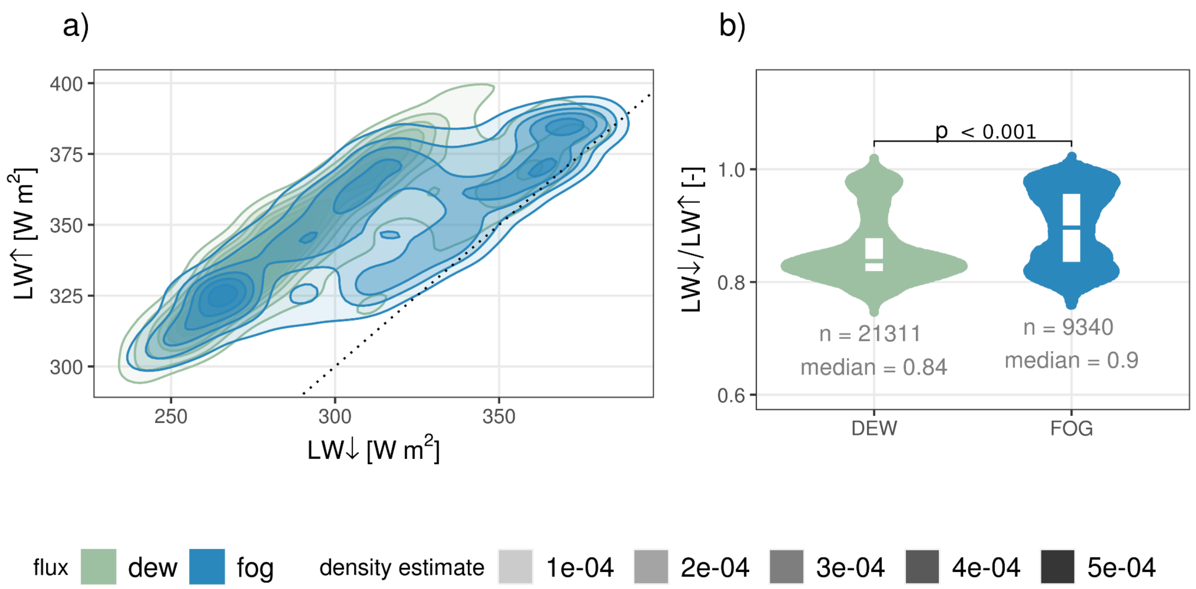

Figure 3. Comparison of upward (↑) and downward (↓) longwave radiation (LW) during the conditions classified as fog and dew from the flux partitioning of lysimeter weight changes. Panel a) illustrates

the smoothed Kernel-Density-Estimate, with the black dotted line displaying the identity line. Panel b) illustrates the distribution of radiation ratios during dew and fog conditions. The difference between the distributions is statistically significant with a significance level $p < 0.001$.

In the manuscript, we changed the Material and Method section by reformulating 2.3 and adding a new subsubsection (2.3.1), explaining the approach. The respective results were added in section 3.2 as (new) Figure 5 (only for rH = 97 %). In the Discussion, we extend the paragraph (l 361 - 366) to put greater emphasis on the threshold parameter selection uncertainty and the link to the (partially) incorrect assumption of only one flux occurring at a time.

We hope that this approach convinces the reviewer that even without a visibility sensor a separation between fog and dew can be performed to a sufficient degree based on an rH threshold.

***Concerning (2)** we changed all passages in the manuscript claiming that the presented method is more accurate than the method of Zhang et al. 2019. As stated above, this was not the scope of the manuscript and cannot be performed without on-site measurements.*

MAJOR POINTS

Title: the paper is clearly focused on NRWI, and evaporation in my view is rather treated a side aspect (and not very clearly associated with NRWI except for the short discussion about downward latent heat flux being in good agreement with water vapor absorption estimates from the lysimeter). In my view a title of the kind of ``Lysimeter based quantification of non-rainfall water inputs to a Mediterranean ecosystem'' (maybe clearly classified as a technical note) would represent the contents much better.

Thanks for sharing your thoughts on the title and for your concrete suggestion. Based on your concerns and our intention to formulate the core message of the paper as its title we would come up with the following suggestion as an alternative:

“Resolving seasonal and diel dynamics of non-rainfall water inputs in a Mediterranean ecosystem using lysimeters”

62: ``Recently Kidron and Kronenfeld (2020b) found that temperature inside the micro-lysimeters deviated from that in the surrounding soil, \ldots''---here some rewriting is required. Firstly, the original paper makes unacceptable generalisations that require some caution when using that reference; secondly, the statement as presented here does not correctly reflect the contents. Kidron and Kronenfeld (2020b) used microlysimeters (ML) with a huge gap between ML and original soil, a cold-air trap that leads to excessive cooling of the pit at night and consequently to lower than normal soil temperatures. And that's the key effect, \textbf{not} that the soil temperature is different from the surrounding soil: if the soil inside the ML is colder than it should be, then you expect additional condensation to occur in the ML, thereby artificially suggesting and NRWI that is in fact an artifact. As you can see in Riedl et al. in Fig. 5 the soil inside our MLs is actually somewhat warmer or equal in temperature compared to the control. In this way, the artifact of the ML critisised by Kidron and Kronenfeld (2020b) is avoided. Thus, the problem with Kidron and Kronenfeld (2020b) is that they generalize from their overly simplistic ML to all ML (which is not correct, a simple lid actually solves the problem, or at least reduces this artifact). A normal-size lysimeter with such a large gap around the lysimeter would also act as a cold air pit, the only advantage you have with a large lysimeter is that you could more easily add heating wires to heat the soil to more closely match the control temperature. Thus, I agree with your argumentation in lines 64--66.

Thank you, we understand that this sentence was misleading in the way that it did not clearly point out that the problem lay in the ML design that many past studies were based on and not the ML technology itself. As suggested we reformulated the sentence to make this point more clear:

“It was recently suggested that the temperature regime in many formerly used micro-lysimeter setups deviated from the surrounding soil causing an overestimation of the measured NRW (Kidron and Kronenfeld, 2020b).”

169: The 10\,cm T_a is not a real reference, see Monteith (1957). Why not extrapolate to the 1\,cm height? At least you must reference and consider Monteith (1957), this is an omission which is not understandable. In my view all papers with Monteith as author or co-author are of a quality that makes them relevant even after decades, and omitting Monteith knowledge normally goes in the wrong direction (scientifically), away from what we call ``progress''.

Thank you for this comment. We included Monteith 1957 as a reference but would argue that the 0.1 m height of air temperature measurement is appropriately chosen based on the conditions at our site. The grass layer in Monteith 1957 was kept about 1 cm high by regular cutting, which is why this height is suitable for their setup. The average canopy height of grasses in Majadas is however 0.1 m (Migliavacca et al. 2017). Therefore, we prefer to keep the offset value for the temperature at 0.1 m height.

This choice has been indirectly confirmed by Giora Kidron (reviewer 2), who supported our choice for the grass layer, pointing out that may not be appropriate for biocrusts (which we barely have at our site).

Nevertheless, based on your comment we analyzed the data again and found that the average air temperature difference from 0.1 m measurement height and spline extrapolated temperature at 0.01 m height amounts to only 0.03 °C.

313--315: see my remarks further up (line 62). This statement needs a rewording to take care of the flaw of Kidron \& Kronfeld's questionable generalisation to all ML, and the fact that this overestimation could be overcome easily by using a smarter ML design with a lid to avoid the nocturnal cold-air pit.

We changed the sentence in lines 313-315 in the following way:

“Many former studies based on simple micro lysimeters likely overestimated NRWI due to greater heat loss through the walls compared to the surrounding unperturbed soil (Kidron and Kronenfeld, 2020b).”

Appendix A1: this is never referenced in the text and is a mess---either remove or rectify all the errors. If the latter is desired: C_p has wrong units; s see my comments; LW and SW have wrong units; ΔW has wrong units; use Δq instead of δq;

ε has wrong units (should be dimensionless or simply (---)); e_a has wrong units; u_* should have the asterisk in subscript

Thank you very much for making us aware of these errors, we corrected them and added a sentence in section 2.1.2 referencing Appendix A1. We also carefully checked the rest of the manuscript to avoid other errors of this type.

Appendix A2: Eq. (A1) would be simpler to read as \begin{displaymath}T_s = \sqrt[4]{\frac{1}{\sigma \cdot \varepsilon} \cdot \left[LW_\uparrow - (1-\varepsilon) LW_\downarrow \right]} \end{displaymath}

Thank you for making this suggestion, we made the changes to equation A1 as suggested.

Appendix A2: the mathematical convention is to use either $\cdot$ (\verb+\cdot+) or space for multiplications of scalars, and only use $\times$ for vector products; please update equations accordingly. Use ``upwelling'' and ``downwelling'' for radiation fluxes; there is always the potential confusion that a down-looking sensor actually measures upwelling radiation, etc. Replace the erroneous NA with (---) or (dimensionless)

Thank you for making this suggestion, we made the changes to equation A1 and in the methods section as suggested.

Figure 6 and the text is actually a result of the study, not a discussion point. Maybe this is the reason why I think the title and text do not agree---if evaporation as mentioned in the title were the focus of the paper this aspect would have come first in the Results section, not last as an add-on in the Discussion section.

We understand Figure 6 more like an outlook & hypothesis for future work. Changing it into a result of the study would require a more detailed and statistically substantiated analysis which does not fit the focus of the manuscript. Nevertheless, we think this observation is interesting to the community and promotes more research on EC negative latent heat flux measurements. We revised the respective paragraph and tried to make it more clear that future research should systematically test the suitability and limitations of EC to detect adsorption.

We would leave it to the editor to decide whether Fig. 6 should be moved to the results section.

OTHER IMPORTANT POINTS

12: ``eddy covariance-derived latent heat flux estimates'': since the paper focuses on NRWI I found this statement somewhat misleading because it only addresses the ET losses but not the gains that would be associated with NRWI. Moreover, the authors do not even define ET, which indicates that this was not the real focus of the manuscript.

We are not sure that we understand the problem with this sentence since we chose "latent heat flux" in the formulation for the reason that it is not directional, whilst ET as you point out here would only address the losses.

Also for the definition, we would like to ask if there is a problem with the way we defined ET *(“evapotranspiration (ET, mm)”) in line 18 or if this sentence was maybe just overlooked.*

23: horizontal precipitation does not belong to NRWI. It is rainfall (precipitation) which is not measured by standard rain gauges (but e.g. by special rain gauges on vessels).

Thank you for clarifying this point. We deleted horizontal precipitation in the parenthesis.

26: it is not Feigenwinter et al. who defined the classification of fog. Rather use a primary source here, AMS, WMO, or (what I use) the AMS Glossary of Meteorology by Glickman (2000):

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@Book{Glickman2000,
  editor    = {Todd S. Glickman},
  publisher = {American Meteorological Society},
  title     = {Glossary of Meteorology},
  year      = {2000},
  address   = {Boston, MA},
  edition   = {2},
  comment   = {formerly: http://amsglossary.allenpress.com/glossary/},
  url       = {https://glossary.ametsoc.org/wiki/Welcome}
}
```

We completely understand that applying this change makes great sense and exchanged the citation following the suggestions of the reviewer.

30: ``However, differentiating between these two origins is commonly not possible (Li et al., 2021b)''---this is wrong, please read the text: Li just shows the opposite that because the sources of the vapor used in dew formation and the vapor from soil water are of such different origins, using stable isotopes allows to differentiate. You may argue about the word "common", but stable isotopes are common by now (at least the simple-to-measure ones such as ^{18}O and ^{2}H in water and water vapor). MPI Jena is doing this since its establishment. Please reword to convey the correct content and context with this statement.

We agree that the citation of Li at this point in the text was misleading. What we want to express is that in most NRWI studies this differentiation was not performed and Li et al. 2021b were an exception. We changed the sentence in the following way and hope it is more clear now:

“However, most literature summarizes both processes as dew because a distinction requires additional measurements such as stable water isotope (Li et al. 2021b).”

88: ``However, the structure of the cage allowed for grazing to maintain the lysimeters comparable with the rest of the plot.''---this is a challenge and it would be interesting to read some more details how this is successfully done. As is, it is not possible to reproduce this as a reader.

We added a sentence on the height of the fence (50 cm, cage was the wrong wording) and also referred to the pictures added in the appendix based on the suggestion of Giora Kidron (Reviewer 2). We hope that these additions make it more comprehensible for the reader.

For the period analyzed in the manuscript, the strong agreement between EC and lysimeter ET suggests that the grass cover inside the lysimeters was representative of the footprint of the tower in the open space.

During spring 2017, dry matter of the herbaceous vegetation was measured in-situ manually (GrassMaster Pro Drymatter Instrument, Novel ways Limited, New Zealand) on the lysimeter and open space. The mean drymatter over spring in the open area amounted to 704 kg/ha and to 714 kg/ha on the lysimeters. The difference of 10 kg/ha was however smaller than the standard deviation of 348 kg/ha for the open space and not systematically low, which would be the consequence when lysimeters were excluded from grazing. We could include this information of 2017 in the manuscript if the reviewer considers it an important addition.

128--129: Note to Editor: I cannot check the data and code which will be made available once the manuscript is the same. I normally also only publish code and data once a manuscript is accepted, but here the authors do not provide the details in the paper and expect readers to go into the code.

With submission, we sent the access links to the zenodo repository to the editor. The idea was that the data access would be thereby possible for reviewers that prefer remaining anonymous, and to then open up the database before acceptance.

If this didn't work out you can send us an access request on the following page and we grant the access to the data: https://zenodo.org/record/5575521

Apologies for the inconvenience.

134: question: an animal stepping onto the column, is this not bringing ΔW outside of the accepted range and is thus treated there?

If a large animal, such as a cow, is stepping on the column, this should have been removed by the fix threshold value (l 131). In line 134 we refer to lighter animals, such as rabbits or snakes, which should be identified by the comparison between lysimeter weight increase. We added this information in the text to make it more clear to the reader.

"In contrast, if only one lysimeter column shows an anomalous ΔW we considered this as an artifact (e.g., small animals such as snakes or rabbits stepping on the column or issues with the boundary control) that can be removed from the time series (Hannes et al. 2015).

148: ``water input''---reword, NRWI is also a water input and this is \textbf{not} to be included here!

We followed your suggestion and changed "water input" to "weight gain". We hope it is more clear now that we suspect a technical problem in these periods and do not consider these measurements reflecting surface-atmosphere exchange processes.

166: That's why Monteith (1957) uses 1\,cm T_a would be good if you could relate your text more to Monteith's outstanding work which is still our reference.

We hope that we could address this point to the satisfaction of all by our answer in the section "Major points".

251: you show ET with negative sign convention, although you use a positive sign convention for $\lamda E$ in Eq. (2.1). Moreover, you never defined ET nor its sign convention. My recommendation is to use positive values as ET losses from the soil to the atmosphere. I have not seen papers using the reverse sign convention, yours is the first, and this confuses me and maybe also the reader. Recall that the standard hydrological budget equation is still: precipitation = runoff + ET +/- change in soil storage. Moreover, you use a positive ET in Fig. 3b. At least you need to be consistent and declare your symbols and sign conventions (if they should deviate from common sense notation)

We followed the recommendation of the reviewer and used positive values when referring to measured ET in the manuscript.

280: ``evaluation statistics improve by one hour''---I am unable to see this one-hour improvement in Table A2. Is there an error here, either in Table A2 or in the wording?

We checked again and there was no error in Table A2. We rephrased the sentence in the following way to be more specific:

"When comparing only measurements where at least two out of the five lysimeters show weight increases assigned to adsorption, MAE and RMSE decrease by from 4.9 hours to 4.0 hours, and from 5.9 hours to 4.7 hours, respectively."

306: the absolute value of adsorption may be low (as total NRWI may be low), but the relative share in NRWI is quite high in my view. Maybe rather compare the relative numbers to focus on the processes leading to the different components of NRWI

If we understood this comment correctly, in your view the relative share of adsorption to the total NRWI in our ecosystem is quite high. As suggested we added a sentence in the result section to include the information on the relative numbers:

"The largest relative contribution to the mean total NRWI is adsorption with 50 %, followed by dew with 33 % and fog with 15 %"

We also added the following sentence in the Discussion section to focus on the process:

"An important finding of our study is that the relative share of adsorption to annual NRWI is with 50 % much larger than the contribution of dew. Since dew has received greater attention in the past (e.g.Tomaszkiewicz et al., 2015; Beysens, 2018) more long-term studies are necessary to evaluate which NRWI flux is more relevant across years and semi-arid regions."

We also added references to other studies reporting on the relative share of adsorption to ET to underline that in this regard the relative share is reasonable and to underline the potential importance of its contribution to ET:

"Daily adsorption was reported to compensate for 25 % to 50 % of ET in a Spanish olive orchard (Verhoef et al., 2006), and even 93 % of ET in the Negev (Florentin and Agam, 2017). With a maximum compensation of 42 % per week, our findings are comparable to the observations in the Olive Orchard. However, it should be noted that the two cited studies

covered time periods of only several days and therefore the variability of these percentages across the season is not known."

377: ``At our site, this pattern is obvious and indicates that night-time EC measurements could serve to detect adsorption (Fig. 6).''---I partially disagree, but you may convince me with your arguments. In my view adsorption is not directional as dew formation (from vapor above the canopy) or distillation (from vapor below the canopy), and hence should in my view not leave the best trace in EC-based flux measurements. I though (so far) that dew formation should lead to negative $\lamda E$, whereas distillation is not seen by an EC system; and adsorption should only be seen in EC fluxes if its vapor source is above the canopy, but not below. Your Figure 6 of course empirically shows better performance (qualitatively) for vapor adsorption than dew formation, but for me the explanation is not that obvious as your text implies.

Thank you for sharing your thoughts and understanding of the process with us. It is important to differentiate between the processes i) adsorption of redistributed vapor within the soil column, and ii) adsorption of atmospheric vapor. This differentiation is not much different from soil distillation vs. dew. We agree EC should register negative λE when the H_2O recorded from the lysimeters stems from the atmosphere (in the case of dew and adsorption of atmospheric vapor).

Our setup leads to the conclusion that the recorded adsorption mainly stems from the atmosphere and is not redistributed water within the soil column. This was also backed up (data not shown) by measurements of soil water potential from pF meters within the lysimeters at -10 cm depth, from which we can estimate soil pore vapor pressure (with the Kelvin equation). They revealed that even at 10 cm soil depth, pore vapor pressure drops occasionally below the atmospheric vapor pressure measured at 1 m height, particularly at night. Since close to the surface, the soil likely is even dryer, those observations pointed in the direction that the largest fraction of the adsorbed water at our site stems from the atmosphere.

But still - the question remains if EC and lysimeter adsorption measurements are comparable also in other setups and under different conditions with a different tower or vegetation heights. These questions also motivated us for a follow-up analysis where we started working on a set of paired lysimeter and EC observations from several sites, to systematically investigate these patterns across setups and hopefully can give you more profound answers after our next study.

397: ``LE fluxes at dry conditions''---you never defined or used LE, but this appears to be an important statement that should have appeared in Discussion already. Conclusions should not bring up new aspects that were neither addressed in Results nor in Discussion.

Thank you for pointing this out, we changed LE to λE, which was the term that we intended to use, in line with the figure and the reasoning presented in the Discussion. λE was defined in line 120 in the methods section already.

Figure 1: in the text it sounds as if you want to use this as a general workflow also for other sites. But then my recommendation is to avoid site-specific magic numbers in the scheme and provide the site-specific values in the caption to be clear. E.g.: $T_s < (T_\textrm{dew} - T_\textrm{dew, t})$ with the information that $T_\textrm{dew, t}$ = 1.4\,$^\circ$C for your lysimeters and site.

We changed Fig. 1 and the respective sentences in the methods section based on your suggestions.

Figure 1: just of curiosity because we were challenged on this aspect with our ML: why do you not use the high-quality measurements of the drainage outflow of your lysimeter to make sure that drainage loss at the bottom of the soil slab is not erroneously treated as ET? You classify $\Delta W \le 0$ as ET, although it could be drainage loss after intensive rain. This may be an important aspect if you think the method should also be applicable elsewhere.

We removed the tank level changes induced by the lower boundary control system (drainage and capillary rise) already in the raw data filtering, as described in line 130 The reason is, that in the step after - 'outlier identification across columns' - we needed to have these measurement system-induced changes already taken into account and excluded as a potential source of asynchronous behavior between columns.

Figure 1: the percentile approach to separate fog from dew is in my view weak and questionable. Namely dew can only form under conditions that you classify as ``fog'' but fog droplets---if advected---can be present at relative humidities that are not showing saturated air (high $\mathrm{rH}).

It became clear to us that we need to explain better why we chose the percentile approach and what preconditions need to be fulfilled for this approach. We restructured the text and added to 2.2.1 (Material and Methods Section) the following information

"Theoretically, fog occurs at a rH of 100 %. We noticed, however, that the maximum saturation values varied depending on the sensor, with values between 98 % to 104 % (Fig. A2). We, therefore, decided to set a rH threshold (rH_t) that is based on the data distribution of the sensor to account for the individual uncertainty when the air is nearly saturated, for systematic biases, and for drifts. In our study ΔW is attributed to fog when rH_t = 97.1 % which is the 90^{th} percentile of the rH sensor records measured at 1 m height."

We added the sensor height to Figure 1 to make it also more clear that for fog, we look at sensors at 1 m height and for dew, the conditions within the canopy are considered.

At our site, advection fog is negligible which is reflected in low wind speeds and no constant wind direction during fog events.

Figure 4: move the text in the dial at upper right so that there are no overlaps

Figure 4: reduce the size of the end marks of the whiskers

Thanks for the nice suggestions. We implemented both suggestions in Figure 4.

Figure A1: there is an error on the x-axis, this is rH as a fraction, not in \%. Moreover Oswin (1946) and Lewicki (200?) are missing in references. And please don't chomp off the right part of the display. I also cannot see the 95$^\textrm{th}$ percentile in my printout, and the 85$^\textrm{th}$ would profit from a thicker line width. Why do you use x and y in the equation when your variables are actually rH and SWC? You never defined that x = rH and y = SWC.

We added the missing references and applied all suggestions to improve Figure A1. We identified that the 95th percentile could not be computed despite trying several starting estimates and decided to take out the lines illustrating the 85th and 95th percentile to stay consistent.

Table A2: The caption claims that the units of all values are hours day^{-1}, but the table heading claims that this is only hours; and then there might be an error: if cor means correlation, a unitless and dimensionless information, both are not correct. To be standalone you must define cor, mae and rmse (you don't use mse defined on the previous page). Moreover, I cannot see what you want me to see according to the text (see comment elsewhere)

We changed the equations in A2 to complete the definition of cor, mae and rmse and added the units individually to each column in table A2.

DETAILS

32: Meissner et al. is not a complete reference, year etc. are missing

Thank you for pointing this out, we corrected it in the manuscript.

42: what do you mean with ``modeling frequency''---not intelligible to me

We understand how the wording of the sentence was clumsy and now changed it to make it more clear:

“Modeling NRWI also remains a challenge, although a distinction must be made between modeling the frequency and duration of occurrence, and modeling the yields of the different NRWI fluxes.”

51: please add Riedl et al., accepted on 16 November 2021 at HESS. Last manuscript version available here (note that one more co-author appears in the finally accepted version): http://homepage.usys.ethz.ch/eugsterw/publications/pdf/Riedl.2021.FINAL.pdf, discussion paper version available via https://doi.org/10.5194/hess-2021-317

We added the reference as suggested.

77 and elsewhere: data are plural in formal English, please correct

Thank you for pointing out this language-related error, we changed the respective sentences in the whole manuscript

81: \textit{ilex} should be lower case; and remove the excessive white space before 20 trees

We changed ilex to lower case, and identified the reason for the white space, there was a problem with the latex command to insert a tilde.

95--96: ``They rest \ldots'': this sentence is not intelligible to me, please rephrase in an understandable way

We rephrased the sentence to:

"Each column has a 1 m^2 surface area and 1.20 m column depth and is situated on a weighing system consisting of three precision shear-stress cells, respectively (Model 3510, Stainless Steel Shear Beam Load Cell, VPG Transducers, Heilbronn, Germany)."

112: add ``and'' (Pt-100 and capacitive \ldots)

We changed the sentence as suggested.

115: add ``The'' in The Netherlands

We changed the sentence as suggested.

120 and elsewhere: u_* always has the asterisk in subscript, never in superscript

We made the change to the variable as suggested.

120: add reduced space between m and s

In the original latex version of the manuscript we use consequently the siunitx-package, the spacing is done within the command and we would prefer to not change this for individual units.

121: add a comma in R3-50, Gill \ldots

We added the comma as suggested.

127 and elsewhere: note that Figure should be upper case if a specific figure of your manuscript is referenced; it is however lower case if you use figure for ``Zahl'' or ``Wert'' in German

Thank you for pointing this out as well as the clarification. We changed figure to Fig. as specified in the guidelines from hess.

131: delete ``together''

Was changed accordingly.

139: not a number is \textbf{NaN}, whereas \textbf{NA} means not available (it is the code for missing values). That's wrong here. I assume you mean not available \textbf{NA}.

Yes indeed, we mean not available. We changed the expression accordingly.

184: add s to describe\textbf{s}

Was changed accordingly.

190: remove s from model predictions (not model\textbf{s} \ldots)

Was changed accordingly.

207: δ is conventionally only used for isotopic ratios, ∂ is used for partial differentials, and Δ is used for finite differences. Here I think using Δq instead of δq would reduce confusion if readers are familiar with isotopes, and personally, I even think that using capital delta is the correct notation anyway.

Was changed accordingly.

209: the Clausius-Clapeyron relationship is not a straight line and thus ``slope'' is the wrong word here. s is actually de/dT; thus rewording is required

We reworded and hope that the wording is more accurate now.

“s (Pa K^{-1}) is the derivative of the saturation vapor pressure curve defined as (de $_{sat}$/d T)”

209: there is an error here, $\lamda E$ is not the latent heat (of whatever), but the latent heat \textbf{flux}. Please correct.

Was changed accordingly.

221: you defined γ to be in units of Pa K^{-1}, but here the implicit assumption is that is is in kPa K^{-1}. Stick to your definitions; if readers use the equation as is with saturation pressures in kPa, then the first term is 1000 times too large.

Thank you very much for looking deeper into this, I was indeed mistaken here. We changed it in the finalized manuscript.

223: C_p is not specific heat of air. It is the specific heat \textbf{capacity} of the air at constant pressure. Please correct.

Was changed accordingly.

227: replace moments with periods

Was changed accordingly.

229: in the text you correctly use ``diel'', probably being aware that diurnal can also express the opposite of nocturnal. My suggestion is to modify the subsection title to match the text (diel)

Thank you, you are totally right, we changed the section title accordingly.

247: should be ``its'' without apostrophe

Was changed accordingly.

254: a sum has no $\pm$ unless you specify in M\&M how you obtained the uncertainty (the reason: random errors of the mean have an average of zero, and thus for a sum there are no degrees of freedom to specify a random uncertainty). Please correct.

We added a sentence in section 2.2.1 to clarify the origin of the uncertainty.

“Fluxes are presented in the results section as mean and standard deviation across the five lysimeter columns.”

258: you arbitrarily change from ET to ET -- please homogenise (and define the version you keep)

Thank you for this suggestion, we revised the text again for homogeneous use of ET.

263: your total of the components is 41.9 mm, but you specify 42.0 mm. The convention is to either specify the component that contains the missing 0.1 mm or to round accordingly if no additional component is part of the game. Note that modern round rules round 0.05 to 0.00 but 0.15 to 0.20 (this is essential to avoid drift). Another conventional rule is to round up the component that was closest to rounding down, to correctly represent the reported total.

Thank you for having found this error. It should indeed be 41.9 mm in the text. We corrected it accordingly.

264: I am surprised to see 50.6\% vapor adsorption. This seems to be quite a large value, but maybe is correct in this ecosystem. Here some independent (e.g. blotting paper) validation of the components would really have strengthened the paper.

Yes, we agree with you regarding the fraction of adsorption from the total NRWI sum. This is one of the reasons why we presented this manuscript since little literature exists on periods of the length of a year which limits such comparisons. Of course, these values still could have a substantial interannual variability.

Regarding the field campaigns for validation measurements, we also agree. Unfortunately, nearly all field trips and potential campaigns in 2020 were canceled due to the Pandemic restrictions. But for the scope of the PhD project, it is crucial to continue now with the next step of the project.

294: ``Our observation that especially nights 295 are prone to the formation of NRWI is also documented in the literature. '' This is an utterly trivial statement, do you really want to keep this in a scientific manuscript? For me it is on the same level as ``the grass was green and photosynthesis was important during daylight hours, as reported in the literature'' \ldots

Yes, it is correct that nighttime dew formation is common knowledge. But still, we consider the sentence here as a way to guide the reader into the next sentences where we explain that particularly for adsorption, the observed timing differs from site to site (l.299). Some authors also reported nighttime adsorption while others observed the strongest flux between noon and sunset (Qubaja et al. 2020, Verhoef et al. 2006).

336: should be ``its'' without apostrophe

Was changed accordingly.

379: use ``scale up'' instead of ``up scaling''

Was changed accordingly.

Figure 3: suggestion to move the legend to the panels and only show the curves that relate to the respective panel. In the legend some colors are hard to distinguish as is. Moreover, having a legend outside the plot area is Excel standard, not with scientific presentations.

Figure 3: use \verb+par(lend=1)+ to avoid the rounded (and thus unclear) endings of the bars

We included all suggested changes in Figure 3.

Figure 5: panel (e) is not described. I assume that the second mentioning of (d) should actually be (e)

Thank you for pointing out this error. We changed d) to e) as you suggested.

General: \LaTeX typesets equation by assuming that characters are variables (if they are known), hence rH and SWC look odd in your text. Consider using \verb+\mathrm{rH}+ and \verb+\mathrm{SWC}+ instead

Our reasoning for this decision to have rH and SWC in italic is that we tried to consistently write in italic all symbols representing physical quantities or variables. They are defined within the LaTeX document in the glossary file and listed in the manuscript with their respective unit in A1. We would prefer therefore to keep them as they currently appear in the manuscript.

References: add doi or URL to Thom et al. (with scanned papers the doi is normally only shown on the publisher's website)

References: add doi to Sonntag et al.

Unfortunately, there is no doi available.

References: add space before parenthesis in Zhang et al. (2019a)

References: check Dirks et al.

References: check Kosmas et al., seems to be an incomplete / corrupted entry

References: check Nair et al.

References: check Peters et al. (2014)

References: check Rodrigues-Iturbe et al.

References: generally only the doi is necessary, not doi resolved by the standard doi resolver plus the doi with the publisher's doi resolver and/or an alternative URL. See https://www.hydrology-and-earth-system-sciences.net/submission.html#references

References: generally rectify the entries according to the guidelines. Paper titles are normally in sentence case whereas journal names and book titles are using capitalised words

*Thank you for pointing this out, we used the Bibtex Bibliographic Style File from the hess submission guidelines. Theoretically, the style file would ensure that only the entries relevant for the standard hess reference list from the *.bib file are considered.*

As you identified there seems to be a problem concerning the URL-style and styles for papers and journal names and book titles. We tried an updated stylefile version without success and will therefore clarify with the editor or during final typesetting.

References: is there no doi/URL for IUSS \ldots

We checked again but couldn't find any.

References: Meissner et al. is incomplete

References: Monteith is incomplete

References: Orchiston is incomplete

Thank you very much for pointing out all the issues in the list of references. Where not indicated differently, the entry was revised and completed.

The editor is informed about my (friendly) long-term relationship with some of the co-authors.

PS: sorry for the LaTeX markups, I was not aware that HESS removed that option this year …

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