Response to Editor and Reviewers' comments

We thank the editor and reviewer for their careful reassessment of our revised manuscript. We did our best to benefit from all feedback to further improve our manuscript. We provide our responses below in blue colour and hope that with these modifications our manuscript can now be accepted for publication in HESS.

Main changes

Besides the changes commented by two reviewers, the main changes are as below:

- we changed Eq. 22 and Eq. 23 to get rid of using wind speed and temperature gradients that we assumed in the old version, see methods at L372-380, Figure 10, and results at L526-535. For example, the amount of aNRW was changed from “0.07 – 0.38 mm” to “0.16–0.57 mm ”. We added the discussion of stability term in Eq. 22, see L647-652. This did not change our analysis and conclusion.

Editor comment

Your revised manuscript has now been seen by two referees that have previously also reviewed your original submission. I am happy to see that both referees agree in their assessment that the manuscript has improved substantially. However they both also list a number of comments that need to be addressed before I can consider the manuscript for final publication. If you manage to address all remaining issues, I will not iterate the manuscript again with the reviewers, but I will evaluate the next iteration myself in order to save time.

Looking to forward to receiving your revised version, which should include a point-by-point response to the issues raised.

Report #1

The authors have done a rigorous revision of the paper and provided a thorough response to the reviewer remarks. The manuscript reads much better now and is more accessible to a wider audience. There are a number of minor remaining aspects that need to be addressed before the manuscript can be published.

Remarks:

Ln 10: ...the soil....

Changed as suggested at L10.

Ln 179: add a period after (2008)

Changed as suggested. A blank space was added after “(2008)”. L179

Ln 196: Still I think the formula used here is incomplete. Although I know the books by Stull and Garratt, a more recent book by Moene and Van Dam mentions to include the upwelling component that acts as reflecting part of the downwelling radiation in their formula 2.28:

Typical values for the surface emissivity can be found in Table 2.1. For most surface types ε_s is between 0.9 and 0.99. This implies that there is *some* reflection of longwave radiation.[2] Hence, the total upwelling longwave radiation is the sum of the emitted radiation, $L_e^{\uparrow}$ (see earlier) and the reflected radiation:

$$L^{\uparrow} = L_e^{\uparrow} + (1-\varepsilon_s)L^{\downarrow} \quad (2.28)$$

Your conclusions will not necessarily change, but I think it would be good to have insight in the impact of the reflecting part.

We replace LW_{out} by $LW_{surface}$ in Eq. (1) and added the information that we now derived $LW_{surface}$ using the first-order reflection of down-welling longwave radiation to conform with Moene and van Dam (2014). This changes the dew yield by ca. 0.002 mm per night of a total of 0.04–0.05 mm, which means that this modification changes our previous estimates by 4–5%. This is within the overall experimental uncertainty and thus did not change our findings. L196-201.

Ln 313: I think the approach using COSMO is a good alternative compared to ECMWF data. Nevertheless all models do have difficulties with representing nocturnal boundary layer (depth)s, so perhaps a few words mentioning this uncertainty would be meaningful here. Also a few words of discussion about whether a different definition of the nocturnal PBL height would have affected your results would be illustrative and may help to defend the robustness of your results.

We added that "The top of NBL is difficult to quantify, because in many cases the NBL does not have a strong demarcation at its top. Therefore, many definitions of the NBL are based on relative comparisons of the stable boundary layer state aloft to near-surface state (Stull, 1988)." L317-319

We then further clarified (L326–329) that: "Here we simply use NBL as a background information on atmospheric stability, but did not use it for nocturnal boundary layer budgets (Denmead et al., 1996) as was done by Stieger et al. (2015) at this exact same site, and thus the uncertainty in the exact value extracted for the NBL top from the COSMO-1 model output has no influence on our dew estimates."

Denmead, O. T., Raupach, M. R., Dunin, F. X., Cleugh, H. A., and Leuning, R.: Boundary layer budgets for regional estimates of scalar fluxes, Global Change Biology, 2, 255-264, https://doi.org/10.1111/j.1365-2486.1996.tb00077.x, 1996.

Stieger, J., Bamberger, I., Buchmann, N., and Eugster, W.: Validation of farm-scale methane emissions using nocturnal boundary layer budgets, Atmos. Chem. Phys., 15, 14055-14069, https://doi.org/10.5194/acp-15-14055-2015, 2015.

Ln 361: following as follows (Monteith, 1957). Reword to "as follows" or "following M57"

Changed as suggested. L372.

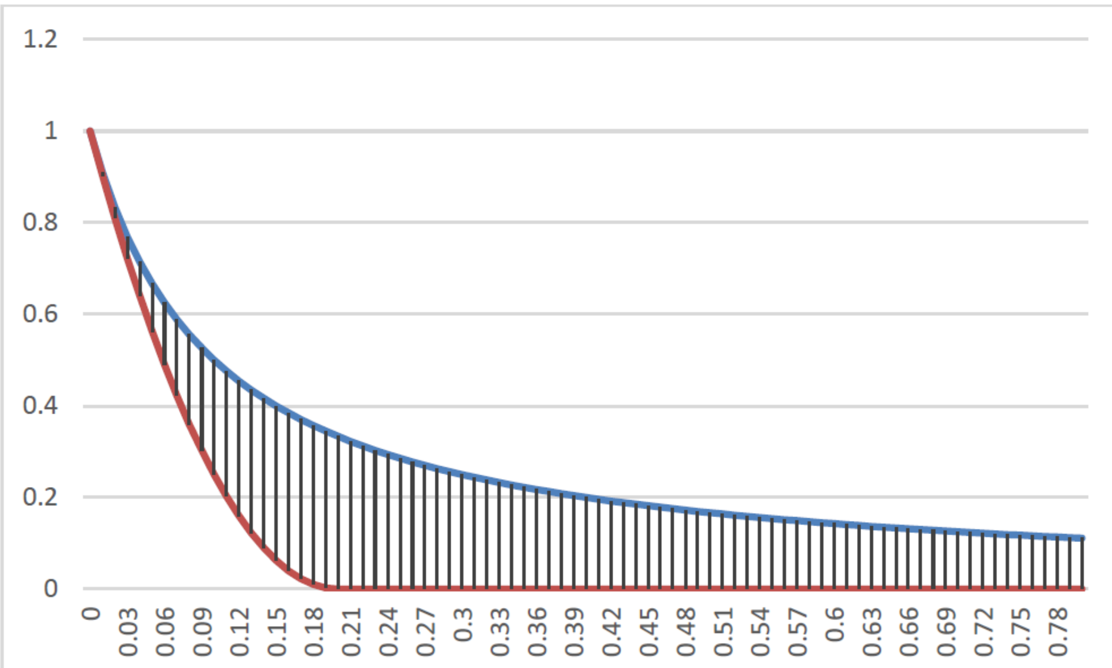
Ln 363: horizontal wind speed gradient => reword, it is the vertical gradient of the horizontal wind.

We replace Eq. 22

$F = \frac{\kappa^2 \cdot z^2 \cdot (\frac{\partial u}{\partial z}) \cdot (\frac{\partial \chi}{\partial z})}{1 + \sigma \cdot Ri}$ by $F = \frac{\kappa^2 \cdot z_{2m} \cdot u \cdot (\frac{\partial \chi}{\partial z})}{\ln(\frac{z}{z_0})} \cdot \Phi$

therefore the term du/dz was removed from new Eq. (22). L372-380.

Equation 22: The exchange function 1/(1+10*Ri) is rather under debate in the field of stable boundary layer research. I would say the used function is very effective in transport, more than is traditionally used in micrometeorology, where (1-5*Ri)^2 is used (without diffusion for Ri>0.2). With that function much less diffusion occurs (see red line, blue line is your function). Please provide an assessment whether this affects your results.



We agree that using the term $(1 – 5\ \mathrm{Ri})^2$ would substantially change the results, therefore we addressed this issue at L651-653 "In future research, we recommend combining isotopic composition measurements with lysimetric measurements to partition NRW from ambient water vapor and distillation. This would provide a useful benchmark to better evaluate the isotope-based estimates of NRW inputs."

In addition, we added the discussion of the uncertainty arising from the stability term (L647-651) "In the M57 approach as shown in Eq. 22, the stability term $\Phi=1/(1+10 \cdot Ri)$ was used. However, the stability term is sometimes written as $\Phi = [1 – 16 \cdot (z – z_d)/L]^{-0.5} = [1 – 16 \cdot Ri]^{-0.5}$ for Ri < – 0.1, and $\Phi = [1 + 5 \cdot (z – z_d)/L] = [1 – 5 \cdot Ri]^{-1}$ for $– 0.1 \leq \mathrm{Ri} \leq 1$ as e.g. in Monteith and Unsworth (2013), which would cause higher condensation rates when using Eq. 22 (see Fig. E1 in Appendix E), hence lower relative contribution of distillation in the total NRW than given the term $\Phi=1/(1+10 \cdot Ri)$."

Equation 23: Maybe I missed it, but how did you calculate or measure the partial derivatives du/dz and dT/dz? As you do not have tower measurements you cannot measure them. In case you would have a tower, then you can only use the bulk Richardson number.

Thanks for pointing out this. In the old version, we assumed that u =0 at $z = z_0 + z_d$ to try to approximate the partial derivatives used by M57 as closely as possible from our available measurements. We however agreed that this assumption caused very large uncertainty of the condensation rate in Eq. 22. Because we now have eddy covariance flux measurement available, we determined *Ri* from the Monin-Obukhov (1954) stability parameter z/L that is directly measured by eddy covariance. Therefore, we replaced the old Eq. 22

$$F = \frac{\kappa^2 \cdot z^2 \cdot (\frac{\partial u}{\partial z}) \cdot (\frac{\partial \chi}{\partial z})}{1 + \sigma \cdot Ri} \quad \text{by} \quad F = \frac{\kappa^2 \cdot z_{2\mathrm{m}} \cdot u \cdot (\frac{\partial \chi}{\partial z})}{\ln(\frac{z}{z_0})} \cdot \Phi$$

and replaced Eq. 23 $Ri = \frac{g}{T_\mathrm{a}} \frac{(\frac{\partial T}{\partial z})}{(\frac{\partial u}{\partial z})^2}$ by $Ri = \frac{z_{2\mathrm{m}}/\mathrm{L}}{1+5 \cdot z_{2\mathrm{m}}/\mathrm{L}}$

By making these changes, we could avoid making estimates for d*u*/d*z*, and d*T*/d*z*. See details at L372-380. This caused higher aNRW rate, but makes our two end-member mixing model more close to M57 approach (see results L526-535, and Figure 10).

Figure 3: Since COSMO is now used the reference to Hersbach et al., 2020 is not relevant anymore?

Thanks for pointing out the mistake. We changed the reference into “(Doms et al., 2018; Westerhuis et al., 2020)”. See Figure 3 caption.

Figures: I think it is good for the readability to list in the captions also what P1a,b, and P2a,b are. Now the reader has to go back to the start of the paper to rediscover.

We added the captions for P1a, b, and P2a, b in the related figures 5, ,6 ,7 ,8 ,10,and 11.

Reviewer #2

The authors considered and clarified within their revised manuscript most of my concerns. Please find from below some last comments or open question. Thanks again for this nice study and analysis on non-rainfall water based on isotopic data, which was a very interesting read.

Specific comments:

L96: Not all surfaces are cover by vegetation so I recommend to generalized it and state [..] dew formation: 1) the downward pathway through the condensation of water vapor on the plant and/or soil surface.[….] or write Monteith (1957) identified two input pathways for dew formation on surface covered by vegetation.

We changed it as suggested. "1) the downward pathway through the condensation of ambient water vapor on the plants and/or on soil surface" L95-96

L215-217: To my understanding it appears that the main rain deficit occurred before event 1 (-44%). After this rain was rather similar to the long-term mean year-to-precipitation as the deficit in % did not further increase nor decrease substantially. However, this also means that the rain after event 1 was not sufficiently to refill the depleted soil water storage during the other periods, which explains why the observed soil water content remained low until event 3.

We added this explanation after the SWC description. "The rainfall after event 1 was not sufficiently to refill the deficient soil water storage, which explains why the observed SWC remained low until event 3." L231-232

L232-237: thanks for the clarification. However, for event 2 bi-hourly values were taken for NRW droplets and leaf samples. Are these leaf samples correspond to that of the NRW droplets? In addition, dew or fog accumulates over time on the plant surface, so it would be good to make clear if you actually measured the NRW droplets on same leaves of the corresponding plant or on different leaves of the same plant species in the area?

Why this might be important: NRW droplets evolve over time, so if you measure on the same plant in the night you actually can measure NRW for a specific time period, or if you measure it on different leaves than you basically sample the accumulated NRW signature since the start of the NRW formation. Please clarify this here.

We added "randomly" into the sentence as shown at L235-236:

"NRW droplets on foliage (fNRW) were absorbed in triplicates with cotton balls from the leaf surfaces of randomly selected plants". This means the NRW samples we took from the leaf surfaces were accumulated NRW.

And at L240-241:

"Simultaneously, leaf samples were taken in triplicates from the randomly selected plants for the three species after softly drying the leaf surfaces with tissue paper.".

We added at L241-243:

"To prevent the disturbance of destructive sampling on the effect of dew and fog formations, the NRW droplets and leaf samples were taken from different plants of the same species in the sampling area."

L239-241: please also describe more in detail how bi-hourly soil samples were taken

We added an explanation into the sentence as shown at L245-246:

“soil samples in event 2 was taken without replicate within 2 h before sunset, as well as every two hours (i.e., four times of sampling in event 2) during the night”.

L382: please add abbreviation RH behind relative humidity

We added “(RH)” after “relative humidity”. L395.

L354: please change to: which was computed from the simulated wet vegetation surface temperature T_{0W}, and the observed soil….

We changed the sentence into “which was computed from the simulated wet vegetation surface temperature T_{0w}, and soil temperature T_{s1cm}”. L365-366.

L585: I guess different root water uptake might be also a factor here (different depths and amount of water).

We added “and root water uptake” at L597:

“In our study, we found significant among-species differences in δ^{18}O and δ^{2}H of leaf water (Table 2), most likely resulting from species-specific leaf water evaporation and root water uptake”.

L621-623: This might in addition also relate to spatial variability of shallow soil moisture and its stable isotope values

We added at L634-635:

“Furthermore, the spatial variability of shallow soil water content and its isotopic composition might enlarge the variability of δ^{18}O and δ^{2}H for distillation.”

L625: Indeed that would be helpful! But I think it might be also related to the way how to measure NRW on leaves. Taking the moisture from the leaves by cotton balls will first disturb the further formation or condensation of dew or fog on the leaf (in case sampling occurs at the same leaf during the night), or in case that NRW was sampled at nearby leaves that water taken from the leaf contains water and isotopic signature that is representative for the entire period since start of formation of NRW.

We added at L641-642:

“Another reason might be that the NRW droplets we took from the different leaves represent the accumulated NRW, while the temporal variability of NRW droplets on foliage might enlarge the uncertainty of NRW partitioning.”

And also in methods L235-243.

L640-644: Please make two sentence out of it and omit as already mentioned in the first round to place too much information into brackets.

We changed it into two sentences:

“The ecological relevance of distillation can be expected if the transfer of moisture is from one hydrological pool that is inaccessible to plants to another that is actually accessible to plants. For example, distillation could transfer soil-diffusing vapor from layers deeper than the effective rooting zone of grassland to droplets forming or depositing on leaf surfaces or surface soil where it can be accessed by the fine roots.” (L660-664)