Author comment on RC1

Anonymous Referee #1

The manuscript presents an interesting topic on non-rainfall water. The authors analyses for 3 events the water in the atmosphere and on the plants of an temperate grassland in Central Europe. The authors report data from a well-equipped test site and showed based on observation that dew formation and fog deposition are an overlooked part of the water cycle at such locations. The manuscript is overall well written, but the structure of the subchapter sometimes makes it difficult to follow the red line, and how this helps to answer the formulated aims of the manuscript. Several aspects in the manuscript require a further improvement; clarification and especially a broader discussion of their results including results on the third objective (see further details in "General and Specific comments"). I want here to emphasize that it was a very interesting read and that the topic is of current interest for readership of HESS. I recommend a major revision and encourage the authors to carefully rewrite, revise and improve their manuscript.

We thank the reviewer for her/his constructive comments (i.e., using Monteith (1957) equation to compute distillation rate, and adding more details in M&M) and positive feedback. We provide our answers point-by-point below.

General comments:

1) A lot of subchapters and abbreviations makes it sometime difficult to follow the red line of the manuscript. I suggest restructuring the chapter/section in order to answer to aims/objectives of the investigation.

- we will remove third level titles in section 3.1;

- we will combine sections 3.2 and 3.3;

- we will combine part of section 4.1 into results section, and merge sections 4.1 and 4.3.

- we will try to remove some of the abbreviations (i.e., write them out) to make the reading more fluent.

2) I recommend adding a much broader discussion on the formation of NRW, including the parallel condensation of water by soil distillation and dew in the introduction.

- we will add some additional material on NRW formation pathways in the introduction.

3) NRW during prolong drought periods. Please use a common definition on the periods during the measurements e.g. term drought or hot days.

- we preferred "prolonged dry periods", because we addressed consecutive days without rainfall.

4) For the third objective, there is no data shown in the manuscript that could give new insight here in section results and authors only discuss potential impact of NRW on ecological functions. Please clarify by adding further points in section results and describe how this was done (M&M section) that justifies the mention the third objective. E.g. the authors could include soil moisture observations during events (section result). Then discuss based on this results their ecological relevance in the corresponding discussion section.

- we will add data on the isotopic composition of soil water, and soil water content, and discuss them in relation to our third objective.

5) There is the need to show the latent heat flux measured with the EC-tower in this manuscript, which might help to clarify some points referring the observations on fNRW or dDew. It would be also helpful to see if EC-station can even indicate the formation of dew at night.

- we will add latent heat flux, although it tells the similar story as F_{H2O} (EddyPro_maual, Equation 5-101, 5-102: https://www.licor.com/documents/1ium2zmwm6hl36yz9bu4). In calm dew nights, the uncertainty associated with EC measurements are large, because some of the underlying assumptions are not fulfilled. For example, in Figure4b, at around sunset in event 1, there was a downward flux, but condensation has not started yet (Figure5a, surface temperature had not cooled down below the dew point), so this might not be condensation, but the drainage of more humid air from aloft. Moreover, at around sunrise, there was a bigger download flux (Figure 4b), this might be entrainment from free troposphere.

6) Discussion on the outcomes of the results are very short and partially parts of 4.1 should be shift into result section.

-we will move part of section 4.1 into the section results.

7) Add result on potential NRW section into the results section and explain in addition the used methods in the M&M section.

-we will add the result on potential NRW into sections M&M and results following the methods in Monteith (1957).

Specific comments:

L7: NRW is more than dew and fog. Thus, I recommend using here in the text: […] (hereafter NRW) mostly formed from dew and fog […]

-we will rewrite our sentence.

L10: I recommend changing: condensation of soil-diffusing vapor to condensation of water vapor evaporating from the soil in the canopy (i.e. soil distillation), […]. The processes described here by the authors is related to the term dewrise or soil distillation, whereby I recommend sticking with the latter term also use in Monteith (1957) within this manuscript.

- here we mean both capillary rise and gaseous transport of soil moisture, therefore we tried to get rid of using “evaporating”. Especially, when soil moisture is very low, the gaseous transport is dominant (L399-400).

L22: [...] (2) of soil-diffusing vapor. Please clarify that water from soil distillation was not measured in this study, but was determine/assumed as end member.

-we will do as suggested.

L22: Please clarify the sentence why a potential of 0.06 – 0.39 mm per night are comparable to 2.8 mm daytime ET. Even after reading the entire manuscript it wasn0t clear to me how the authors came up to this statement and values.

- we will rewrite our sentence to make it more understandable. The aim with this statement was to put the obtained NRW input in relation to daytime ET to underline its importance in the diel near-surface moisture budget.

L28: [...] water deposition. I recommend to change it to: [...] water condensation and deposition. Please differentiate in the manuscript for condensation (dew) and fog (deposition).

- ‘water deposition’ includes both dew formation and fog droplet deposition.

L30: [...] (hereafter NRW) inputs, namely dew and fog. Please name first all possible contributor to NRW on the soil or canopy surface: dew, fog, water vapor adsorption, soil distillation, and guttation.

-we will add all the possible types of NRW. We will mention that our study was conducted in the absence of precipitation, and low soil moisture availability, thus guttation occurring under high soil water content is not applicable in our case study.

L51: Please make clear that the authors refer here to the crop water use efficiency (WUE = ANPP/ET) as in other WUE definition only transpiration are used

-we will do as suggested.

L88: [...] onto foliage, [...]. Please change: onto the plant or soil surface.

-we will do as suggested.

L87-89: but water on plants can also stem from guttation. Please discuss this here and add also info on this at a later stage of the manuscript, how this might affect the results of the study, because water from this might be isotopically different, from other water sources in the plant-soil-atmospheric continuum (e.g. ambient water vapor, soil water, plant water).

-as answered for L30.

L92: delete After

-we will rewrite our sentence.

L103: [...] (3) assess the potential ecological relevance of NRW inputs. The authors report here observation for three events, but no further observations that could allow to make some statement on ecological relevance of NRW. I could not find any method used here to realize this in the Material & Method section and no results are reported within this manuscript on this point. Only in the discussion section, authors discuss potential impact of NRW on ecological functions! Please clarify the point that justifies this objective and how the authors answer this within the manuscript.

-we will add the effect of NRW inputs on the isotopic compositions of soil water.

L112-116: The authors report that rainfall amount in 2018 was 297 mm less than the long-term annual rainfall. In the next sentence, they report that during the 6 months period (April – September) the monthly rainfall, which was 81 mm, were reduced by 38% (49 mm). Something went wrong here, because 49 mm less rainfall per month (April-September) would mean a reduction of 60.5 % per month. I am also wondering that these reported values would mean that during the other months the rainfall was similar to the long terms values? As 6*49 = 294 mm and the total difference between 2018 and the long-term values was 297 mm.

- average level during 2006-2017 = 81+49=130mm, level in 2018 = 81 mm, 49 mm/130mm = 38%;

- 297 mm less is for the whole year, the calculated 294 mm is from April to September.

L117-118: The authors discuss their results in the light of a prolonged drought, but looking at the 3 measurement campaigns only the first event was within a month with less rainfall, because in August monthly rainfall was similar to long-term rainfall, and the monthly rainfall in September was with 130mm much larger as the long term mean rainfall (80 mm). If the authors want to relate their NRW results to term drought (especially important for the ecological relevance part), they need first to define this! Perhaps use better the definition of hot days during the extreme year 2018, instead of the term drought, as only the month in July showed a severe rainfall deficit and not the months August and September.

- we will use accumulated precipitation instead to better show the drought in 2018. The September precipitation was higher because of the heavy rain after our event 3.

L125-127: I recommend reformulating this sentence. The info in the brackets are larger than the rest of the sentence. Please change this for the whole manuscript, as relevant information should be mentioned directly in the sentence rather than in brackets.

-we will rewrite our sentences.

L132-133: Please reformulate this sentence: The EC measurements were processed to 30 min averages for evapotranspiration rate (mm h -1), [...], as half hourly values are not hourly values and please report it as actual evapotranspiration. By the way, I could found any results showing hourly actual evapotranspiration from EC-measurements in mm/h in the result section. Please show for the three events also half-hourly actual evapotranspiration in the Figure.

-we will do as suggested.

L152: It is not quite clear to me how the leaf water sample was taken. As this measurements are essential for the investigation I recommend to add some more sentences to clarify how the authors collected the water from the leaves and when (time before sunset, which is in summer already very early). What does it mean replicated fNRW samples? From where of the plant canopy sample were taken? Can the authors exclude from the form appearance of the water that it actual stems from guttation instead from dew formation? For event 2, bihourly samples were taken. Therefore, my question is, if the authors collected the water from the same leaves or from leaves of different plants during this event, which would make a difference for the collected water. Can the authors also say something on the plant species for which water was collected within each event and between the events?

-droplets on leaf surfaces were taken in the nighttime. It was randomly sampled. It was the short-statured grassland with 10-20 cm of the vegetation height. We took triplicates from three species (*Lolium sp*. (long-narrow leaf, higher plants), *Taraxacum sp*. (long-wide leaf, shorter plants), and *Trifolium spp*. (short-wide leaf, some are shorter and others are higher), thus 9 replicates in total. There was no significant difference of the droplet samples from different species. We will give more details in M&M. As answered for L30, we will add data of soil water content, and exclude the confusion of guttation.

L157: Not clear, what was measured here? [...] in soil moisture (hereafter δs). In addition I couldn't found anything on that measurements in the result section.

- We will add isotopic composition of soil water.

L166: Is it possible that the heating of the tube affected measurements?

- we tested the effect of heating with tap water: raw tap water (δ^{18}O= -11.4 ± 0.1‰, δ^{2}H = -81.1 ± 0.9‰; n=9); tap water after vacuum extraction (δ^{18}O= -11.2 ± 0.2‰, δ^{2}H = -82.1 ± 1.8‰; n=9). There was no significant difference of δ^{2}H (1.0‰; $p > 0.05$) between raw tap water and extracted tap water, and the difference of δ^{2}H between raw tap water and extracted tap water was within measurement uncertainty of δ^{2}H (better than ±1.0‰, L159) for IRMS. There was difference of δ^{18}O (0.2‰; $p < 0.05$) between raw tap water and extracted tap water, but much smaller than the observed δ^{18}O difference between fNRW and aNRW under unsaturated conditions (0.6‰, 0.9‰, and 0.3‰ for 03:00 CET of event 1, 23:00 and 01:00 CET of event 2 respectively, Fig.7a; Table 1).

L184: Please explain this more in detail

- We will add more details on how we calibrated the data.

L207: Please add also here a statement about guttation, e.g. under the assumption that guttation did not occur during the events....

-we will mention in section introduction that our events were in the absence of precipitation and low soil moisture availability to get rid of the confusion by guttation.

L202:224: Please explain this more in detail what was done here to determine the four unknowns in the eq. 2-4.

-we will give more details: with one time of sampling, we have 3 equations and 4 unknown values; with two times of sampling, we have 6 equations, and 8 unknown values; but we assumed $\delta18O_{dDew}$ and $\delta2H_{dDew}$ were constant for this two times of sampling, therefore, unknown values became 6, which can be solved with 6 equations.

L226: Please reformulate: [...] In unspecified explicit,[...]

-we will reformulate it.

L228: Could the authors also add the info why this type of regression was used here?

-we will add more details from Gat 1981 to explain this. Because $\delta^{18}O$ and $\delta^{2}H$ are always dependent each other, therefore orthogonal regression is recommended.

L247: add info where the reader can see this i.e. [...] levels (see Fig.xxx). I wonder why the authors do not show in addition to temperature and humidity the measured radiation variables from the EC-station.

- we will refer to the figures wherever needed.

- we will add radiation variables, although latent heat flux and evaporation rate tell the similar story as F_{H2O} (linearly correlated with each other).

L247-248: I recommend adding here the info that T_0 was estimated and not measured.

-we will note that T_0 was computed values.

L250: Was this before or after sunset for the specific event? Perhaps add text in Fig.5a and b that vertical lines shown are the times of sunset and sunrise. Also add in the figure caption what the vertical lines stands for.

-Yes, we will add the legends that vertical dash-lines represent “sunset/sunrise”.

L260: In the first event q_a decrease is very low in comparison to the other events! This event was also with the month of the large rainfall deficits. Are there any estimate or measurements of NRW amount available? E.g., showing the measured latent heat flux from the EC-tower or lysimeter, leaf wetness sensor or estimates based on any model that predicts dew formation.

-we will add latent heat flux, although F_{H2O} has already shown the transition of evaporation and condensation. We do have micro-lysimeter and leaf wetness measurements, but unfortunately the micro-lysimeter and leaf wetness data was not available in 2018. But we will calculate condensation rate according to Monteith (1957) as suggested below.

L271: Please explain the gaps in Fig. 6 a-d during P1b

-we will note in Figure 6 that "gaps were calibration periods".

L276: Please refer to δ^2H_a, $\delta^{18}O_a$ here instead of δ_a

-we will do as suggested.

L288: Looking at Fig. 6 a and b, there a partially large difference between fNRW and aNRW especially for first and second, but also to some extent for the third event. The authors report later that much of the dew comes from the soil itself and not atmosphere so I would not expect that fNRWand aNRW are identical! Please describe results more carefully here and discuss it later.

-we will do as suggested.

L293: [...] The relationships between the isotopic compositions of fNRW and aNRW were related to RH [...] please add in Fig. 7 a, b, c the RH on the second y-axis. As it is difficult to follow results until L300 without seeing measured isotopes and RH in one plot.

-we will do as suggested.

L302: Please explain the deviation of aNRW from the LMWL in Fig. 8. Does the position of aNRW below the LMWL means that aNRW stems from local ET water?

-yes, aNRW below the LMWL means that aNRW stems from local ET water. We will add the information as suggested.

L306: but for event 2-sampled fNRW under 97 to 98 are similar to that of aNRW. Others show large spread (deep purple triangles)? Is there no other reason that could explain the isotopic position of the samples fNRW that are much below the eq. line? E.g. nighttime evaporation processes of dew water on the leaf canopy. Would be good to check here the latent heat flux of the EC tower measurements for these times. Please add here for the discussion findings from Chen et al. (2019) (see Fig. 5), where data for soil, dewrise and dew water as well as vapor are shown.

-in L312-313 we mentioned, $\delta^{18}O$ and δ^2H of fNRW were higher and lower than aNRW respectively. Re-evaporation can occur, but should have caused both $\delta^{18}O$ and δ^2H of fNRW being higher. We will address this statement into discussion to make it more understandable.

L301-310: The authors mention in L157 the measurement of soil moisture (hereafter δs). I couldn't found a description of the data in the results section (already mentioned). Please add this here and describe it. This could clarify in Fig.8 where the water came from soil or evaporation of dew from canopy!

-we will add the isotopic compositions of soil moisture.

L301-310: another point might here that a mix of guttation water with dew might lead to a shift in the isotopic composition. It would at least fit as the deviation was seen for both events for the first sampled foliage water! Please at least mention it and discuss possible affects of guttation on stable isotope composition in the discussion e.g. see Xu et al. (2019).

-we will address in section introduction that our events were in the absence of precipitation and low soil availability, and remove the confusion of guttation;

- but guttation in discussion might distract readers from our storyline.

L311-320: Not clear to me how the authors finally estimate the contribution of dew or soil distillation on the collected dew water, when the amount of dew, fog or from soil distillation are unknown for the events! This should be clearly describe in the M&M section.

-we will calculate distillation rate following Monteith (1957), and add this in sections M&M and results.

L323: As aNRW are simulations, the uncertainty of the used assumptions to determine aNRW in the two end-member mixing model should be included and naNRW as well as dDew with naNRW should be reported in the result section.

- we split NRW under the assumption of equilibrium fractionation. We think include naNRW into results will distract readers from our storyline. But we will rearrange our structure.

L323-329: these are results and the used method of e.g. Wen et al. 2012 should be describe in the Material and Method section and results should be shown in the results section!

- as answered for L323.

L323-344: I recommend enlarging the discussion about the result here in a much broader context. Compare results with previous studies and discuss possible effects from e.g. guttation or dew re-evaporation on the sampled isotopic composition fNRW and the method on the partitioning of NRW inputs using a two end-member mixing model.

- we will add a broader discussion.

L351: Not sure about this reported values here. A) Please clarify how dDew was potentially 22-83% according to Monteiht 1957? B) Please report methods used here in the manuscript in the M&M section and not adding this info a Table caption (i.e. Table 2). C) More in detail, it is unclear how the authors come up with different times for dDew and aNRW. D) In addition, I recommend to use eq. mentioned in Monteith

1957 to calculate potential contribution of soil distillation and dew, present this result in the result section and compare it with the latent heat flux observations an than discuss it in this section 4.2.

- as recommended, we will use equation in Monteith (1957) to estimate distillation rate.

L353: Not clear to me how NRW gain is comparable to average ET of 2.8 mm? There were no results on actual NRW water, the authors only report potential NRW+soil distillation which were somehow taken from report rates in Monteith 1957. At least soil distillation is soil dependent and also depends on the canopy or? If soil is bare we might see evaporation instead of soil distillation. This means reported values are location dependent!

- we will rewrite this part.

L371-372: this values should be reported in the result section and added to the other plots to better distinguish from where water collected on leaves are coming from. From the M&M section it was also not clear how and when this samples were taken e.g. suction cups or destructive? Please add also this missing part in the M&M section

- we will add the contents as suggested.

L380: From my perspective, the reported results (until now) are not a direct indicator that soil evaporation is synchronously happen with condensation. Please reformulate this in a more careful way. Perhaps it would be worth to calculate potential dew and soil distillation based on the eq. from Monteith 1957.

- we will calculate distillation rate as suggested.

L389: Not clear to me why water that comes actually from the soil is not accessible for roots? Vapor transport might be largest during very dry conditions. However, this was only the case for first event. The other two event were observed during months with higher rainfall amounts than long-term average values.

- the events were during 4-5 consecutive days without precipitation. The confusion would be removed by adding accumulated precipitation and soil water content.

L391: Please discuss somewhere that the amount of water transferred by vapor transport from soil depends on soil properties.

- we will do as suggested.

L395: From which soil depths is this water coming from? Is this deeper than the effective rooting zone of the grassland? Would be an important point here to discuss, as only deeper than the roots zone located water would actual lead to a benefit of dDew for plants.

- we will add soil water content, and isotopic composition of soil water, and discuss more details.

L401-403: the authors reported the estimated wilting point of the soil in the M&M section. Would be worth to mention this somewhere in the Results section to see if soil was actually near the wilting point during the 3 events, which would emphasize that NRW could reduce water stress during this time and discuss this point, e.g. Groh et al. (2018) that the occurrence of dew during times with water stress might alleviate drought stress for plant.

- we will add soil water content, and isotopic composition of soil water.

L416: My recommendation for this section is to present less individual results and to focus more on answering the question/objectives of the study and its impact in a broader context.

- we will rewrite our conclusion.

Chen, G., Sun, L.Z., Auerswald, K., 2019. Effects of Wilting and Dew on the Water Isotope Composition of Detached Grass in Temperate Grassland. Journal of Agricultural and Food Chemistry, 67(34): 9460-9467, 10.1021/acs.jafc.9b02978.

Groh, J., Slawitsch, V., Herndl, M., Graf, A., Vereecken, H., Pütz, T., 2018. Determining dew and hoar frost formation for a low mountain range and alpine grassland site by weighable lysimeter. Journal of Hydrology, 563: 372-381, 10.1016/j.jhydrol.2018.06.009.

Xu, Y., Yi, Y., Yang, X., Dou, Y., 2019. Using Stable Hydrogen and Oxygen Isotopes to Distinguish the Sources of Plant Leaf Surface Moisture in an Urban Environment. Water, 11(11): 2287.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-493, 2020.

Monteith, J. L.: Dew, Quarterly Journal of the Royal Meteorological Society, 83, 322-341, https://doi.org/10.1002/qj.49708335706, 1957.

Author comment on RC2

Anonymous Referee #2

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Review of the manuscript hess-2020-493

The role of dew and radiation fog inputs in the local water cycling of a temperate grassland in Central Europe

by Yafei Li et al.

Summary This study investigates the role of fog and dew deposition in the water budget of a grassland in Switzerland. The authors aim to distinguish different pathways of the liquid water sources, e.g. fog deposition, dew deposition from the atmosphere to the surface, and dew deposition from the soil upwards towards the vegetation. The study uses isotopic composition of H and O in the water vapour in the atmosphere and the liquid water. I think the authors did an tremendous effort in performing a measurement campaign to measure these components during three different nights and in understanding the pathways. This is also an interesting new approach. My main criticism about this manuscript is that the description of all the isotopic ratio's and compositions is written in a too much technical way. The reader is offered a number of values without interpretation what it mean related to the three proposed pathways. In the current shape the paper is only interesting for experts in isotopic signatures and does not serve the wider fog research community, while I think this huge research effort deserves this wider audience. More detailed comments have been listed below.

Recommendation: Major revisions required

We thank the reviewer for her/his constructive comments (i.e., clarifying the motivation why isotopic method is needed, and revising the manuscript for wider audience) and positive feedback. We provide our answers point-by-point below.

Remarks

Ln 7: "In a warmer climate, non-rainfall water (hereafter NRW) formed from dew and fog potentially plays an increasingly important role in temperate grassland ecosystems under the scarcity of precipitation over prolonged periods". Please reword. I find this a confusing sentence, since warmer should be compared to a reference (warmer than....) and secondly I do not see the rationale that in a climate with high temperatures the relative contribution of occult precipitation will increase. Under climate change the hydrological cycle is expected to accelerate, which means more precipitation and thus less relative contribution by occult precipitation. Please rephrase.

- we will rewrite as suggested. We addressed that NRW is important during consecutive no-rain days.

Ln 11: remove "at all"

- we will rewrite as suggested.

Ln 13 : the abstract misses a statement why isotopes are needed to identify the pathways. I would say that if I install eddy covariance, a fog collector and a microlysimeter, I can also obtain the mechanisms contributing to the NRW budgets. So motivate why a more difficult method is needed.

- we will rewrite the statement why isotopes are needed.

-on the one hand, micro-lysimeter can quantify the condensation from ambient water vapor, but cannot quantify distillation. Because distillation is the internal cycle from one part (soil) to the other (leaf surfaces) (Monteith, 1957). That is why we addressed that isotopic measurements could be combined with micro-lysimeter to quantify distillation amount if we know the mixing rate of distillation and condensation from ambient water vapor using isotopic splitting, and the condensation amount from ambient water vapor using micro-lysimeter.

-on the other hand, EC measurements are uncertain during calm nights (friction velocity $u^* \leq 0.1$ m s^{-1} (Jacobs et al., 2006)). As shown in Jacobs et al. (2006), dew amounts by EC measurement was much smaller than the values from micro-lysimeter. Similarly, during the three nights in our study with dew formation and radiation fog deposition, u* was smaller than 0.06 m s^{-1}. Furthermore, as shown in Figure4b, F_{H2O} showed an abrupt downward flux, but this might be cold air drainage instead of condensation, because surface has not cooled down below dew point as shown in Figure5a; abrupt downward flux was also observed at around sunrise, which might be entrainment from free troposphere. The uncertainty of EC measurements will be quantified with the energy budget closure following Eugster and Siegrist (2000).

Ln 10-20: an interpretation should be provided what a certain permille for a certain isotope means. The reader is now overloaded with values without guidance about the interpretation. In such a way the paper is only interesting for a small incrowd.

- we will interpret our results in a broader way.

Ln 34-35: cite in chronological order, here and throughout the whole manuscript.

- we will do as suggested.

Ln 80-85: please add a few lines what are the physical reasons why local evaporation and entrainment at the PBL differ so much in *d*. This will help the non-involved reader.

- we will do as suggested. *d* value decreased with stronger non-equilibrium evaporation because of the varied diffusive velocity for different water molecules ($^1H^2H^{16}O$: $^1H^1H^{18}O$ = 0.9723: 0.9755). Continental evaporation is mostly non-equilibrium fractionation process. Local (continental) evaporation experienced stronger evaporation as compared to entrainment from free troposphere, thus had lower *d*.

- *d* = δ^2H–8* δ^{18}O; at equilibrium fractionation, $\Delta\delta^{18}$O: $\Delta\delta^2$H =1:8, hence *d* keeps rather constant; at non-equilibrium fractionation, $\Delta\delta^{18}$O: $\Delta\delta^2$H > 1:8, therefore evaporation would cause the decrease of *d*.

Ln91: in height: please be more precise. Do you mean in the soil?

- means a.g.l.; we will revise it.

Ln 104: please specify in more detail the what is meant by ecological relevance and how you will measure that.

- ecological relevance means the effect of NRW inputs on plants and soil moisture. We will revise in the next version.

Section 2.2.1: please add which software was used for the flux processing and with which settings.

- we will refer that eddypro processing was used.

Ln 130: I am quite concerned about the height of the flux measurement since 2.4 m is very close to the surface, which means that there will be a relatively large "flux loss". Please specify how much this is and whether it will influence your results.

- we will specify this as recommended. But "flux loss" would not affect our results, because we used isotopes instead of EC data to quantify our results. As recommended by the first reviewer (RC1), we will use the equation by Monteith (1957) to calculate distillation rate, and then the condensation rate of ambient water vapor. This condensation rate from isotopic splitting will be compared with the condensation rate by EC measurement to analyze the uncertainty of EC measurements in dew and radiation fog nights.

Ln 130: What happens to the contribution in the transport of the turbulence that happens below 2.4 m and is as such not seen by the EC sensor? Since the site is that the bottom of a valley I can imagine that thin katabatic flows are present from the valley walls to the valley and that they generate small scale turbulence. Does ignoring this component affect your conclusions. Please reflect and if possibly quantify.

- we will quantify the effect of katabatic flows using energy budge closure following Eugster and Siegrist (2000).

Ln 142: The equation is incomplete. The upwelling LW_up flux consists of sigma*Tˆ4 +(1-emiss)+LW_down and the latter component is missing. This would not have been a problem if the emissivity of the surface would equal 1, but you explicitly report it amounts to 0.98. Please recalculate your results.

- we used longwave-outgoing radiation instead of LW_up here, therefore no LW_down is needed here. But we will recheck this.

Ln 199: why wasn't potential temperature gradient used for the PBL height determination?

- we will use potential temperature gradient instead.

Ln 200-202: I think it is this method should be reconsidered. The NBL depth can vary spatially enormously, especially in complex terrain where the experiment was done (i.e. a valley) while the ECMWF product is at 30 km spatial resolution. Furthermore the vertical grid spacing of ECMWF is too coarse to detect the NBL height properly. Also the reported values are very high for nights where you can expect fog or dew. As a rule of thumb one can use that the NBL depth amounts to 700* u_star (friction velocity). That would mean that here the u_star would be 1 m/s and that is really really high for nights with fog or dew.

- we will use COSMO (http://www.cosmo-model.org/) model instead. The resolution is 4 km (meridional) × 6 km (zonal) over Switzerland (Westerhuis et al., 2020).

Ln 200-202: concerning Figure 3 I doubt whether the interpretation is correct since I think at the y axis the height above sea level is shown. The surface inversion should be at the surface (i.e. 0 m) right? Not at 650 m above ground level. This can also change the story about my previous point.

- There was a mistake in the computation of the vertical height. The ECMWF model will be replaced by COSMO model as answered above.

Ln 211: "while in saturated conditions, fNRW was a mix of aDew and aFog". I disagree on this since it is very hard to create fog in a night with a lot of dew at the same time. Dew takes out water vapour so fog in inhibited to develop. This contrasts with your statement.

- We stated in L58-64 that this is radiation fog. As shown in Figure4d, intermittent radiation fog occurred at our site. Not only events 2 and 3, combined dew and radiation fog is often observed at the CH-CHA site. It is true that dew takes out water vapor from near surface atmosphere (Figure5b), but both air temperature and surface temperature cooled down (Figure5a). This causes an increase of relative humidity at surface temperature (Figure4c).

- The inhibition of dew on fog might be true in the first hour of dew, as mentioned by Monteith (1957). Because "sufficient latent heat would be released to raise the temperature of the leaves above the dew point, preventing condensation until further cooling had taken place." But with the further cooling down of surface temperature and air temperature, the latent heat did not warm the temperature above dew-point (Figure5a).

Ln 213: typo: is -> as

- we will change as suggested.

Ln 222: It is good that you are honest about your assumption. But how realistic is the assumption. Could you spend a few words on it?

- we will give more statement as suggested. We will calculate distillation rate following Monteith (1957), and then the condensation rate will be calculated from splitting ratio, which will be compared with previous research.

Ln 248: net longwave radiation loss: can you be more quantitative? Was it -80, -50 or -10 W/m2

Figure 5: the top of panel b can be at 12 of 15 g/kg.

Section 3.2-3.4 are hard to follow and only useful for specialists in isotope measurements. The numbers a presented as a flood of values without discussion or interpretation what they mean. I did not get so much from these sections.

- we will revise as suggested, and restructured our results and discussion.

Ln 354: “This amount of NRW gain was comparable with the average evapotranspiration rate of 2.8 mm day-1 (daytime) during ...”. I do not understand what the authors want to say with this statement. How is dew at night comparable with evaporation during the day. The mechanisms are completely different!

- we want to give general concept how much is this NRW inputs, but we will rewrite to get rid of confusion.

Ln 377: “ minor influence of large-scale air advection”: this is in complete contrast to the large diurnal cycle of specific humidity that is clearly driven by katabatic flows, as shown by the authors.

- we will clarify this point. We mean the synoptic-scale flow has a minor influence because of the anticyclonic influence. Katabatic flows are density driven flows of mesoscale extent, induced by the local topography and the regional thermodynamic conditions in a situation with weak large-scale influence.

Ln 393: u-> u2m

Figure 10: I am not sure both panels are meaningful since in the definition of RH, the temperature plays an important role through the denominator in RH =q/q_sat(T). So I have the feeling we look twice at the same effect.

Formula B1: Perhaps I overlook something but I have the feeling that equation B1 is wrong when I compare it to Equation 3.19 in Campbell and Norman (1998). In CN98, the vapour concentration should be entered in mol/mol, but here in Pa. Please check, and check whether this affects your results.

- we will revise our figures, and wind speed abbreviation.

- no units for both our equation B1 and Equation 3.19 in Campbell and Norman (1998), it is just ratio. Here I showed how we rewrite this equation:

$$q_0 = \frac{0.622 \cdot C_{va}}{1-0.378 \cdot C_{va}} = \frac{0.622 \cdot \frac{e_{s0}}{p}}{1-0.378 \cdot \frac{e_{s0}}{p}} = \frac{0.622 \cdot e_{s0}}{p-0.378 \cdot e_{s0}} \left(\frac{kg}{kg}\right) = \frac{0.622 \cdot e_{s0}}{p-0.378 \cdot e_{s0}} \left(\frac{kg}{kg}\right) * 1000 \left(\frac{g}{kg}\right) = \frac{622 \cdot e_{s0}}{p-0.378 \cdot e_{s0}} \left(\frac{g}{kg}\right)$$

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