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Hydrology and Earth System Sciences
Discussions

Interactive comment on "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.

Anonymous Referee #1

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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 interest-

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ing 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.

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.

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.

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

4) For the third objective, there is no data shown in the manuscript that could give new insight here in the results section and authors only discuss potential impact of NRW on ecological functions. Please clarify by adding further points in results section 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 (result section). Then discuss based on this results their ecological relevance in the corresponding discussion section.

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.

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

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

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 [. . .]

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.

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.

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 wasn′t clear to me how the authors came up to this statement and values.

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

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

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

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

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sources in the plant-soil-atmospheric continuum (e.g. ambient water vapor, soil water, plant water).

L92: delete After

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.

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.

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.

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.

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.

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?

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

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

L184: Please explain this more in detail

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

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

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

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

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.

L247-248: I recommend adding here the info that T0 was estimated and not measured.

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.

L260: In the first event qa 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.

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

L276: Please refer to δ2Ha δ18Oa here instead of δa

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 fNRW and aNRW are identical! Please describe results more carefully here and discuss it later.

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.

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?

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.

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!

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).

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.

L323: As δaNRW are simulations, the uncertainty of the used assumptions to determine δaNRW 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.

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!

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.

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.

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!

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

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.

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.

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

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.

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.

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.

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