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# *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.

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Author comment on RC2

Anonymous Referee #2 Received and published: 15 December 2020 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



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Discussion paper

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, FH2O 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 (1H2H16O: 1H1H18O = 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.

The value d = $\delta$2H–8* $\delta$18O; at equilibrium fractionation, $\Delta\delta$18O: $\Delta\delta$2H =1:8, hence d keeps rather constant; at non-equilibrium fractionation, $\Delta\delta$18O: $\Delta\delta$2H > 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*TËĘ4 +(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 model instead. The resolution is 4âĂĽkm (meridional) × 6âĂĽkm (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. In supplement we showed how we rewrite this equation. ← ←

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

→ →

Eugster, W. and Siegrist, F.: The influence of nocturnal CO2 advection on CO2 flux measurements, Basic and Applied Ecology, 1, 177-188, https://doi.org/10.1078/1439-1791-00028, 2000.

Jacobs, A. F. G., Heusinkveld, B. G., Kruit, R. J. W., and Berkowicz, S. M.: Contribution of dew to the water budget of a grassland area in the Netherlands, Water Resources Research, 42, https://doi.org/10.1029/2005WR004055, 2006.

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

Westerhuis, S., Fuhrer, O., Cermak, J., and Eugster, W.: Identifying the key challenges for fog and low stratus forecasting in complex terrain, Quarterly Journal of the Royal

Meteorological Society, 146, 3347-3367, https://doi.org/10.1002/qj.3849, 2020.

← ←

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-493/hess-2020-493-AC2-supplement.pdf

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