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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 #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

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.

Ln 11: remove "at all"

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.

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.

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

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.

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

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

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

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.

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.

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.

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

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.

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.

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.

Ln 213: typo: is -> as

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?

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.

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!

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.

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.

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