Review of HESS 2024-110

Observation-driven model for calculating water harvesting potential from advective fog in (semi-)arid coastal regions. By Felipe Lobos-Roco, Jordi Vilà-Guerau de Arellano, and Camilo del Río.

General comments.

Estimating water harvesting potential from advective fog in (semi-)arid coastal regions is an important subject and well worth the effort of developing and evaluating a model. I do however have serious reservations about the building blocks in the model, both Appendix A and Section 2.1.2. In the fog collector model, the variable definitions are not clear and, as I see it, mixing ratios would better that specific humidity in this context. For the cloud base and related parameters it seems extremely optimistic to determine cloud base from near surface (1.5 m?) measurements of T and q at two stations and used to infer the vertical gradients. In the end a factor 0.5 is applied to the results (line 219).

I would like to see the model details cleaned up and more clearly explained

Detailed comments

line 75: "total specific humidity (q)". Maybe make it clear that this is mass of water vapor + droplets + (if present) ice/Total mass of a parcel, and presumably in kg/kg. Many authors will use q as a symbol for mixing ratio which in the atmospheric context of mass of water/mass of dry air.

line 79: Equation (1) appears to be dimensionally incorrect unless q_h is defined as a time integral of q with units like kgs/kg? Give a clear definition of "fog harvesting capacity, q_h". Same with q_{in} and q_{out} - are these amounts the inflow and outflow specific humidities. Same concerns in Appendix A

line 83: q_{in}, q_{out} and q_h appear to have same dimensions?

line 83: Assuming $q_s(in) \simeq q_s(out)$ implies no temperature change including any release of latent heat due to water vapor condensation on the grid. Will that be true? The rationale used, "given that q_l is by two orders of magnitude lower than q_s", is invalid. What matters is the size of q_l relative to any change in q_s!

line 89, 90 and Appendix A: Figure1 is useful, and makes it clear that q_h is NOT a specific humidity. Better to use a different symbol, and specify the units (maybe in kg rather than L/m^2). See notes at end.

line 122: How close to the shore is Fiego Aracina airport?

Figure 3: Points 10,11,12 seem to be missing. Negative correlations?

lines 134-139: I found this confusing. What time of day are the fog situations we are looking at? Earlier in the paper, sea breezes are cited as the cause of the flow from the ocean, so daytime, while here I am not sure. Does "stratified" mean stably stratified? Over water in the marine boundary layer solar radiation has only a small impact on sea surface temperature.

line 143+: 2.1.2 Cloud Base, CB It seems extremely optimistic to determine cloud base from near surface (1.5 m?) measurements of T and q at two stations used to infer the vertical gradients. The explanation is rather vague (Lines 160-170). The interpretation of the results seems rather generous. Is there any chance of some more comparisons with sounding data of some sort?

line 153: Equation (3) is probably wrong. Equations 3 and 4 in Lobos-Roco et al (2018) make sense, this does not. Also it is not clear to me exactly what subscript 1,2 means, and the iteration process needs more explanation.

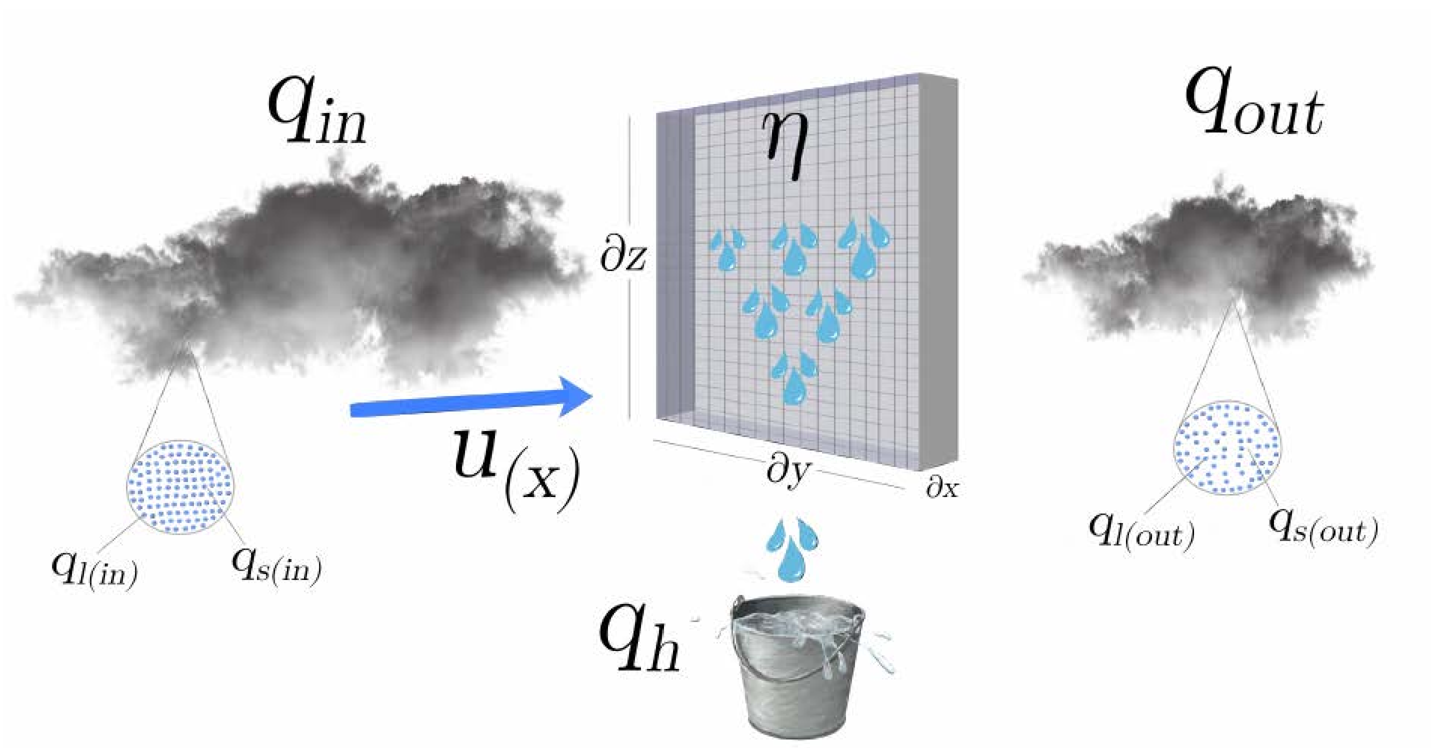
line 208+: There is a missing) in Eq (7) but it seems reasonable, Thus I was surprised to read (line 219) that " Our estimations of liquid water content obtained from Equation (7) systematically double the observed values. Consequently, we applied a correction factor of 0.5 to our estimations, as illustrated in Figure 5b. Something is wrong!

line 241+: Collection efficiency. Could this be determined in a laboratory wind tunnel study? Here it just looks like model tuning.

Fig 7b: Annual means seem wrong, based on monthly values shown.

Alternative analysis.

1) Use q, q_l, q_s as mixing ratios rather than specific humidity - since, with specific humidity, mass of total parcel will change from q_{in} to q_{out}.



Is the assumption that the air is always saturated, so that water vapor mixing ratio $q_v = q_s(T)$, necessary?

Work with fluxes of **liquid water**, and let A = dydz

Flux in, $Fl_{in} = \rho_a u A q_{lin}$; Flux out, $Fl_{out} = \rho_a u A q_{lout}$, both in kg/s,

Then water collection rate, $Q_h = Fl_{in} - Fl_{out} = \eta\, A dx\, Fl_{in}$, also in kg/s with η having dimensions m^{-3}. With a different symbol, Q_h, since this is different from other q usage.

This has assumed no vapor-liquid transfers as the parcel goes through the mesh. But is that true? Is there "dew" produced as well as cloud drops captured?
