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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Relative humidity gradients as a key constraint on terrestrial water and energy fluxes" *by* Yeonuk Kim et al.

Anonymous Referee #2

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Recommendation: major revision or reject with encouragement to resubmit

This manuscript deals with the role of relative humidity in evaporation estimates. It presents a novel approach to evaluate this role, and as such, I think this is very valuable. However, there are a couple of issues, some of which are major, that need to be addressed. I also had a hard time to follow the paper, so I think the authors need to work on a better structure. Also, the authors do not discuss any shortcomings of their study, showing a lack of critically assessing their own findings. There are certainly quite a few, as shown below, and these need to be assessed and discussed before any conclusions can be drawn from the analysis. As some of the major issues described below are likely to substantially change this manuscript in order to be addressed, at least major revisions need to be made, or, alternatively, the manuscript could be re-

jected with encouragement to resubmit, so that it would again enter an open review discussion.

Major comments:

1. "Land-atmosphere equilibrium". The authors describe that the relative humidity difference between the surface and the atmosphere is a key driver for evaporation, and its depletion is an indication of thermodynamic equilibrium. This picture is incorrect. The water vapor transport from the surface into the atmosphere is driven primarily by buoyancy, that is, by the sensible heat flux, not by the difference in vapor pressure. Hence, a depletion of the relative humidity difference is solely a reflection of well-mixed air near the surface. This misconception is reflected in much of the manuscript, with buoyant mixing not mentioned anywhere, so this needs to be addressed in a revision.

2. Shortcomings of the PM equation. The authors use the Penman Monteith (PM) equation as the basis of their work, although this equation has some clear deficiencies. This was, for instance, clearly shown in the paper by Milly and Dunne (2016, "Potential evapotranspiration and continental drying", Nature Climate Change), also by Renner et al. (2019, "Using phase lags to evaluate model biases in simulating the diurnal cycle of evapotranspiration: a case study in Luxembourg", HESS). The authors take the PM equation for granted, but I think they need to be more critical and evaluate potential flaws in their work in light of these shortcomings.

3. Role of advection. Literature by de Bruin (e.g., de Bruin et al., 2016, "A Thermodynamically Based Model for Actual Evapotranspiration of an Extensive Grass Field Close to FAO Reference, Suitable for Remote Sensing Application", J. Hydromet.) shows that it is basically the equilibrium evaporation term that dominates evaporation rates, and that the second term comes from advection, e.g. in the case of evaporation from irrigated land in an arid region. Given that the evaluation includes data from Costa Rica from an irrigated site during the dry season, advection could play quite an important role in shaping the evaporation estimate. But as far as I can tell, advection is not being

mentioned as a phenomenon in the manuscript. In addition to the shortcomings of the PM equation, I think there is a good reason to doubt the analysis so these factors need to be assessed.

4. Closure of the energy balance. The authors write on L177 that they did not enforce an energy balance closure, attributing the lack of closure due to unmeasured heat storage terms (but without quantifying and supporting this attribution). However, I do not think that this is a feasible way to do this analysis. It seems to me that the scientific consensus is that the imbalance is mostly attributable to secondary circulations in the convective boundary layer (see, e.g., the review by Mauder et al., 2020, Boundary Layer Meteorology, https://doi.org/10.1007/s10546-020-00529-6). I think this aspect also needs to be addressed thoroughly in the analysis.

Minor comments:

Title: "relative humidity gradients" are not a constraint. They are highly dependent on moisture and heating of air, hence not an independent variable, and they are not a physical constraint, such as those imposed by the energy- or water balance, or the laws of thermodynamics.

L34: "LE predictions remain highly uncertain" - I doubt this statement. The basic constraints for evaporation have been quite well established over decades, so what are the factors that remain uncertain? The authors need to be more specific than this wide-sweeping claim.

L38: Actually, the "pioneering work" on governing physics of LE started with Schmidt (1915), as described by de Bruin et al (reference provided above).

L46: "rh budget" - there is no rh budget, because relative humidity is not a conserved quantity, as it jointly depends on temperature and moisture content. So you can talk about an energy budget or a moisture budget, but not of a rh budget.

L51: I would not describe the PM equation as reflecting governing physics, it is at best

semi-empirical, with shortcomings (see above, major comment 2).

L69-83: I do not understand the derivation (and it seems odd to have this derivation partly in the introduction and partly in the appendix). What does surface relative humidity stand for? When we deal with a vegetated surface that experiences water limitation, then any water that does evaporate either diffuses out of the soil (for which I would think a surface resistance would be rather critical to consider) or it is evaporated inside a leaf, but with the exchange with the atmosphere constrained by stomatal conductance. So how do these equations (2) and (3) reflect a physical picture of the evaporation process?

Eq (4): As pointed out in the major comments, the second term in the PM equation is likely reflecting advective conditions (see paper by de Bruin et al., mentioned earlier).

L103: "When the vertical gradient of rh dissipates...": see Major Comment 1.

L114: Again, I do not understand the reasoning of the authors (see comment above, L69-83).

Figure 1: Why is the initial state represented by the atmospheric conditions, and the final state represented by the surface conditions? Doesn't evaporation start at the surface? Actually, evaporation may already start within the soil (bare soil evaporation) or inside leaves. How does this fit into this diagram? I find this quite confusing.

L120: "In the equilibrating process" - which process do you mean?

L120: "the air parcel is adiabatically cooled" - why is it cooled/lifted? Isn't it rather by the mixing of the moistened air from the surface with the unsaturated air of the atmosphere due to buoyancy that depletes the difference?

L149: "rh budget" - relative humidity is not a conserved quantity, so there is no budgeting. If you talk about mass conservation, you would need to formulate this in terms of specific humidity or similar.

L151: "This is logical in that LE_G itself operates to diminish the vertical rh gradient." No, it does not. Vertical mixing is related to buoyancy, not to VPD. Although mixing also depletes the rh difference, it is not the same process.

L152/53: The classical equilibrium evaporation rate $LE = s/(s + \gamma) * Q$ is not derived from the assumption of a saturated atmosphere. In the original derivation by Schmidt (1915), the only assumption is that the air immediately in contact with an open water surface is in thermodynamic equilibrium, so that the addition of energy is partitioned accordingly. But it does not assume that the atmosphere is saturated.

L194-201: I don't understand the need for the wavelet analysis. Why is it necessary? It seems to me that it makes the analysis more complicated than necessary.

L203-212: I am skeptical about the use of daily averages. Relative humidity, wind speeds, air and surface temperatures, and aerodynamic conductance show pronounced variations at the diurnal scale. How do you account for the covariations among these variables if you use daily means?

L220-226: Same question: How do you account for covariations among variables when you use daily mean forcing?

L229-234: I do not understand what the wavelet analysis should tell me. Why do you not simply use autocorrelations?

L240: That LE_G is close to zero in the absence of irrigation in 2016 supports the interpretation mentioned above that the second term in the PM equation relates to an advection effect.

L245: Figure 2: I would appreciate a little more information of the site - like precipitation input, solar radiation etc. to provide more background about the site.

L256: Does the case shown in Figure 3c represent a case with irrigation? Then, I guess, LE_G relates to advection effects?

L265: Figure 3: I would find it informative to also see H and net radiation, as well as the diurnal variations in the rh's.

L286: The statement that "the land surface is generally under thermodynamic equilibrium with the atmosphere at the global-annual scale" is, I think, incorrect. The finding that rhs $\approx$ rha simply means that the air near the surface is well mixed, likely due to buoyancy.

L322: "land-atmosphere equilibrium is achieved ..." - again, the authors neglect the role of buoyancy that mixes the air near the surface here and which is likely to play the dominant role in reducing the relative humidity difference.

L329-334: I think the implications need to be rethought, given that the role of buoyancy in depleting a difference in rh has been neglected.

L335: Conclusions - same here, given the methodological flaws of the study, this paragraph needs to be rethought.

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