Response to anonymous referee #2

Relative humidity gradients as a key constraint on terrestrial water and energy fluxes (HESS-2020-643)

March 14, 2021
Dear Anonymous Referee #2,

First of all, thank you very much for reviewing our manuscript.

We are pleased to receive your comments and suggestions. In the following, we present our detailed responses to your suggestions with our replies in blue; the specific revisions we intend to perform in underlined.

On behalf of all authors,

With regards,

Yeonuk Kim,

Corresponding author

1. General comment

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

We thank Reviewer #2 for the constructive feedback on the manuscript. We will endeavor to improve the structure of the manuscript and address the issues highlighted by the Reviewer in a revised manuscript. Below we provide point by point responses to issues raised along with how we will improve our manuscript based on your suggestions.

2. Major comment 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.

Thank you for pointing out the issue. In this criticism, the Reviewer implies that “equilibrium” is a concept related to atmospheric stability or generating vertical motion of a fluid. From this point of view, our “land-atmosphere equilibrium” would indeed be a misconception since it is not related to generating turbulent mixing. However, this interpretation overlooks other types of thermodynamic equilibria. Although the “equilibrium” term in this manuscript is not related to generating turbulence, it is tightly related to chemical equilibrium (Kleidon et al., 2009) (see L90~113), and steady-state equilibrium evaporation inside an atmospheric boundary layer (ABL) model (McColl et al., 2019) (see L141~155).

We agree that a depletion of the *rh* difference reflects well-mixed air near the surface. Nevertheless, this well-mixed situation can be understood as an “equilibrium” condition without representing a misconception. From a thermodynamic point of view, the LE_G term associated to the dissipation of the *rh* vertical gradient can be seen as a process producing entropy by bringing the chemical potential of the surface water to that of the atmosphere (Kleidon et al., 2009). Also, the term “equilibrium” in boundary layer meteorology is dynamically defined as the steady-state of an ABL (McNaughton and Jarvis, 1983;Raupach, 2001;McColl et al., 2019). For instance, McColl et al. (2019) stated that *“While there are various definitions of equilibrium ET, arguably the most fundamental is the evaporative state achieved by a closed system forced with constant incoming radiation that is partitioned at the lower boundary between latent and sensible heat fluxes”*. As we addressed in L141~155, evaporation approaches a steady-state of Equation (6) in an ABL model (McColl et al., 2019). We showed that this condition is equivalent to a depletion of the *rh* difference. Therefore, the proposed PM_{rh} model can be understood as an extension of the new theory by McColl et al. (2019); we highlight this point in relation to the equilibrium concept as presented in our study.

We agree that buoyancy generated by temperature gradients enhances vertical motion on top of the mechanic turbulence increasing diffusivity of eddies transporting both heat and vapour. To account for diffusivity in the flux gradient equation, buoyancy driven vertical motion is parameterized in aerodynamic resistance (r_a), which includes a thermal stability correction function (see equation (7)). Our derivation of PM_{rh} model stems from the flux gradient equation (see Appendix A), and thus buoyancy-driven energy is implicit in r_a. It should be noted that the *rh* difference between the land and the atmosphere does not affect r_a in our framework. We will more explicitly describe r_a in the revised manuscript along with a mention of buoyancy considerations to reduce confusion.

A key aspect to consider is that thermal gradients (and sensible heat flux) are not independent of the vapour gradients (and latent heat flux) and the *rh* gradients can help to capture these linked effects. For example, in a dry land situation (e.g. *rh* of land drier than *rh* of the atmosphere) typically the atmospheric thermal lapse rate will be negative. The $rh_s - rh_a$ will be negative and the LE_G acts to equilibrate the drier land. The role of the thermal gradients is implicit in the e^* (saturation vapour pressure): the larger the thermal gradients the more negative will be the LE_G term and the effect of thermal convection or buoyancy (r_a will decrease) will further enhance this. We will discuss the thermal lapse rate effect on the evapotranspiration components in the next version.

3. Major comment 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.

We agree that the PM equation has several limitations. However, we did not intend to take the PM equation for granted. Rather, the proposed PM_{rh} model is introduced in order not to include surface resistance in the evaporation model; surface resistance is the key parameter of the PM equation (please see L64~70). The above-referred studies (Milly and Dunne, 2016;Renner et al., 2019) showed that bias introduced by the PM equation mostly originates from inaccurate representation of surface resistance (or conductance), and our proposed PM_{rh} model does not include surface resistance as a means to overcome this shortcoming in the PM equation.

Of course, the proposed PM_{rh} model shares other shortcomings with the PM equation, such as the linearization of the saturation vapour pressure slope, the assumption of identical aerodynamic resistances for sensible heat and water vapour, and the assumption that the land surface can be treated as a single plane (i.e., bigleaf). We agree that these shortcomings should be acknowledged in the manuscript, and thus we will explicitly describe them in the updated version as limitations of the proposed PM_{rh} model.

4. Major comment 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.

Thank you for pointing out this important issue. Under local advection of sensible heat (i.e., horizontal sensible heat advection from an adjacent dry field), eddy covariance (EC) observations no longer represent net fluxes from a control volume (Leuning, 2004). In these conditions, the energy balance equation ($LE + H = R_n - G$) for the control volume may be rewritten as $LE + H = R_n - G + Q_{adv}$ (de Bruin et al., 2016), where Q_{adv} represents the sensible heat horizontally advected.

In our sugarcane site in Costa Rica, however, there is no evidence that local advection plays an important role. The flux observation plot was surrounded by similar sugarcane agriculture which also utilizes irrigation in dry season (Figure R1). Around this homogenous landscape, effects of local advection can be marginal (Leuning et al., 2012). Also, daily mean H was rarely negative in dry season regardless of irrigation application (Figure R2). Negative values of H are used as an indicator of local advection (Kutikoff et al., 2019). Therefore, local advection of sensible heat may not be significant in our study site. We will add this information to the supplementary material to support our findings in the next version.

Further, even if local advection plays a non-negligible role, our decomposition approach (i.e., LE into LE_Q and LE_G) is not affected by Q_{adv} in principle. Since we defined available energy (Q) as $LE + H$ instead of $R_n - G$ in calculating rh_s (L178), the decomposition approach is simply determined by LE and H using the EC technique, regardless of whether $LE + H = R_n - G + Q_{adv}$ or $LE + H = R_n - G$. Therefore, even under local advection conditions, the decomposition approach is robust. However, one of the assumptions of the PM_{rh} model, identical eddy diffusivities for water vapour and sensible heat, can be problematic under strong local advection conditions (Lee et al., 2004). This limitation will be discussed as a shortcoming of our approach in the next version.

If Q_{adv} plays a non-negligible role, it should increase LE_G term in principle as the Reviewer also argued in the minor comments (see 12. Minor comment 7, 25. Minor comment 20, 27. Minor comment 22). This is because local advection of sensible heat is typically accompanied by negative H (de Bruin and Trigo, 2019), which implies a large LE_G value. We agree with these points and the influence of advection on LE_G term will be discussed in the revised manuscript.

Figure R1 Costa Rica sugarcane site satellite view (retrieved from Google Earth)

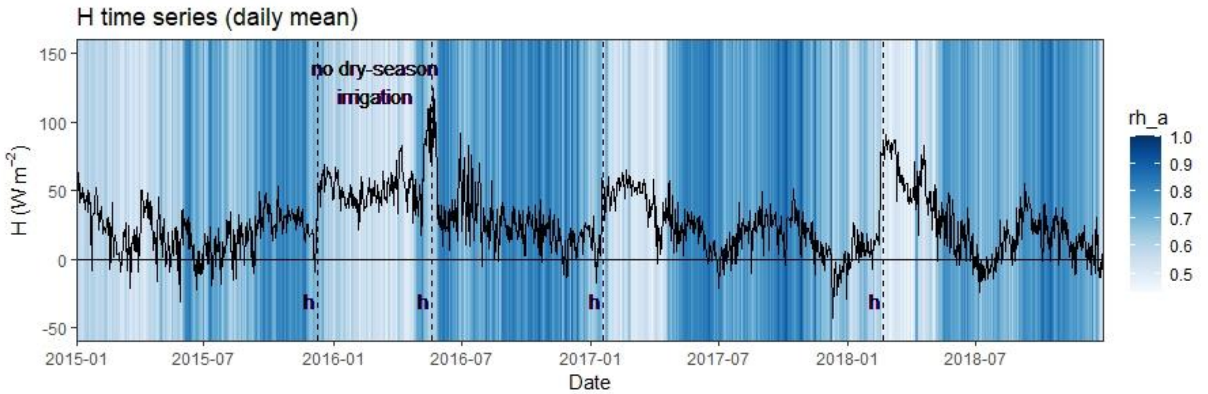

Figure R2 Time series of daily mean H for the sugarcane EC tower site in Costa Rica. Background color is atmospheric relative humidity (rh_a), and thus low rh_a implies dry season. Dashed line with "h" indicates sugarcane harvest.

5. Major comment 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.

Thank you for pointing this out. We agree that the well-known surface energy balance closure problem of eddy covariance (EC) observations ($R_n - G > H+LE$) can lead to systematic uncertainty in our analysis, which we also discussed in our response to Reviewer 1. For instance, rh_s can be underestimated if we use $H = R_n - G - LE$ instead of using EC-observed H in equation (8) (L190). We understand your concern, but we still believe our approach is an appropriate way to deal with this issue. We will add an in-depth discussion regarding the energy balance closure problem based on the below paragraphs.

As described in L172-179, we hypothesize that most of the energy imbalance in our site may be contributed by unmeasured canopy and soil heat storages in the soil layer between the soil heat flux plates and the surface. Although we cannot exactly quantify the storage terms, we have clear reasoning regarding the role of heat storages in our energy imbalance. First, it is expected that unmeasured canopy and soil heat storages in this site are significant since the sugarcane canopy grew up to 3.6 m tall with a dense canopy. Indeed, when the canopy height was less than 1 m, the surface energy balance was almost closed (97%), whereas the closure was 83% when canopy height was higher than 1 m (see L174~175). This result supports our reasoning. Also, it is widely accepted that the influence of secondary circulations on the energy balance closure is small for a homogenous landscape (Mauder et al., 2020;Stoy et al., 2013;Leuning et al., 2012). Since our site is located within a homogenous landscape (Figure R1), the influence of secondary circulation on the energy balance closure may be negligible. Even if the lack of energy balance is due to an underestimation of $LE + H$, there is no consensus on a universally appropriate method to correct LE and H (Mauder et al., 2020). Therefore, we did not enforce energy balance closure for the Costa Rica site.

Wehr and Saleska (2021) recently demonstrated that regardless of whether the lack of energy balance closure of EC observations is due to $LE + H$ or $R_n - G$, applying the flux gradient equation to observed LE and H without energy balance correction is the best way in determining surface resistance (conductance). This is because applying the flux gradient equation to the observed LE and H can dispense with the unnecessary assumption of energy balance closure (i.e., $LE + H = R_n - G$). They showed that bias introduced by underestimated *LE* and *H* is smaller than the bias introduced by the energy balance closure assumption. This finding may be applied to our analysis in calculating rh_s instead of surface conductance. For instance, as described above (4. Major comment 3), it is better to use observed *LE* and *H* to calculate rh_s under local advection conditions since "$LE + H = R_n – G$" does not hold under advection conditions. This is another reason why we imposed *Q* as *H*+*LE* and do not enforce energy balance.

As for the FLUXNET dataset, we provided analysis using energy balance corrected *LE* and *H* (Bowen ratio preserving method in Pastorello et al. (2020)) in the supplement. We found that the results for corrected and uncorrected versions were almost identical. This is a natural consequence. In equation (8), *LE* and *H* are included in the numerator and denominator respectively. Multiplying the same ratio to *LE* and *H* in equation (8) to correct *LE* and *H* based on the Bowen ratio method does not significantly change the resulting rh_s. Therefore, the lack of surface energy balance closure does not significantly impact our analyses and interpretations unless the lack of energy balance is dominated by *LE* only or *H* only. If the lack of energy balance is dominated by *LE* only or *H* only, our results and interpretation may include systematic bias, and it is a shortcoming of this research. <u>We will discuss this issue in the next version.</u>

6. Minor comment 1

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.

We agree that relative humidity is a physical quantity that depends on both moisture and heat of air. However, the main finding of this study and implication of the proposed model is that vertical relative humidity gradients constrain the latent heat flux (and thus evapotranspiration). This has not been recognized previously, but we feel that we have demonstrated this point in the present study.

7. Minor comment 2

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.

In this sentence, we intended to highlight that spatiotemporal variability in land surface dryness makes it difficult to predict actual *LE*. <u>Following your opinion, we will revise this sentence to be more specific.</u>

8. Minor comment 3

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

Thank you for your suggestion. <u>We will acknowledge Schmidt (1915) in the next version.</u>

9. Minor comment 4

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.

Thank you for pointing out the erroneous term. <u>We will revise it in the next version. ("rh budget" → "rh changes with time")</u>

10. Minor comment 5

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).

We agree that PM equation is semi-empirical although some of its physics is correct. We will revise this sentence. ("understand the governing physics of" → "express")

11. Minor comment 6

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?

The full derivation of the proposed PM_{rh} model is in the appendix, and equations (2) and (3) in introduction are results of the derivation. There is no derivation in the introduction. We will clarify the paragraph (L69-83).

The surface relative humidity (rh_s) represents a physical quantity, relative humidity (i.e., vapour pressure divided by saturation vapour pressure) at the land surface. Here, the land surface is defined as a single plane located at $d+z_{0h}$ (d=displacement height, z_{0h} = roughness length for heat) following the bigleaf framework of micrometeorology (Knauer et al., 2018). We will add the definitions of the relative humidity and the land surface explicitly in the theory section.

The proposed PM_{rh} model (equations (2) and (3)) conceptually bypasses the need to characterize surface resistance in order to describe water vapour transport and it is the key novelty of the model. The PM_{rh} model is not impacted by whether the water vapour flux originates from the soil or from inside stomata of vegetation. Instead, the model only describes exchange of water vapour and heat from the land surface to the atmosphere through a turbulent process, which is parameterized by r_a. The focus of the model is to decompose water vapour exchange into two thermodynamic processes following the notion of Monteith (1981). In this way, evaporation can be understood as a combination of diabatic and adiabatic processes.

12. Minor comment 7

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).

We agree that a large resulting value for the second term in the Penman equation can be an indication of advection. We will include the role of local advection of sensible heat in the next version. However, it is worth noting that even under the advection-free conditions, the second PM term is still required. de Bruin et al. (2016) expressed wet surface (irrigated grass) evaporation under advection-free conditions as $LE = \frac{s}{s+\gamma} Q + \beta$, where, s is the linearized slope of saturation vapour pressure versus temperature, γ is the psychrometric constant, Q is the available energy, and β is a constant correction. They introduced β due to unsaturated air inside the atmospheric boundary layer even under advection-free conditions. β should be understood as the second term of the Penman equation in our equations (4) and (5).

13. Minor comment 8

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

Please see our response above (2. Major Comment1); we do not believe this expression to be a misconception.

14. Minor comment 9

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

Please see our response above (11. Minor comment 6); the focus of the proposed PM_{rh} model is to decompose water vapour exchange into two thermodynamic processes following the notion of Monteith (1981). In this section of our manuscript, we interpreted the PM_{rh} model using the psychrometric chart. Interpretation of evaporation as two thermodynamic processes using the psychrometric chart is a widely accepted approach (Monteith, 1981;Monteith and Unsworth, 2013;Monson and Baldocchi, 2014).

15.Minor comment 10

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.

Thank you for pointing this issue out. The illustration using the psychrometric chart is based on work by Monteith (1981). This diagram describes the magnitude of turbulent flux (length of arrow) at a view point from a parcel of air located at a reference height. Since the parcel of air receives heat and water vapour from the land surface, the final state is represented by the surface condition while the initial state is represented by the atmospheric conditions at the reference height. It should be noted that this diagram does not indicate a partial derivative of atmospheric state with respect to time (i.e., $\frac{\partial}{\partial t}$). Rather, the difference between the initial and the final states should be understood as the magnitude of the turbulent heat fluxes instead of changes in atmospheric state. We will explicitly address this point in the revised manuscript.

16. Minor comment 11

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

The equilibrating process indicates the second term in equations (2) and (3). We will explicitly describe the process in the next version.

17. Minor comment 12

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?

We did not mention "lift" in the manuscript, and this concept is different from the "adiabatic lifting of air". Here, the adiabatic process indicates that there is no incoming energy into the system ($Q=LE+H=0$) (Monteith, 1981). We agree that this process is generated by the mixing of the air from the surface to the air to some level above the surface. The mixing driven by buoyancy is implicit in r_a as we mentioned above (2. Major Comment1). We will try to revise this phrasing to reduce confusion.

18. Minor comment 13

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.

Thank you for pointing out the erroneous term. We will revise it in the next version. ("rh budget" → "rh changes with time")

19. Minor comment 14

L151: "This is logical in that LEG 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.

We believe that the statement is not problematic. Please see our response above (2. Major Comment1).

20. Minor comment 15

L152/53: The classical equilibrium evaporation rate $LE = \frac{s}{s+\gamma} Q$ 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

Thank you for pointing out the original derivation by Schmidt (1915). We will refer the original derivation in the revised version of the manuscript. The thermodynamic equilibrium (or chemical equilibrium) between open water surface and the air in contact with the surface is equivalent to the saturation of the air (i.e., $rh_s = 1$). If the air at a reference height is not saturated under this condition, the second term of the Penman equation is required (equations (4) and (5)), and thus $\frac{s}{s+\gamma} Q$ alone cannot represent actual evaporation. de Bruin et al. (2016) also introduced the correction term β due to unsaturated air (see 12. Minor comment 7). Therefore, our interpretation in lines 152/3 is consistent with the derivations we present.

21. Minor comment 16

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.

We believe the wavelet results provide useful information that support our interpretations. The primary purpose of this research is decomposing LE into LE_Q and LE_G terms and identifying spatiotemporal variabilities of the two terms which yield behaviour of LE. The wavelet result shows how the variance of LE is explained by LE_Q and LE_G terms in different time scales over specific period. For instance, the strong positive correlation between LE and LE_G in the longer time period in Figure 2 (d) demonstrates that LE_G variability plays a non-negligible role in seasonal and interannual behaviour of LE. This is an unexpected result since the theory presented by McColl et al. (2019) implies zero seasonal variability of LE_G. We will try to better justify the use of wavelet and better interpret the results.

22. Minor comment 17

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?

We agree that sub-daily scale variabilities are important in this analysis. This is the reason why we highlighted Figure 3. Nevertheless, applying our decomposition method to daily averaged variables still provides useful information in that it can reveal seasonal and interannual variability of LE. As mentioned in the above response (21. Minor comment 16), seasonal variability of LE_G plays an important role in seasonal and interannual behaviour of LE which should be investigated further in different regions in the world. Decomposing LE into LE_G and LE_Q at daily time scale and identifying spatiotemporal variability is important to validate and extend the theory presented by McColl et al. (2019).

23. Minor comment 18

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

Please see our response above (22. Minor Comment 17).

24. Minor comment 19

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

Please see our response above (21. Minor Comment 16).

25. Minor comment 20
L240: That LEG 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.

Thank you for your insightful comment. We agree that LE_G term can be related to local advection of sensible heat. Please see our response above (4. Major comment 3). We will discuss this interpretation in the next version.

26. Minor comment 21
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.

We will add more information on the site in the next version of the manuscript or supplementary material.

27. Minor comment 22
L256: Does the case shown in Figure 3c represent a case with irrigation? Then, I guess, LEG relates to advection effects?

Thank you for your comment. Figure 3(c) does not represent a case with irrigation (pre-harvest period). However, we still agree that a positive LE_G term can be related to the local advection of sensible heat. Please see our response above (4. Major comment 3). We will discuss this interpretation in the next version.

28. Minor comment 23
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.

We will add those variables in the next version of the manuscript or supplementary material.

29. Minor comment 24 and 25
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 $rh_s \approx rh_a$ 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.

Please see our response above (2. Major Comment 1).

30. Minor comment 26 and 27
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.

We will revise the discussion and conclusion section, and the role of buoyancy will be acknowledged. Please see also our response above to (2. Major Comment 1).

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