Response to anonymous referee #1

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

March 14, 2021
Dear Anonymous Referee #1,

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

We were 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 that we intend to perform are indicated by underlined text. In this response, we reconstructed your comments based on the last paragraph's suggestions while covering all of the comments.

On behalf of all authors,

With regards,

Yeonuk Kim,

Corresponding author

1. General comment

This interesting paper explores a formulation of ET fully independent of the surface resistance, relying instead on a humidity resistance depending on the gradient of moisture between the surface and screen level, following the ideas displayed in Monteith (1981). Conceptually this approach allows to skip any explicit dependency of the characteristics of the surface, particularly those of the vegetation. The approach is worth exploring and this manuscript intends to show us to what point it can be used, especially for interpretation of the processes in place, more than in a parameterisation mode.

In a set of recent papers McColl and colleagues showed that, for scales of a day or longer, ET is essentially determined by the balance between surface moistening and surface heating, with the consequence at these temporal scales that more atmospheric moisture is the result of more ET, and the term describing it is equivalent to the diabatic term in the Penman-Monteith equation, therefore independent of the characteristics of the surface.

In this manuscript, the authors place themselves in this framework and try to extend it to sub-daily scale by using the humidity resistance in the adiabatic term, avoiding to prescribe any characteristic of the surface (vegetated or not). Their theoretical reasoning is easy to follow, substituting the pressure vapour by the relative humidity, and it is intended to be valid even for non-saturated surfaces through the prescription of a "surface relative humidity", rh_s.

We thank Reviewer #1 for your interest and constructive feedback on the manuscript. Your summary is consistent with what we wish to communicate regarding our study.

2. Specific suggestion 1

Suggestions: clarify somehow the aims of their research in the initial parts of the paper, especially in the abstract, when one may get the impression that a new ET parameterization is presented, while in reality what we have is a nice method of analysis of the diabatic and adiabatic components of ET;

Critique: The proposed method cannot be used as an independent way to determine *LE*, because *LE* is used to determine rh_s, so obtaining again *LE* from this rh_s value would be of no use (unless I miss something). However the method is useful to separate the observed *LE* into its diabatic and adiabatic parts (taking into account that $Q=H+LE$ is a tough assumption), and this is where most of the interpretation effort is put on.

Thank you for your suggestion. We agree with your suggestion. We focused on diagnostic analyses instead of predictive analysis using the proposed PM_{rh} model in order to demonstrate the importance of partitioning *LE* into diabatic and adiabatic components to improve understanding of the surface energy balance. Therefore, we will follow your suggestion by revising the abstract to explicitly highlight the decomposition analysis.

In this paper, we applied the new PM_{rh} model for a diagnostic purpose (i.e., using the model to aid in interpretation of governing mechanisms of measured or predicted ET). As such, rh_s was determined by measured *LE* and *H* in this study. However, it should be noted that the proposed PM_{rh} model has the potential to be used to determine ET in principle if a model to determine rh_s is available, although it is beyond the scope of the study. For instance, a combination of surface temperature and soil moisture (remotely sensed or field measurement) can be used to predict rh_s (e.g., Hajji et al., 2018). We will describe this point in the discussion section to suggest potential applications of the proposed model in the future.

3. Specific suggestion 2

Suggestions: elaborate on the meaning of rh_s;

Critique: The authors do not explicitly comment or define rh_s, which is still the missing piece in most of the approaches dealing with non-saturated surfaces. In the present case, they go around the problem of defining or calculating rh_s by deriving a value for it by using observed *LE* and *H* values from EC-systems, using their formulation (2).

As described in the manuscript (L77), rh_s is the relative humidity at the land surface, and is a purely physical quantity (vapour pressure divided by saturation vapour pressure). Here, the land surface can be 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., 2018a). We noticed that the land surface was not explicitly defined in the preprint version, and thus we will add the definition of rh_s and the land surface explicitly in the theory section.

4. Specific suggestion 3

Suggestions: explain better what are the expected consequences of their hypotheses in the ulterior data analysis (such as imposing $Q=H+LE$ or the chosen form for r_a);

Critique: To do it they assume that the available energy *Q* is *LE+H* instead of R_n-*G* trying to circumvent the unavoidable problem of the closure of the surface energy budget. This decision could be understandable but it is poorly justified and the consequences of it are not reflected upon. Unfortunately the result of these strong hypotheses concerning rh_s and a discussion of the values obtained is not explicitly shown or made.

Thank you for pointing out this problem. We agree that the well-known surface energy balance closure problem for eddy covariance (EC) observations ($R_n - G > H+LE$) can cause systematic uncertainty in our analysis. For instance, rh_s can be underestimated if we use $H = R_n - G - LE$ in equation (8) instead of observed *H* using EC system (L190). We appreciate your concern and we will add discussion points regarding the energy balance closure problem and chosen form for aerodynamic resistance (r_a). A more thorough examination of this point is presented below in this response to Reviewer comments.

As described in L172-179, we determined that most of the energy imbalance in our site is contributed by unmeasured canopy and soil heat storages. Although we cannot exactly quantify these storage terms, we reason that this is the primary source of energy imbalance as follows: First, it is expected that unmeasured canopy and soil heat storages in this site are significant since the sugarcane canopy grows up to 3.6 m tall with a dense canopy. Indeed, when canopy height was less than 1 m, the surface energy balance was very nearly closed (97%), whereas the closure was 83 % when canopy height was higher than 1 m (see L174~175). This result supports our reasoning concerning canopy storage terms. 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 circulations on the energy balance closure may be negligible. Even if the lack of energy balance is due to 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 force energy balance closure for the Costa Rica site.

Wehr and Saleska (2021) recently demonstrated that regardless of whether the lack of energy balance of EC observation is due to *LE* + *H* or due to $R_n - G$, applying the flux gradient equation to the observed *LE* and *H* without applying an energy balance correction is the best approach to determining surface resistance (conductance). This is because applying the flux gradient equation to the observed *LE* and *H* dispenses 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, although in our case we are calculating rh_s instead of surface conductance. This is another reason why we imposed Q as $H+LE$ and do not enforce energy balance.

As for the FLUXNET dataset, we provided an analysis using energy balance corrected LE and H (Bowen ratio preserving method in Pastorello et al. (2020)) in the supplementary file; the results for corrected and uncorrected versions were almost identical. This is actually 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 by H only. If the lack of energy balance is dominated by LE only or by H only, our results and interpretation could include systematic bias, representing a shortcoming of our approach. We will discuss this issue in the revised version.

As for the chosen form of r_a, the influence of this choice is also expected to be marginal compared to the energy balance problem. Knauer et al. (2018b) showed that uncertainty caused by different r_a on surface conductance is low compared to the energy balance closure problem. This finding can be applied to our analysis. Specifically, in equation (8), r_a is multiplied by both denominator and numerator, and thus a small difference in r_a should not significantly affect the resulting rh_s.

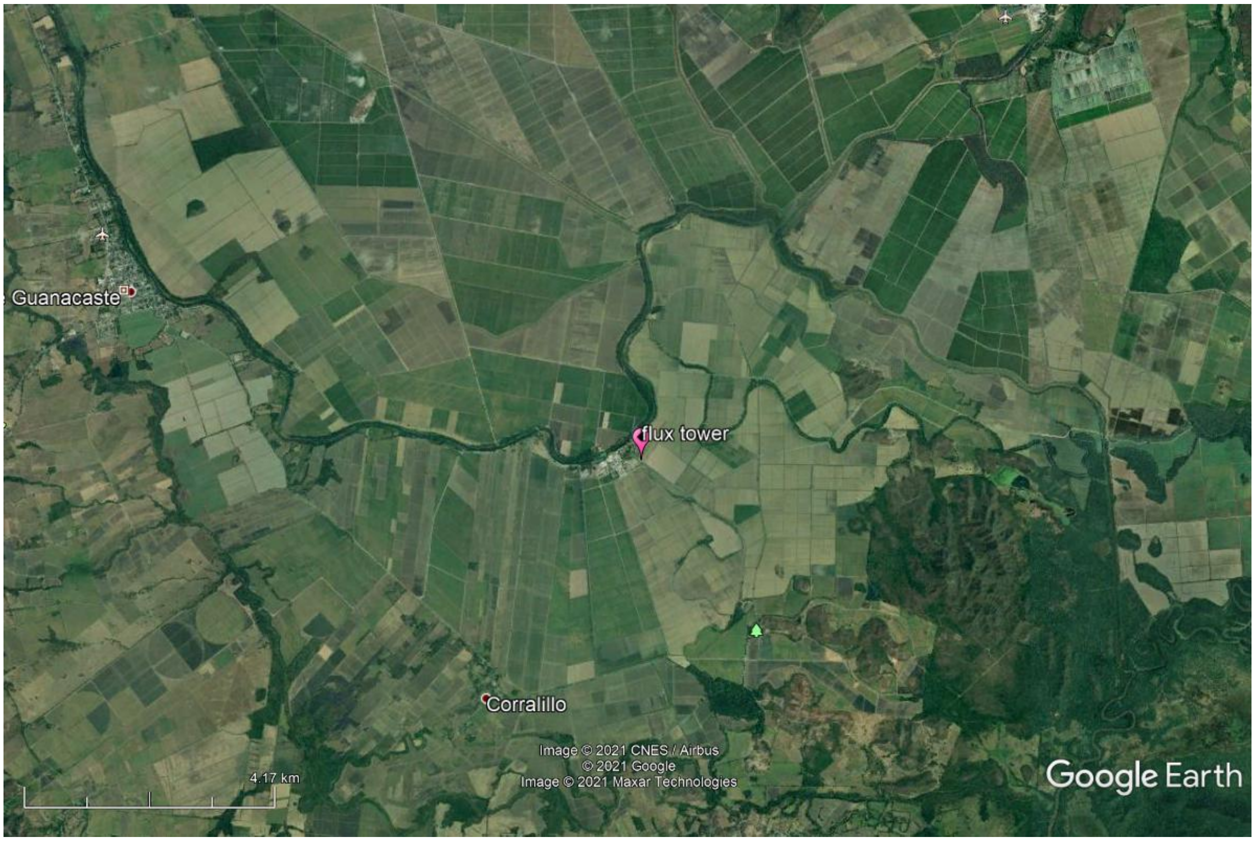

Figure R1 Costa Rica sugarcane site satellite view (retrieved from Google Earth)

5. Specific suggestion 4

Suggestions: expand they interpretation of data, currently very shallow, into a remade Discussion section;

Critique: Fig 4 contains a lot of information in its 8 sub-figures, which are not really commented. A similar comment can be made about Fig 5 and its 12 sub-figures. What is the use of displaying so much information if then it is not discussed? In general section 4 would need to be more developed in terms of interpretation of results, which is now very shallow, especially sections 4.2 and 4.3.

Thank you for your suggestion. In the revised version of the manuscript, we will provide subsections of the Discussion section similar to the current Results subsections, and expand our interpretation. Also, we will more clearly explain Figures 4 and 5 in the Results section.

Specifically, we will discuss why the spatiotemporal variability of evaporative fraction is explained by LE_G (adiabatic term) instead of LE_Q (diabatic term). Also, we will compare diurnal and seasonal variability of LE_Q and LE_G terms in Figure 3 and Figure 5 (e1)~(e4). The spatiotemporal variability of LE_G in Figure 5 will be interpreted further by considering possible connections between the LE_G term and precipitation.

6. Specific suggestion 5

Suggestions: joint the current "Discussion" and "Conclusions" sections into a more comprehensive and developed new "Conclusions" section;

Critique: As the paper is now sections 5 "Discussion" and 6 "Conclusions", both very short, could be merged into one larger Conclusions section. Instead a real "Discussion" section could come from an expanded version of the analysis of the results.

Thank you for your suggestion. We will follow your suggestion by rewriting the Discussion section as mentioned above and transferring a part of the current Discussion into the Conclusion.

7. Specific suggestion 6

Suggestions: consider to summarise the information in the supplementary material and incorporate it straight into the manuscript

We felt that providing detailed information on the field observations is valuable for this paper, and thus we prepared the supplementary material. The reason we structured the presentation as the main paper plus a supplementary file is that the manuscript already covers a lot of information (e.g., new theory, and results from three different datasets), and incorporating supplementary material directly into the manuscript may overwhelm some readers. We will endeavor to revise the manuscript as concisely as possible while including essential information by expanding the Results and Discussion sections.

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