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Discussions

Interactive comment on "Spatial variability of mean daily estimates of actual evaporation from remotely sensed imagery and surface reference data" *by* Robert N. Armstrong et al.

Anonymous Referee #1

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The novel approach to spatially scaling evaporative fluxes could be a significant contribution to HESS and quite valuable for some readers. It is very exciting to think of scaling point measurements to the widely available and affordable remote sensing data. However, the manuscript requires some restructuring for clarity and a much deeper discussion on potential implications and limitations. The model is clearly presented, and with some minor clarifications could be easily reproduced. Some figures could be combined or eliminated. The biggest weakness is that the discussion and conclusion lack a detailed assessment of the uncertainty in the model, including what regions it might have performed poorly in and more general speculation on its applicability outside the study area.

Specific comments:

Fundamentally the authors need to discuss the implications of assuming the ground heat flux is negligible while at the same time utilizing differences in surface temperature to derive variations in the upwelling longwave radiation (and thus available energy for evaporation). Perhaps this is a minor error, but that is unlikely given the gap in explained energy shown in Figure 13. If all that unaccounted for energy is partitioned into sensible heat it would require Bowen ratios of around $\sim$ 0.9, which is probably too high for a grassland. If the unaccounted for energy is turned into kinetic energy, it will contradict other assumptions in the model. This project would benefit from a discussion about the implications of these assumptions and speculation of their magnitude using the energy partitions observed at the EC system that day.

More information needs to be provided about the EC data used to validate the model. With only one day of measurement, it is critical that it is a top quality flux (or at least specify any issues clearly). At a minimum standard quality control metrics should be reported for the flux (e.g. a 0 flagged flux by the methods of Mauder and Foken 2004), and it should be clearly stated that the measurements were not made in the wake of the tower at any point during the day. The authors should report if any gap filling techniques were required to derive the daily flux and which methods were used to calculate the spectral corrections and density corrections. Furthermore, the 100 m EC fetch should be overlaid onto the map, or clearly outlined how it was defined in the text. Is it simply a linear transect, an ellipse, or derived from a footprint model? Since this is the primary validation, it should be clear what segment of modelled flux from the surface is being compared to the EC fluxes.

A discussion on the uncertainty is lacking. Are any of the crops C4 plants? Could that be a problem in other systems? How likely is it that wind speed and turbulent energy is consistent across all surface classes? Abrupt changes in surface, from smooth open water, to forest, to hill slopes, does not simply translate to a greater roughness (as stated on Page 8, Ln 4/5). Internal boundary layers and small pressure cells produce microclimates as a result of divergent or convergent flow at abrupt roughness and topographic changes. These microclimates could increase air flow through the canopy (thus reducing roughness height and surface resistance) or decrease flow within the canopy (increasing roughness height and surface resistance) and drastically change Ea, which will depend on the direction of the wind at the time. This may be trivial on the overall estimates presented in this paper, or it could be significant (depending on the surface heterogeneity and wind variability) but more evidence that the authors have considered this would help a reader understand where and when the model is applicable.

Figure 15 shows the only true variability in the model outside those directly measured at a point location (since Zo is classified from the same DN that defines the albedo). It seems appropriate to start the discussion with the variability in DN (or alpha) and Ts (or upwelling Longwave), not conclude with it since all other results can relate to this variability in surface parameters. Specifically, the authors should address how the variability in DN and surface temperature impact estimates for E.

On page 14, line 26, the covariance between Q* and G was stated to be very small. However, G is 100% dependent on Q*. Does this unorthodox computation of covariance appear greater when calculated within each roughness class? More importantly, what is the covariance between your remotely sensed variables (DN and Ts)? Could this be a problem if these are highly correlated?

Technical Corrections:

Reference the company that makes the software/toolbox used to partition the surface.

Some figures are barely discussed in the text, making the reader wonder how relevant all 17 figures are. For instance, Figure 13 was only discussed in one sentence and briefly referenced in another. Figure 12 is referenced once, then completely described in text such that the figure added no value.

Add jitter to the boxplots outliers so the density of outliers can be assessed.

You don't need both CV and SD since they provide redundant information when the mean is available. Since you have such high N, you technically don't need either because the interquartile range is adequately defined and it is a better descriptor of the variability when the data is skewed.

Remove the coefficient of variation of the aerodynamic component (page 12, Ln 9). It looks like the distribution is highly bimodal and you don't refer to it anywhere.

Vegetation types are not clearly defined. Stick to the same discerptions throughout, and be sure all are labelled on the map. "Tree rings" suggest a site used for dendrochronological analysis, did you mean a small corpse of trees surrounding the wetlands (a ring of trees)? If so this is not clear.

Numerous typos were found, please edit for clarity. This is not a comprehensive list, but these jumped out while reading: Pg. 15 Ln 11, Pg. 13 Ln 8, Pg. 7 Ln 15, Pg. 6 Ln 19, and Equation 5.

The paragraph starting on line 20 of page 4 is difficult to follow. Are there three radiation validation sites, or two? I had to check the results to understand your methods. Please clarify.

Don't describe the visual difference, simply state if differences are significant or not (e.g. Page 12 Line 34/35). If it is not normal, just use a non-parametric test (something simple like Wilcoxon).

A roughness length of 0.4 m seems short for a dense stand of 10 m tall trees; please justify.

Title of section 3.4 is misleading. Is this exploratory analysis or an analysis of the variability within the model?

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