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Interactive comment on "Components of near-surface energy balance derived from satellite soundings – Part 2: Latent heat flux" *by* K. Mallick et al.

Anonymous Referee #1

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General

The paper explores a novel approach for estimating ET, or more specifically, several parameters required to model ET using satellite imagery, from remote sensing data. While the approach has limitations, which are acknowledged by the authors, it has the potential to be a useful tool for enhancing ET estimates over large scales by providing observation-based estimates of the parameters needed for ET modelling. Although the paper presents the initial evaluations of the approach and additional studies are needed to refine and confirm the utility of the methods presented, the potential benefits of the technique to the modelling community are sufficient to merit publication. Nonetheless, there are several aspects of the study that need clarification of further explanation (see

below); the authors are also urged to work with a grammarian to improve the English syntax and structure of the paper.

Specific

1. P2, L42: The "scales relevant to decision making" should be defined more rigorously. What are these scales?

2. P4, L89: These "counter arguments" need to be explained (justified) more fully. For example, the Bowen ratio is determined in terms of gradients in Eqs. 5 and 6. It is not clear how well measurements of temperature integrated over a volume of the atmosphere will represent the true gradient, particularly if these quantities vary nonlinearly with height? Also, relationships the authors present are based on assumptions of similarity (e.g. the eddy diffusivity terms are not shown in Eq. 5) given the course horizontal and vertical spatial scales of the soundings data, the authors need to justify the assumption that the measurements of the turbulent transport of heat and water are within the surface boundary layer and it conforms to similarity theory.

3. P6, L129: The authors use the 1000 mb and 925mb levels to estimate the temperature and pressure gradients. Since especially the latter is unlikely to be within the constant flux layer, it is questionable that the underlying assumptions of similarity, etc. are valid. Or, why they should be expected to be valid? While the authors point to studies such as Swinbank and Dyer (1967), those analysis are based on the assumption that the sources and sinks for heat and moisture identical and uniformly distributed. The large spatial scales corresponding the sounding measurements place that assumption in doubt.

4. P8, L187: With the possible exception of tall tower data, the source area of EC flux measurements is typically much less than 10 km2 so the mismatch in the scale of the source areas between the tower and soundings data likely exceeds three orders of magnitude. The smaller source area of EC systems would tend to mask the impacts of spatial heterogeneity that would be seen at the coarser resolution of satellite data.

5. P10, L219: A correlation of 0.34 may be statistically significant, but it suggests only a modest relationship between the satellite and tower-derived estimates of evaporative fraction. Moreover, from Fig. 2 it appears that the maximum tower measurements of latent heat flux are about 350 Wm-2; in that case, an error of 79 Wm-2 would be an error of 20% to 25%.

Technical

1. P2, L36: It's less managing climate change than its effects.

2. P3, L53: The sentence beginning here is awkward and confusion. It needs to be rewritten.

3. P5: The authors use a number of non-standard symbols (e.g. using phi to represent available energy and P to represent water vapour pressure). Using more typical symbology would avoid confusion for the reader. Also, the authors need to confirm that all of the symbols are consistent. For example, the authors inter-mingle "z" and "Z" to represent height.

4. P6, L121: The word "plain" is misspelled.

Interactive comment on Biogeosciences Discuss., 11, 8085, 2014.