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Discussions

Interactive comment on "Evaluation of an extreme-condition-inverse calibration remote sensing model for mapping energy balance fluxes in arid riparian areas" *by* S.-H. Hong et al.

Anonymous Referee #3

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This paper presents a study of using the SEBAL to estimate daily energy balance components in arid riparian areas based on satellite (Landsat) imagery. Authors evaluated the model performance by comparing the model outputs to the ground-based measurements at three sites in the southwestern U.S. In general, the methodology (including problem setup, modeling, and data analyses) appears to be appropriate and the study could potentially be valuable addition to the hydrologic studies over arid/semi-arid riparian regions.

Although my overall opinion of the paper is positive, the paper in its present form is not publishable in the HESS and requires substantial revisions. Listed below are my major

concerns as well as suggestions for the authors to consider.

1. My major concern is whether the application of the SEBAL approach could be extended to other or more general arid riparian regions. In the Introduction, authors state that the study areas are very "heterogeneous" and if the approach works "under these challenging conditions, it is likely to perform well in most arid and semi-arid regions." In the manuscript, however, I have read many explanations and discussion on why certain bias/errors occurred in this study associated with the three selected areas and what specific techniques were used. One of the examples is that, in Lines 19-23 (Page 13499), "these differences area about two to three times larger than those typically reported for SEBAL. The much larger than usual MRD is attributed to the heterogeneity of the riparian sites..." If the "heterogeneity" is the cause, how could readers believe that the approach would perform well in other heterogeneous riparian areas?

2. I am a modeler, but not a SEBAL user. I am curious about the acceptable level of accuracy for SEBAL estimates. According to the results (e.g., Figures 4 and 6), the deviations (from 1:1) are obvious and R^2 values are not very impressive, ranging from 0.56 to 0.75 (Table 4) and from 0.32 to 0.78 (Table 8).

3. I understand that the selection of "hot" and "cold" pixels is very essential to the success of SEBAL application. But how sensitive is this procedure to the quality of ET estimation? In other words, if a less accurate or a bad selection of "hot" pixel is made, how would it impair the model performance in estimating the ET? It is not clear according to the manuscript and some quantitative analysis in this respect would be interesting.

4. The manuscript is too long, containing too many details that are not necessary. Pages 13485-13487: if the descriptions of the method are not new to the SEBAL users, many contents of these pages could be removed. Sections 3.2, 3.3, and 3.5: have too many details and could definitively be shortened.

5. Locations of the study areas Please provide map(s) showing the locations of the

three areas. It is very important to include information such as where/how large the study areas are and where the field-based measurements were made.

6. The manuscript needs careful editing, e.g.: "has" (Line 9, Page 13482) "Peters Lidard et al., 2004" (Line 14, Page 13482) "exists" (Lines 11-12, Page 13482) "otherssuggest" (Line 13, Page 13487) "Since," (Line 20, 13488)

7. "Table 5" (Line 2, 13491): mentioned/used in the context before Tables 3 and 4. Please re-arrange tables in sequence.

8. Figures 4, 6, 8, 9, 11, 12 and 13: The fonts and symbols of these figures are too small.

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