

Interactive comment on “Estimate soil moisture using trapezoidal relationship between remotely sensed land surface temperature and vegetation index” by W. Wang et al.

Anonymous Referee #3

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General Comment: The paper describes the estimation of soil moisture using the trapezoidal method and a numerical, energy balance-based assessment of the vertices of the trapezoid. The paper is sufficiently original to warrant publication in HESS. The paper is fairly well written, but it would improve the readability of the paper even more if greater attention was provided to improving language fluency. Before the paper can be considered finalised, I would like the authors to address the following issues. 1. The method's dependence on ground-based data is one significant limitation of the method, restricting its use in areas where ground-based meteorological data (i.e., air temperature, wind speed, relative humidity) are available. This method could not be

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used with any level of confidence in areas with poor to no data coverage, particularly in remote and mountainous regions. The authors should try to point out some of the limitations of the method, as well as ways to get around these limitations, if feasible. 2. Like in the expression of ra [in Eq. (10); p. 4], u^* also can be written as a function of atmospheric stability; however, the authors define u^* strictly as a function of neutral stability. What are the implications in introducing inconsistencies in model formulation? The authors need to provide justification for doing this. 3. Using three stations to derive air temperature accurately is questionable; could the authors comment on this issue. 4. $\beta=0.0065\text{C m}^{-1}$ represents a long term average; what are the implications in using this long term average in short-term calculations of soil moisture? 5. The authors should consider doing a sensitivity analysis to determine the impact small variations in some of the parameters (that have been set constant) have in the final calculation of soil moisture. It is always important to know which parameters exert the greatest influence on the final calculations. Technical Comment: In revising the manuscript, the authors should pay particular attention to making sure that all references to equations in the text or in the Figures are correct. Figure 2 provides many instances where equations being referenced are incorrect; e.g., Eq. (14) in the 6th box of the flowchart should be Eq. (12) and Eq. (15) (same box) should be Eq. (13). The authors should make sure that all parts of the manuscript are consistent with every other part of the paper. Editorial Corrections: 1. Acronym for WDI in the Abstract should be defined 2. Move “well” in “...trapezoid based WDI can well capture temporal variation in surface soil moisture, ...” (in the Abstract) to the end, to give “...trapezoid based WDI can capture temporal variation in surface soil moisture well,...” 3. Replace “a lot of” in Introduction to “many” 4. Remove “The ‘triangle’ method fits the scatter-plot...using a triangle”; redundant with previous sentence and, therefore, can be removed with minimal impact 5. “scatter in a close range”; you mean “cluster”? 6. “are still hard to be well established,” can be re-written as “are still hard to establish” 7. Many errors appear in Figure 2, especially concerning referencing of equations; consider revising. 8. P. 7; “near critical depth”; what is meant by that? 9. Table 1; use same font size throughout 10. P. 8; simplify

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“ The locations of the meteorological observation sites, 16 soil moisture observation sites and 87 rain gauging sites are plotted in Fig. 4” to “Locations of these sites are plotted in Fig. 4”; there is no need to repeat the number of sites of each kind addressed in a previous sentence. 11. P. 9; “Denosing” in Section 4.2 subheading should be “De-noising” 12. P. 10; Eq. (X)? Missing equation number? 13. Material on P. 10 seems out of place (after the description of the study area); shouldn’t this material be part of a single Methods section?

Final Comment: It is unfortunate that the authors base the validation of their method on data from a semi-arid area. Their choice of data does not permit for a robust evaluation of the method, especially with respect to the spatial dimension. The authors should consider conducting additional validation (maybe at a later time) for areas with greater spatial variability.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 7, 8703, 2010.

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