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Interactive comment on "Regional evapotranspiration from image-based implementation of the Surface Temperature Initiated Closure (STIC1.2) model and its validation across an aridity gradient in the conterminous United States" *by* Nishan Bhattarai et al.

Anonymous Referee #2

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This manuscript provides an evaluation of STIC1.2 in estimating actual evapotranspiration at the spatial scale by combining the model with satellite remote sensing data. In addition, the authors also compare the performance of STIC1.2 with other two existing remote sensing algorithm (i.e., SEBS and MOD16). In general, the topic of this MS is of interest to the HESS' readership and the manuscript is well written. However, there are several major issues in this study, which introduce additional uncertainties and preclude a focused evaluation of the models themselves (as described below). In

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this light, a MAJOR REVISION is needed. Major: 1. My largest criticism lies in the use of MOD11A2, where LST is reported as the average values of clear-sky LSTs during an 8-day period. As there is no information about which day (or days) out of each 8-days contributes to the final 8-day averages, this 8-day average LST is highly likely to not correspond to the 8-day averages of meteorological variables (i.e., air temperature, VPD, etc). For example, the 8-day LST might only be a result of day-1 LST, or the average of day-1 and day-7 LSTs. Even they correspond well with each other, using 8-day averages may still lead to additional uncertainties due to differences in the temporal variability between, say, daily LST and air temperature. For example, H_day1+H_day2...+H_day8 does not equal to 1/8 * (H_calculated using 8-day average LST and meteorological inputs), as all the responses are non-linear. To focus on evaluating the model itself, it is recommended to work on the instantaneous scale rather than 8-day averages. 2. Page11 (Line 8-17) Again, validation should be carried out at an instantaneous scale but not daily (or 8-day averages), as upscaling can introduce additional uncertainties. In my opinion, upscaling from satellite overpass to longer time scales is another scientific question. 3. Page11 (L29-30): Any explanation of this model performance: overestimation in dry year and underestimation in wet years? Additionally, according to your Figure 6, it seems that this "overestimation in dry and underestimation in wet" pattern persists across sites (i.e., spatially). This may suggest some systematic uncertainty of the model. Given this, I do not agree with the statement given in Page 15 (Line 4-12). First, does any previous study support this wet/dry patches around the studied sites? If not, this is just your speculation. Second, the footprint issue could lead to random errors rather than a systematic underestimation. Finally, it is the authors' responsibility to ensure the footprint of a flux site corresponds (or encompasses) the MODIS footprint so that to eliminate data uncertainties and to allow a focused evaluation of the model. Minor: 4. Page7(L7): Delete "the" between "both" and "variables"; 5. Page9(L20): Please specify the equation for NDVI and/or provide references. 6. Page 16 (Discussion on MOD16): It is worthwhile reading Yang et al. (2016, WRR; doi: 10.1002/2014WR015619) for a more physical explanation on

the MOD16 uncertainty.

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