

Interactive comment on “Evapotranspiration monitoring based on thermal infrared data over agricultural landscapes: comparison of a simple energy budget model and a SVAT model” by Guillaume Bigeard et al.

Anonymous Referee #1

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Overall Comments: The authors present a study that contrasts two distinct approaches to utilizing thermal infrared (TIR) remote sensing for estimating surface evapotranspiration: a surface energy balance model that is forced directly by TIR data, and a more complex SVAT model that assimilates TIR observations.

I believe this is an interesting topic to address, but I do not feel the authors have performed a satisfying comparison of these two methods. One source of major concern is the explanation of what exactly was done. For example, it remains unclear to me how TIR data is “assimilated” into the SVAT model. This needs to be very clearly described.

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Likewise, the use of TIR for SEB appears to be through optimizing a few parameters, but this likewise remains unclear to me. It is also unclear exactly what years / seasons / crops were evaluated at each of the two sites (France and Morocco). At one point in the manuscript they mention two available meteorological stations, one in France and one in Morocco, which indicates that the data may be from multiple years. Was each crop evaluated for a single growing season at each site, multiple years for each site? The lack of clarity and detail about the methods used, particularly how TIR data is used to constrain, or for data assimilation, in each model makes it difficult to evaluate the results effectively.

The paper initially appears to be focused on the evaluation of two different types of models, surface energy balance versus a full SVAT model, for estimating evapotranspiration from thermal infrared remote sensing. At some point it transitions into a sensitivity analysis paper, which does not tie back to the original point as far as I could tell. A major revision should seek to bring out the use of TIR data in these two model frameworks, without a heavy focus on broad sensitivity analysis of the two models.

The English phrasing in the manuscript could generally be improved for greater clarity and to reduce confusion in some of the explanations of methods and results. Likewise, the use of a spell checker will catch a few spelling errors that exist in the reviewed manuscript.

Specific Comments:

1) Lines 10-11: The following statement in the Abstract should be re-phrased for clarity. “TSEB has been shown to be more flexible and requires one single set of parameters but lead to low performances on rising vegetation and stressed conditions. “ It is not clear to me what “low performances on rising vegetation” means.

2) Lines 14-17: The final couple of sentences in the Abstract are confusing and should be re-written for clarity.

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- 3) Section 2.2: While citations are provided for complete descriptions of each site, the first paragraph should include information on the explicit contrasts or similarities of the two sites, such as: where all three agricultural species grown at each of the two sites? How is irrigation managed / used at each site, and for each crop? What are the mean climate variables during the growing season such as temperature, VPD, precipitation?
- 4) Section 2.2: Clarify for what years / seasons each crop / site was monitored with meteorological instruments and eddy covariance. This should be clarified in the second paragraph of 2.2.
- 5) Section 2.3: Rather than using a few 10-day periods, why not use the full growing season records for each crop / site to more fully evaluate the capabilities of each model. I would think that the assimilation of TIR data into the SVAT would have a payoff that increases over time, as erroneous parameter values are further corrected / improved with each assimilation cycle.
- 6) Section 2.4: The authors mention that a “multi-objective calibration method” is used, with “five target functions”. Please clarify what this means. What are the functions: a set of objective functions that each minimize the difference between a variable and the observed quantity? Or, are multiple objectives used here. The objective functions, and exactly what variables they pertain to, needs to be clarified.
- 7) Figure 2: The axis values for MAPD confuse me. In both cases they start at 43, decrease rapidly to 23, and then increase again to 53. I would expect monotonically increasing axis values. . .
- 8) Section 3.1: The authors previously state that water stress periods are primarily confined to the senescence phase, but here point out that the changes in canopy radiation transfer, pigment contents, etc, are not taken into account by TSEB. This goes to my earlier point that the entire set of growing seasons should be simulated and evaluated with both models, not just a few 10-day periods. Stress is likely to be found at both sites, either between irrigation events or rain events.

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- 9) Figure 3: In the legend describe the difference between the top and bottom panels.
- 10) Section 3.2.3: It is hard to believe that the SVAT model is only sensitive to wind speed for LE computation, and not other meteorological inputs such as radiation forcing, or VPD. How do the authors explain this?
- 11) Figure 6: The relatively minor impact of biases in T_s (i.e. thermal infrared temperature measurements), relative to the reference RMSE, indicates that TSEB is not very sensitive to TIR inputs. Doesn't this contradict the premise that this is one of two models that can be used for ET monitoring from TIR data?
- 12) It would be very nice to see a figure identical to Figure 6, but for the SVAT model.
- 13) Section 3.26: At the end of this section the authors appear to argue that their two well-watered sites that do not apparently see significant water stress during the growing season may not be best suited to an experiment such as this, focused on evaluating two TIR-based ET approaches. I would tend to agree that at least an additional site that experiences significant periods of water stress throughout the growing season is merited.
- 14) Section 3.2.8: The authors state that the parameter V_{max0} has a different value at every time period for each crop. I don't understand what this means exactly. Is this parameter varied in the assimilation procedure, and it shows large variability from time period to time period? This data should be shown, even if in Supplementary.

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