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Hydrology and Earth System Sciences
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Discussions

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.

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We first wish to thank the reviewer for his useful comments and corrections that we have, for most of them, taken into account. Significant rewriting has been necessary. The point by point responses are detailed below. We believe that the article has been considerably improved.

Note: two versions of the manuscript are provided, one with corrections and rewriting in response to reviewers highlighted in green, another one without colors.

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Discussion paper

Overall Comments:

[1] 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. Likewise, the use of TIR for SEB appears to be through optimizing a few parameters, but this likewise remains unclear to me.

We agree completely concerning the TIR data. We aimed to place this specific study within a larger context where TIR data are intended to be used to constraint the SVAT model trajectories through data assimilation. Within this study, data assimilation of TIR data within the SVAT model has not been implemented as we believe that a preliminary analysis of model calibration and sensitivity study to input errors is necessary. To this objective, a Multiobjective Calibration Iterative Procedure (MCIP) has been implemented to tune its parameters in the view of using TIR data. As this will be the subject of further work, we don’t mention the use of TIR data into the SVAT model anymore in the abstract and in several places of the new version of the manuscript, following the referee comments.

We have modified the title of the paper to clarify the purpose of our study.

[2] 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?

Concerning meteorological data, two stations are used, one in France and one in Morocco. Concerning micro-meteorological data (including latent and sensible heat fluxes), 3 fields have been instrumented: two in France (Auradé and Lamasquère sites) and one in Morocco (Sidi Rahal site). During the 3 years of study in France, crop rotations allowed us to gather data on wheat (3 seasons), maize (2 seasons) and sunflower (1 season). In Morocco, we used 1 crop season. We believe that table 1 clearly shows the number of growth season for each crop but we have also reformulated section 2.2 in the new version of the manuscript.

[3] 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.

Agree. Cf. point [1].

[4] 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.

We agree that the initial version of the manuscript was confusing. To our opinion, the different modeling frameworks of the two approaches, mainly the solving of a soil hydric budget for SETHYS and the use of surface temperature as an indirect proxy of the crop hydric conditions for TSEB, deserve a sensitivity analysis. The abstract has been rewritten (cf. point [1]) to point out that the paper is mainly dedicated to a sensitivity analysis of the two approaches based on a unique database. With regards to the use of TIR data, the cross sensitivity analyses of the models through the linkage of the radiative temperature and the SWC shows the different response of the models to the crop hydric conditions. These results can then be analyzed in the light of the models performance from the sensitivity analysis. Following the reviewer's comment and in order to match the objectives presented in the abstract and the content of the work, the abstract and the introduction have been reformulated. See also response to point [1].

[5] 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.

OK. The manuscript has been reviewed by a native english speaker.

Specific Comments:

1) Lines 10-11: The following statement in the Abstract should be rephrased 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.

Agree. In the new version of the manuscript, this sentence has been replaced by : “TSEB is run with only one set of parameters and provides acceptable performances for all crop stages apart from the early beginning of the growing season (LAI < 0.2 $m2.m{-2}$) and when water stress or senescence occurred.”

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

Agree. The corresponding sentences of the abstract have been rewritten.

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?

Cf point [2] of the overall comments.

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.

Cf point [2] of the overall comments.

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.

OK. TIR data is not assimilated into the SVAT model (cf point [1]). By contrast, sets of parameters representing specific phenological stages and hydric conditions (stressed/unstressed) are sought in the view of a future application of the SVAT model at the scale of a heterogeneous agricultural landscape. Consequently, the periods should be long enough to gather a sufficient amount of data (of good quality, meaning a good closure for the energy budget) and not too long so that the crop and hydric conditions don't change too much. 10-days has been shown in several studies to be a good tradeoff. Section 2.3 was reformulated in the new version of the manuscript to stress this aspect.

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.

OK. Five objective functions are optimized simultaneously. The five objective functions are detailed in section 2.4 (l.7, p.11). They are built to minimize the distance between model predictions and observations thanks to the Root Mean Square Error (RMSE). An ensemble of simulations based on a monte-carlo sampling of the parameter space is carried out. For each simulation corresponding to a specific parameter set, five objective functions are computed (RMSE of LE, H, Rn, Tb, W_rz). The joint optimization of these 5 objective functions is obtained following a Pareto ranking. Basically, a simulation is classified as “better” than the others if all the objective functions have lower values. For more details the MCIP methodology is described in Demarty et. al, 2004 and 2005.

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. . .

Agree. It seems that exportation to pdf went wrong, axis values have been corrected in the new version of the manuscript.

8) Section 3.1: The authors previously stated 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.

Thank you for your comment. We agree that this point deserves clarification. It appears that irrigation was properly scheduled for all our study sites and seasons. Consequently, stress didn’t occur during the growth phase of the crops. This has been carefully checked by looking at the whole time series (and not on 10-days period) of the measured root-zone soil moisture (through the SWI, cf. section 2.3) and the ratio between potential and real evapotranspiration (SE indicator, section 2.3). Stated differently, the SWI and SE indicators have been computed for the entire crop seasons and indeed stress periods are limited in time and occur basically during senescence at our study sites.

With regards to the 10-days periods, experimental data are uncertain by nature, subject to acquisition problems and, specifically for eddy-covariance systems, the energy balance closure is not guaranteed. By working on 10-days periods, we are sure that the set of observations is complete and, for eddy covariance measurements, that the energy balance closure is good (>80% as stated in the new version of the manuscript in section 2.3, p.9, l.30).

For both models, SEtHyS and TSEB, the changes in canopy radiation transfer are taken into account by changes of the fraction of green (based on the LAI which is a model input) and the soil and vegetation albedos which are tunable parameters. As a consequence, the senescence phase is treated equally and with equal capabilities as other periods from the radiative transfer point of view.

9) Figure 3: In the legend describe the difference between the top and bottom panels.

Thank you. Legend was reformulated for more clarity.

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?

OK. The SVAT model solves the surface energy balance. The radiative forcing in the short and long wavelengths is obviously one of the main drivers of the convective fluxes as shown in figure 4. Likewise, VPD, even if it is not a direct input of the model, significantly impact LE flux predictions following the formulation of LE based on a gradient of vapor pressure. Nevertheless, in this study, we considered typical errors that may be expected on input forcing when scaling up to the agricultural landscape. At this scale, some input variables will be more uncertain than another. For instance, at the landscape scale, a measure or an estimation of Rg can be obtained accurately while wind speed, that may be derived from large-scale re-analysis, is always very uncertain as it depends on local conditions. This is the reason why wind speed appears more impacting than radiative forcing in our study. Moreover, it is important to note that a white noise is added to the meteorological forcing and that the RMSE for reference simulations on H and LE are equal or superior to 30 W.m-2 and 50 W.m-2. As a consequence, the addition of a white noise can bring some compensation with this level of reference error. Following the reviewer's comment, the text of section 3.2.3 has been updated.

11) Figure 6: The relatively minor impact of biases in Ts (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? OK. We got the reviewer point. Nevertheless, surface temperature is only a proxy of hydric conditions when water is limiting. As most of our study sites are irrigated and located in temperate areas (at least for the french sites), water limiting conditions only occurs during short periods. It is likely that when focusing on water stressed periods, the sensitivity to surface temperature error for TSEB is expected to be higher. Nevertheless, as stated in point [8], stress periods are almost absent from our data set during the growth period. Finally, it has been shown that the assumption of a canopy transpiring at a potential rate in this model (even it is can be bypassed in some conditions) is strong and limit the sensitivity of the model to surface temperature errors.

12) It would be very nice to see a figure identical to Figure 6, but for the SVAT model.

As explained in point [1], TIR data are not assimilated in the SVAT model but used to constrain the SVAT model multiobjective calibration (MCIP methodology). The specific contribution of TIR data to SEtHyS model calibration was published in Coudert et al. 2006, 2007 and 2008. An equivalent to the Figure 6 can be found in Coudert and Ottlé 2007 (Figure 3). We agree that this was not clearly stated in the previous version of the manuscript. Several rewritings all over the manuscript have been proposed to avoid the confusion.

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.

Yes. This is a very relevant issue and we agree that water stressed period are very occasional within our database apart from the end of the growing period when vegetation is senescent. That is the reason why we recently conducted a new experiment in Morocco (seasons 2017-2018 and 2018-2019) focused on water stress during which stress was intentionally triggered on one wheat field. Nevertheless, the processing and use of these new data is beyond the scope of the paper. These limits have been stressed again in the new version of the manuscript when the performances of the models during stress periods are analyzed and also in the conclusion part.

14) Section 3.2.8: The authors state that the parameter Vmax0 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.

We are sorry that it was not clear in the former version of the manuscript, but indeed what was called "assimilation procedure" in the previous version of the manuscript, referred to adjustment/optimization of the parameters values. It has been changed to "calibration procedure" in the new version. In addition, among the 22 parameters, Vmax0, which represents the leaf photosynthesis capacity of Rubisco (Table 2), and which affects the assimilation rate and consequently the global evapotranspiration flux, was identified as one of the most sensitive. From a period to another, the calibration procedure leads to a variability of the optimal values of the Vmax0 parameter along the growth season. The calibration results show generally higher values during the periods with higher vegetation LAI where the evapotranspiration flux is maximal. Another coherent result (according to measurement of photosynthesis assimilation rate and stomatal conductance) is the lower values of Vmax0 obtained for corn and sunflower than for wheat. The figure 1 (attached to our answers) illustrates these results.

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However, in order to clarify and focus the paper on its main objectives, we have removed the discussion on the parameters sensitivity analysis and calibration (section 3.2.8 in the former version of the manuscript).

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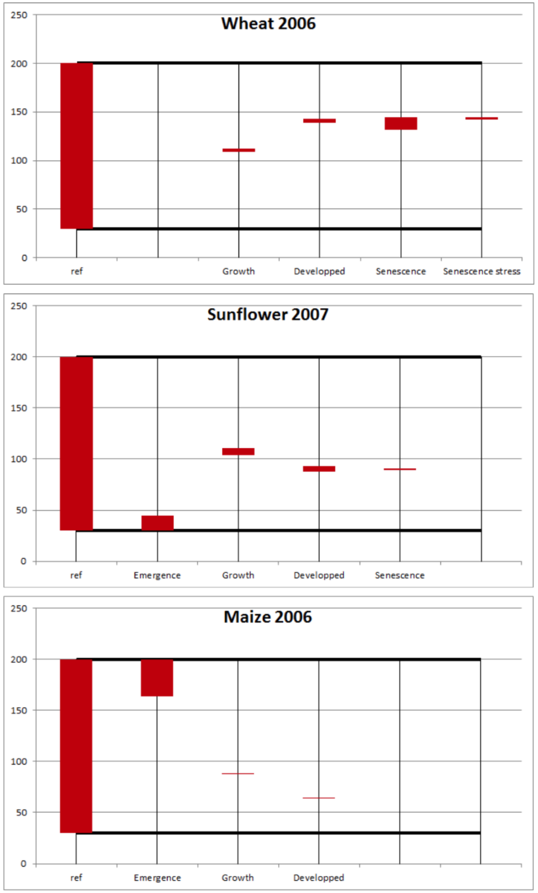
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Fig. 1.