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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 and some new analyses have 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.

General comments:

[1] With regards to application of TSEB, in the model description, it appears they are using most if not all of the original formulations of the Norman et al (1995) model, for example Eq. (9) for partitioning net radiation (Rn) for the soil and canopy elements. However later they state that they adopt a more physically-based Rn divergence model of Kustas and Norman (1999). Yet in the sensitivity analysis (Table 1) this extinction coefficient for Eq. (9) is retained and evaluated later in Figure 9 which is not consistent with what is stated in the text.

Agree. The word "out-of-the box" used in the introduction was certainly confusing as we wanted to state that we had implemented and tested most of the improvements published since the original version of TSEB, apart from the Penman-Monteith version. The short answer is that the added value of most of the tested improvement were not clear with our database. This is why we choose the term "out-of-the-box" and also for the simplicity of the presentation in the previous version of the manuscript. The detailed responses are listed at point [2]. The introduction has been reformulated (also in response to reviewer 1) and "out-of-the-box" has been removed.

Concerning the specific formulation of net radiation, we agree there has been a mismatch in the text as the formulation used in our study is the one presented Anderson et al. (1997), following the Beer's extinction law and accounting for the dependence to the solar zenith angle (Rnsoil=Rn*exp(-Kapa LAI/sqrt(2.cos(phi)))). We've also tested the new formulation for clumped crops as clumped crops may intercept a lower part of the incoming radiation than if leaves were randomly distributed (RnSoil=Rn * exp(-Kapa*Gamma*LAI)) as introduced by Campbell (1998). Following Kustas et al., AFM (1999), we also tested the formulation of radiative budget separating short and long wavelengths. However the latter two formulation didn't improve the estimations of LE with our dataset and were not retained. We give more detail about this point in the next question. This has been corrected in the new version of the manuscript and the reference to the improvement with regard to the initial Norman et al. (1995) formulation are properly referenced in the text following the reviewer (new section 2.4, p.11, l.24).

[2] While in the references they appear to cite papers that have included new formulations being implemented in the TSEB since the original 1995 paper, they are not included in this paper. The TSEB model has undergone several modifications since it was first presented by Norman et al. (1995).

As stated above, we had already tested several of the new development listed by the reviewer. The detailed responses are given in table 1 (attached to our answers). Following these results, we choose to keep the following parameterization in our version of the TSEB model: - Rn: Anderson, 1997 - G: Norman et al., 1995 - Transpiration: Anderson, 1997 (Priestley-Taylor) - Surface resistance: Norman et al., 1995 - Aerodynamic resistance: Norman et al., 1995

* refinements to the algorithm estimating soil aerodynamic resistance and shortwave and longwave transmittance through the canopy (as they mention in their paper; Kustas and Norman, 2000)

We agree with the reviewer that our text was (very) confusing. Cf. point [1].

In addition, considering the specific point of the formulation of net radiation, we agree that there has been a mismatch in the text. Both the Kustas and Norman (2000) and a modified version of the formulation of Norman et al. (1995) including the gamma factor for clumped canopies have been tested and obtained results were very close: the Root Mean Square Error (RMSE) of net radiation were 46.5 W/m^2 for the version derived from Norman et al. (1995) and 61.5 W/m^2 for the version of Kustas and Norman (1999). The modified original formulation providing with slightly better results, we choose to keep it in our study. Concerning the soil resistance, the version of Kustas and Norman (2000) was also adopted in our version of TSEB (cf. their equations 7 and 8). The presentation of the TSEB model has been reformulated and we hope that the parameterizations we used are now clearly stated.

* a means for adjusting the Priestley–Taylor formulation for canopy transpiration (Kustas and Norman 1999)

OK. Actually, the adjustment of the PT coefficient was also already activated in our version of TSEB. This is now clearly stated in the new version of the manuscript (section 2.1.2, p.5, l.24). Nevertheless, only marginal improvement are obtained as 0.7% of unlikely soil condensation cases occurs in our database at the half hourly time step.

* incorporating rigorous treatment of radiation modeling for strongly clumped row crops, accounting for shading effects on soil heat flux (Colaizzi et al. 2012a, 2016a,b)

OK. The Kustas and Norman, Agr. Jour., (2000) and Kustas and Norman, AFM (1999) modifications for sparse canopies and clumped crops were not adopted (cf. point [1] and point [2] above) as they do not give better performances on our crops and surface conditions (see results in the table 1 attached to our answers). The results of the sensitivity analysis to the Kappa parameter showed that optimal values were close to the original Norman et al. (1995) value of 0,45 and a clumping factor Gamma equal to 1. The only exception was sunflower, whose optimal value was about 0,75 (see Fig.9a of our former version of manuscript) corresponding to a clumping factor above 1 suggesting a clumped row. This seems consistent with the geometry of sunflower. The last developments of Colaizzi 2012a were not implemented because no separate measurements of bare soil and canopy temperatures were available in our database, as a consequence no validation was conceivable. However, we thank you for this comment because a recent experimental campaign in a small watershed (Auradé site of CESBIO in the south west france) with surface radiometric temperature measurements and “thermodynamical” soil and vegetation temperatures measurements with thermo-buttons has been carried out and will be used in further work to investigate this point.

* incorporating alternative formulations for computing the canopy transpiration such as Penman–Monteith (PM) or light-use efficiency (LUE) parameterizations (see Colaizzi

et al. 2012b, 2014, 2016c; Anderson et al. 2008). The later two canopy transpiration formulations are mentioned but not applied in this paper.

Agree. To our opinion, the incorporation of new formulations of canopy transpiration was an important drawback of our study. The Penman-Monteith (PM) approach has thus been implemented and tested. The comparison results between the PM and the Priestley-Taylor (PT) approaches are summarized in the table 2 (attached to our answers) for all data (for the half-hourly time step and for daily average) and then by stages of growth and by crops.

LE dominates the partition of convective fluxes within our irrigated sites and percentage of errors may thus be high on sensible fluxes as they exhibit much lower absolute values than LE fluxes. The two versions are thus quite close in terms of fluxes predictions, in particular for LE but the PT version is systematically better. A deeper look at the results shows that LE is strongly overestimated by the PM version, mainly during the rising and the growth stages of growth. This leads to significantly higher RMSE and MAPD while correlation coefficients remain close between the two versions. This could probably be related to the fact that our sites are located in relatively wet environment (the moroccan site is located at the center of an irrigated perimeter of 3000 ha while the sites in south western France are also surrounded by crop fields). The introduction in the parameterization of LE of a dependance to wind speed aiming to better represent advection fluxes in the PM version doesn't achieve the expected improvement within this specific conditions.

With regards to the results presented above, we choose not to retain the PM formulation of transpiration.

[3] Alternatively, the SEtHyS is a SVAT model with 22 parameters and so it is unclear why such a comparison is actually being made between a relatively simple but fairly robust thermal-based model and a SVAT having a large number of tunable parameters.

OK. The approaches are compared as they are both extensively used to map evapotranspiration and because modeling concepts are fundamentally different: for SEB models, surface temperature is the proxy for the crop hydric conditions while the hydric conditions are predicted thanks to a mechanistic water budget for SVATs. Several teams including our are working on the joint use of both approaches through the assimilation of snapshot evapotranspiration estimates to constrain the SVAT "continuous" predictions following Crow et al. (2005). To this objective, errors and biases of both modeling frameworks must be characterized carefully with regards to phenological stages and input data. To our knowledge, this is done in this paper thanks to a unique database as it includes several sites and several seasons. The introduction and the abstract have been reformulated following the reviewer comment in the view to better described the objectives, to highlight that the comparison is carried out on a large and unique data base in the sense that the cropping conditions of our study sites are quite specific.

[4] It's also unclear why this comparison does not include application of a newly developed and presumably more robust two-source model SPARSE developed by one of the co-authors (Boulet et al.,2015).

When the work has been carried out, SPARSE model was under development. Nevertheless, recent comparison between the TSEB model and SPARSE have shown that SPARSE was very close to TSEB (Boulet et al., 2015). The comparison to SPARSE is beyond the scope of the paper but SPARSE will probably be adopted in further studies.

[5] Additionally, for the sensitivity analysis, the authors do not appear to be aware of the several studies that have already performed sensitivity analyses for key inputs to TSEB. These include two of the papers mentioned in this manuscript... Timmermans et al (2007) and Zhan et al. (1996). There is also Li et al (2005) mentioned in the manuscript and then there is the paper by Kustas and Norman (1997) and Kustas et al. (2012).

OK. Thank you for the references. Zhan et al., 1996 intercompared model for sensible heat fluxes. The sensitivity analysis was also carried out with regards to H only. In addition, forcing meteorological variables such as incoming radiation and air humidity were not considered in this study. Finally, the database comprises a much lower number of observations than ours. Li et al. (2005) is focused on the sensitivity to Leaf Area Index (and thus fractional cover Fc as an empirical relationship relating Fc to LAI is used). They highlighted that introducing a 20% bias in LAI lead to about 15-20% difference in H for the parallel version of TSEB and about half of these values for the series version (5-10%) demonstrating the higher robustness of this latter version with regards to LAI/Fc inputs. Kustas and Norman (1997) analyzed the sensitivity of four versions of the two source model: the initial version of Norman in its parallel and series configuration and two version taking advantage of two view angle of Tb. The selected input variables for the sensitivity analysis are: wind speed (bias of 50%), air temperature (bias of 3K), LAI (bias of 50%), green fraction (reduction of 0.2) and radiometric temperature (bias of 1.5K). Kustas et al. (2012) performed a “worst case scenario” on one specific day of acquisition adding what they called “large” errors on Ta (+1 and +3K), Tr (-2K) and wind speed (+1.5 m/s) while biases appeared quite similar to the previous studies. The study of Timmermans et al. (2007) is quite similar as a bias of 25% is added or subtracted to Tr, Ta, u, z0m, LAI, fc, vegetation height hc. One specific day is chosen.

With regards to the sensitivity analysis presented above, our study is positioned quite differently as (1) realistic errors (errors that can be expected when applying the models over a heterogeneous agricultural landscape) are applied including both white noise and biases and (2) we cover larger growth, crops and hydric conditions thanks to our unique database.

[6] In summary it appears they conduct an analysis with a dated TSEB model without some of the more current refinements and comparing it to a SVAT that has a number of tunable parameters that would be difficult to prescribe over a large area without detailed ground information. There are a significant number of analyses performed

making it a long paper and is somewhat diffuse in its focus. While I think the paper has some unique findings, it does not consider some of the main advances in TSEB when evaluating model performance for these agricultural sites.

Agree. Cf point [1] and [2] with regards to the considered refinements and point [3] together with responses to reviewer 1 regarding the focus of the paper. We believe that the significant rewriting of the manuscript together with the new analysis that have been carried out make the paper clearer now.

[7] Early season conditions when the canopy is small, the soil is playing a major role in the energy exchange, and there is no discussion of soil roughness effects on the TSEB formulation that has been discussed in the literature (Kustas et al., 2016).

Agree. A mention to the need for a potential adaptation of the soil resistance parameterization for rough and partially vegetated surfaces, very likely conditions at the beginning of the crop season and after harvest, is added at (section 3.1, p.13, l.22) referencing Kustas et al. (2016).

[8] Errors in TSEB during senescence will largely depend on how well the green fraction is determined...However it should be pointed out that these later stages of vegetation condition are not as important to capture the ET as during the main growing season.

Agree. This was added in the manuscript (section 3.1, p.13, l.15).

[9] While I consider this work as having some merit, particularly the analyses performed with SEtHyS, it seems the authors do not consider to any degree of the advances/refinements made in the TSEB model since Norman et al (1995) and therefore I question how relevant is their analyses and conclusions using the 20+ year old formulations evaluated here in comparison to the more current parameterizations. Agree. Cf. point[1] and point[2].

Specific comments:

Page 9: It appears the leaf area and green fraction data are very local and may not reflect conditions viewed by the radiometer. This can be a major issue. Is there any indication where they sampled is representative of the radiometer field of view?

This is indeed a potential major issue. On the experimental sites, special attention is given to sample vegetation on areas representative of the radiometers' fields of view.

Page 9: Eq (15). What values are assumed in the Penman-Monteith equation for computing LEpot?

OK. Potential evaporation was computed with the classical Penman-Monteith formulation (and not ET0 from the modified FAO56 approach). The jarvis formulation extrapolated to the canopy based on LAI is used for the canopy resistance and the minimum resistance is equal to 90 s/m. This has been specified in the new version of the manuscript (section 2.3, p.9, l.31).

Page 10: How is the calibration of SEtHyS carried out and what level of calibration is shown in Figure 2 for the SEtHyS model?

OK. More details about the MCIP methodology were added in the manuscript, in particular in response to reviewer 1. 5 objective functions are optimized simultaneously. The five objective functions are detailed in section 2.4. They are built to minimize the distance between model predictions and observations thanks to RMSE. An ensemble of simulations based on a monte-carlo sampling of the parameter space is carried out. At each simulation (based on a specific parameter set) corresponds several objectives functions to minimize (RMSE of LE, H, Rn, Tb, W_rz). The global minimization is obtained following a Pareto ranking based on these objectives functions. Basically, a simulation is classified as "better" than another (thus at a lower rank) if all these objectives functions have lower values. For more details the MCIP methodology is described in Demarty et. al, 2004 and 2005.

Figure 2 presents simulation with optimal set of parameters as stated in section 2.4. This was added in the caption for more clarity.

Page 10: So the TSEB performance is "sought in its out-of-the box configuration presented in Norman et al (1995)" suggests none of the refinements over the last 20 years are incorporated in this analysis.

Indeed, this was badly formulated. The manuscript was updated with better overview of improvements tested and used. Cf. point [1] and point [2].

Page 10. The 3 parameters identified for study are the Priestley-Taylor coefficient, the net radiation extinction parameter and the fraction of soil net radiation for estimating soil heat flux, G. There is some interdependency here between the amount of canopy net radiation interception and the value of the Priestley-Taylor parameter (Kustas and Norman, 2000). Also for G, refinements of the TSEB include time varying formulation proposed by Santanello and Friedl (2003).

Yes. Agree with the reviewer. Thank you. A sensitivity analysis carried out by Diarra et al. (2017) demonstrated these equifinality issues between Kapa and the Priestley-Taylor coefficient but this study also showed that the partition of available energy between H and LE is quite robust with regards to these parameters values.

Concerning other formulations of the conduction fluxes, the parameterization of Santanello and Friedl (2003) was also tested but didn't provide an improvement of the results (cf. table at point [2]).

Page 12 line (10): TSEB could be provided albedo inputs from remote sensing. This is something easily done in the model if made available.

Agree. Thank you. The objectives of the study was to evaluate the best performance from the best possible inputs. The observed albedo were thus used. As stated by the reviewer, accurate albedo can be obtained from remote sensing observations either by computing specific empirical equations to some band reflectances (usually red and NIR bands) or using directly products (such as MODIS).

Page 12 (line 15): The authors do not seem to be aware of the soil resistance formulation that is sensitive to soil roughness which is discussed in refinements to the TSEB model (Kustas et al., 2016).

We thank the reviewer for his suggestion. This paper shows that the values proposed for the a and b coefficients of the rs parameterization (Norman et al., 1995) are associated with an underestimation of latent heat fluxes for sparse vegetation in semi-arid conditions. Nevertheless, the absolute values of LE are low during the emerging and rising stages and even if high relative error values (MAPD) are highlighted, absolute values are limited.

Page 12 (Line 30): Its unclear what version of SEtHyS model (1-4 from page 10) is being used in these comparisons.

Agree. The performances and sensitivity analyses presented were done with the "optimal" sets of parameters, i.e. the sets of parameters processed for each phenological stage and culture class. This is stated clearly in the manuscript (section 2.4, p.10, l.23).

Page 13 (line 5): The Crow et al (2008) paper actually showed the utility of TSEB in providing an indicator of plant stress for assimilation in a water balance model.

This paper is very interesting and is a good illustration of the possible complementarity between TSEB and a WEB-SVAT. We hope that the focus of our study is now more precisely explained. We precisely intent to bring elements concerning the domains of validity of the models and their performances through a variety of surface and meteorological conditions, taking into account models parameters and inputs sensitivity in order to consider the different couplings between both approaches for agricultural landscape spatialization purposes.

Page 15 Sensitivity analysis to meteorological inputs: It has been long recognized that to apply TSEB regionally requires a way of reducing the need for accurate absolute surface-air temperature differences. This was the motivation for the development of time differencing modeling schemes (Anderson et al., 1997; Norman et al., 2000).

Yes exactly, the question of the coupling between air temperature and surface conditions is determining in the surface budget and the convective fluxes calculation. Our proposition is to compare local measurements to regional estimations of air temperature from reanalysis in order to have a realistic uncertainty on model input when simulation run at field scale or homogeneous entity scale for SEtHyS and pixel scale for TSEB for regional application. Time differences in surface temperature methodology for estimating relevant air temperature is suited to large scale applications while in our study we propose to estimate the uncertainty for field scale simulation for landscape/regional application. The text of section 3.2.1 has been modified to make this clearer.

Page 15 Sensitivity analysis to vegetation forcing inputs: The use of micrometeorological measurements close to the canopy height is ill-advised in general due to roughness sublayer effects and so comes as no surprise for the TSEB since the aerodynamic resistances are key to the TSEB calculations. This should be removed.

Yes, this joins our previous answer. TSEB is supposed to be applied at high resolution TIR pixel (i.e. Landsat 8).

Page 17: Sensitivity analysis to radiative temperature for TSEB: This is well documented and the reason why time differences in radiative temperatures were developed early in the TSEB applications (see Anderson et al., 2004)

Page 17-18: sensitivity analysis to water inputs and soil water content for SEtHyS: This is a major issue with SVAT models. That is why approaches like Crow et al (2008) of combining water balance with remote sensing energy balance is appealing. Moreover, for regional analysis it will be very difficult to acquire irrigation information in a timely manner.

Yes, indeed. The Crow et al. (2008) is one of the founding paper for our work. How to combine a complex SVAT that suffers from uncertain water inputs (irrigation) at the plot scale and an energy budget model providing snapshot evapotranspiration estimate from instantaneous surface temperature observations ? Characterizing the model errors and the domain of validity of both models is the prerequisite step that we develop in this study before a joint use of both approaches through data assimilation. We hope that the positioning and the objectives of our study are more clearly stated in the new version of the manuscript.

Page 22 (figure 9): These results are related to some extent on the radiation partitioning which the authors appear to have adopted the original formulation of Norman et al (1995) for net radiation extinction and without any clumping effects which row crops tend to have (Anderson et al., 2005).

OK. This has been clearly stated in the previous point, in particular point [1] and [2].

Page 25 (figure 11): Did the authors consider the fact that extinction of diffuse light through a canopy is quite different from direct and perhaps that is another factor affecting the Priestley-Taylor value?

We totally agree with the reviewer remark. Overcast and clear skies conditions are treated the same way in the calculation of the net radiation and of the radiation divergences through the canopy in the version of TSEB we used. However the more physically-based description described in Campbell et al. 1998 and implemented in Kustas & Norman 1999 with specific extinction for diffuse or direct radiation through the canopy was tested but did not give better results. Clear sky radiation is mainly direct while overcast radiation is more diffuse, which is certainly affecting the Priestley-Taylor coefficient value. A proposition to take this into account would be to use the “SKYL” factor (accounting for the ratio between sky irradiance and total (sun + sky) irradiance) instead of modulating the Priestley-Taylor coefficient according to the cloudiness. Our dataset doesn’t include “SKYL” in situ measurements, it could be eventually estimated with some error. In our study, the “SKYL” is not taken into account neither for TSEB nor for SEtHyS and the physical description of the radiation divergence through the canopy are consistent between both models. The interest to propose directly a modulation of the Priestley-Taylor coefficient in such conditions lies in the multiplicity of the factors

that should be determined in this case of low atmospheric demand in term of evapotranspiration simulation. In our sense, this goes in the direction of the conclusions of Kustas & Norman 1999. This comment was added in the manuscript in section 4.2 (p.24, l.5).

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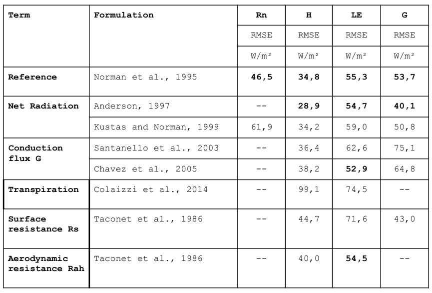
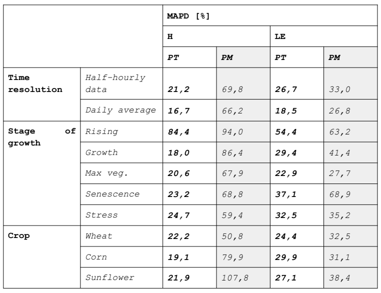


Term	**Formulation**	**Rn**	**H**	**LE**	**G**
		RMSE	RMSE	RMSE	RMSE
		W/m²	W/m²	W/m²	W/m²
Reference	Norman et al., 1995	**46,5**	**34,8**	**55,3**	**53,7**
Net Radiation	Anderson, 1997	--	**28,9**	**54,7**	**40,1**
	Kustas and Norman, 1999	61,9	34,2	59,0	50,8
Conduction flux G	Santanello et al., 2003	--	36,4	62,6	75,1
	Chavez et al., 2005	--	38,2	**52,9**	64,8
Transpiration	Colaizzi et al., 2014	--	99,1	74,5	--
Surface resistance Rs	Taconet et al., 1986	--	44,7	71,6	43,0
Aerodynamic resistance Rah	Taconet et al., 1986	--	40,0	**54,5**	--

Fig. 1.



		MAPD [%]			
		H		**LE**	
		PT	***PM***	***PT***	***PM***
Time resolution	*Half-hourly data*	***21,2***	*69,8*	***26,7***	*33,0*
	Daily average	***16,7***	*66,2*	***18,5***	*26,8*
Stage of growth	*Rising*	***84,4***	*94,0*	***54,4***	*63,2*
	Growth	***18,0***	*86,4*	***29,4***	*41,4*
	Max veg.	***20,6***	*67,9*	***22,9***	*27,7*
	Senescence	***23,2***	*68,8*	***37,1***	*68,9*
	Stress	***24,7***	*59,4*	***32,5***	*35,2*
Crop	*Wheat*	***22,2***	*50,8*	***24,4***	*32,5*
	Corn	***19,1***	*79,9*	***29,9***	*31,1*
	Sunflower	***21,9***	*107,8*	***27,1***	*38,4*

Fig. 2.