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Interactive Comment

Interactive comment on "Inter-comparison of energy balance and hydrological models for land surface energy fluxes estimation over a whole river catchment" *by* R. Guzinski et al.

Anonymous Referee #2

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This paper compares the temporal and spatial distribution of ETR computed by 3 models (2 surface energy balance SEB models and one distributed hydrological model, MIKE-SHE) in a large Danish river catchment.

The paper acknowledges the large range of values obtained by various SEB models and therefore the need to compare all outputs before considering data assimilation of ETR maps obtained by SEB models in a distributed hydrological model. All 3 models are presented on the same level, even though the difference in terms of input, calibration issues and constraint is large. The differences between SEBs and MIKE-SHE are well described in introduction. However, since water balance models at least close the water budget, they seem therefore superior in a way to the energy balance methods which are bounded by the sole available energy. If the final goal is to assimilate ETR maps derived from remote sensing in a distributed model, one expects from this paper to answer at least partially the following question: “what is the added value of the surface energy balance models in an Observing System whose state-space model is a coupled hydrology-SVAT model ?”. The results only briefly tackle this issue; it should be emphasized in a revised version. As it is, the point of view of the authors is that of the micrometeorologist rather than the hydrologist. For instance, all results are expressed as instantaneous fluxes at different times of the day (different overpass times). Instantaneous fluxes at 11:30AM and 13AM local time can differ from more than 100W/m2, which makes me doubtful about the relevance of such an intercomparion. In order to intercompare the models, results should be translated in, at least, daily totals in mm/day. Why not use classical EF extrapolation and interpolation methods to convert instantaneous ETR estimates to daily and seasonal ETR values ? The cumulative values should also be compared to the information amount already provided by the potential ET which is a good reference to assess the added value of any ETR model (this is also true for instantaneous values).

Major comments:

- Abstract is not conclusive enough and the introductive part of the abstract (roughly half of it) is too long. You should state clearly the outcomes of the study. I don’t see, in its present state, what insight the paper brings to the hydrological modelers to consider assimilating ETR maps in their models. After reading the paper, if I was a modeler, I’d keep the current calibration method based on the sole available data incl. surface temperature ! Actually, whether one must directly assimilate surface temperature instead of ETR maps derived from surface temperature in a distributed model is still an open question, and the answer lies in a careful analysis of the various uncertainties and the consistency of the spatial covariances, not only on the average catchment outputs. This should be commented.

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- The structure of the energy budget (dual source model with series resistances) is essentially the same between MIKE_SHE and both SEB models but the formulation of the aerodynamic resistances (soil to aerodynamic level, vegetation to aerodynamic level) are different (Shuttleworth Wallace vs original Norman et al.). What part of the current findings comes from this difference ? It would be easy to implement the same resistance scheme in the 3 models, this would ensure that the differences come from the methods (constraint on ETR by surface temperature vs by the water budget) and not the algorithmic choice.

- The reference mentioned for TSEB-ART is a conference abstract, I think that for the sake of comprehension of the paper the model should be better described in the manuscript so that the reader can grasp the difference between both SEB models. Actually, the presentation of the SEB models should focus on the main difference between the two models: both use two surface temperatures to constraint the dual source energy budget. However, in DTD, the constraint on ETR is the same as the original TSEB model, the day/night surface temperature data being used only to get rid of systematic errors in the surface temperature estimates. The vegetation is transpiring at the potential rate unless there is an inconsistency in the soil surface temperature retrieval. In TSEB-2ART on the other hand, it seems to me that the directional temperatures are used to derive the soil and vegetation component temperatures, and that the transpiration rate is therefore computed directly as a residual term. It's not clear how the resulting inconsistencies are treated. It seems from P5917 L13 that inconsistencies are not corrected for but simply ignored. The lack of robustness of most ETR retrieval algorithms based on the use of surface temperature data is an open issue for hydrological applications and should be dutifully commented in the paper.

Detailed comments:

- P5909 L8: This statement is too strong. Both approaches (single and dual source with series resistances) compute an aerodynamic temperature as a common source of heat for the whole surface. Whether this temperature has a physical meaning is

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not certain. I'd rather say "the two-source models have the advantage of explicitly representing the separate contribution of the soil and the vegetation and avoiding the need of an excess resistance whose value differs largely from one reference to the other"; provide a more recent reference for the "excess" resistance (kB-1) issue (e.g. (Matsushima 2005; Kustas and Anderson 2009; Boulet, Olioso et al. 2012))

- P5913 L20: Same as above (Norman et al., 1995 is a rather old reference on the kB-1 issue)

- P5917 L12-15: What is the percentage of discarded pixels/dates that don't meet each of the 3 conditions ?

- P5915: I find this part confusing and actually do not understand how the various Rs values are derived. If b and c values for short crops (LAI<2) are different in temperate and arid regions then Rs is an empirical relationship. It seems to me that the Shuttleworth-Wallace (SW) expression of this resistance gives satisfaction in all climate conditions with the same amount of input data. It's actually the expression used in the Mike-SHE model. Why not use this resistance formulation then ?

- P5931: why use a uniform and constant vegetation height in the SEB models rather than the formulation used by Mike-SHE ?

- P5940 and 5941: all findings indicate that MIKE-SHE simulates a lot more stressed vegetation than both SEB models(lower ETR and higher vegetation temperature). This should be commented more largely in the text. Given your knowledge of the crops and the climatic conditions, do you think that the stress level simulated by MIKE-SHE is realistic ? If not, this would help answering the question about the added value of SEB models in an Observing System.

- P5943: merging all seven years in a single Figure is confusing. I don't understand why the line (catchment average) is so far below the circles (even though those circles represent clear sky days only). Please indicate the catchment average potential ET in

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a second line in order to assess the overall catchment water stress.

References

Matsushima, D. (2005). "Relations between aerodynamic parameters of heat transfer and thermal-infrared thermometry in the bulk surface formulation." Journal of the Meteorological Society of Japan 83(3): 373-389.

Kustas, W. and M. Anderson (2009). "Advances in thermal infrared remote sensing for land surface modeling." Agricultural and Forest Meteorology 149(12): 2071-2081.

Boulet, G., A. Olioso, et al. (2012). "An empirical expression to relate aerodynamic and surface temperatures for use within single-source energy balance models." Agricultural and Forest Meteorology 161: 148-155.

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