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Hydrology and Earth System Sciences Discussions

Interactive comment on "Seasonal evaluation of the land surface scheme HTESSEL against remote sensing derived energy fluxes of the Transdanubian region in Hungary" *by* E. L. Wipfler et al.

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Dear editor,

Referee #2 raised a number of points that we will address below:

Comment 1: . . .They are probably lead to believe this by highly underestimated error bars on the SEBAL method. The claimed accuracy (3-5%) is typically the one which is claimed by the satellite instruments which measure the top of the atmosphere radiation

budget. As the surface radiation fluxes and subsequent evaporation estimations are only derivations from top of the atmosphere observations they can by no means be as accurate ... unless we know the atmosphere perfectly!

Response:

* The accuracy of 3-5% refers to the seasonal timescale and a spatial resolution of 25 km2. The temporal scale the referee refers to is that of < 1 day.

Comment 2: The description of HTESSEL seems to be incomplete. The equation 4 is inconsistent with the different types of evaporation presented afterwards.

Response:

* Referee#2 probably refers to differences between mm and W/m2. We removed inconsistencies in the revised paper

Comment 3: Throughout the seasonal average is used as an evaluation scale. I guess the summer season is meant but it could also be any of the 3 other seasons or all together. I have not seen where this is clearly defined and should I have missed it then it needs to be reminded at a few strategic locations and detailed in the captions of the figures.

Response:

* The definition of the season used is given in Section 3.3.3. We added a reminder in Chapter 5 and 7 of the revised document.

Comment 4: The comparison of SEBAL and the fluxes observed at the 2 towers needs to be detailed and underpinned with some figures. To my knowledge the accuracy of flux towers are not better than single digit per-cents. So if SEBAL is better than that we should see it clearly for E, H and E/Rn in a comparison with Fluxnet data.

Response:

* The advantage of SEBAL above tower data is that it provides spatially covering data at a spatial resolution that is comparable with the model output. The objective of the comparison of SEBAL and the tower fluxes in Section 3.2 is not to show that SEBAL fluxes are better than tower data or to compare the accuracies of both data. On the contrary: we aim to evaluate the accuracy of the SEBAL fluxes in the test region. We are aware that the fluxes measured in the towers are prone to measurement errors. Furthermore, we use short wave downward radiation from the towers as input for SEBAL. Strictly speaking, the comparison is not between two independent datasets. Yet, the comparison gives at least some indication of the accuracy of SEBAL for this specific region, time window and satellite sensors.

Comment 5: Using the 2 towers some evaluation of the spatial correlation of the fields could be made. This should be one of the trumps of satellite-derived products, which need to be compared with model outputs.

Response:

* It is not clear to the authors how this should be done with only 2 towers?

Comment 6: Using honest error estimations of P and E a shaded zone should be drawn around the line E=P in figure 6. Are then the areas which are assumed to be irrigated or influenced by groundwater uptakes significant outliers? This would be much more convincing. Drawing that zone by hand I guess it will be a close call.

Response:

* We calculated the standard deviation of the TRMM data with respect to the meteo station data as a measure of the accuracy, which is 85 mm.

* We also added the accuracy in the revised paper and in Fig. 6.

* With Figure 6, it was intended to show the possible correlation between irrigation and effective precipitation. To test the statistical significance of our null-hypothesis, i.e. that P-E <0 for the red gridcells in figure 7, we calculated the p-value of the null-hypothesis,

using an estimated standard deviation of 85 mm. The p-values for the dark red cells in the regions A, B and C were 0.68, 0.73-0.8 and 0.84-0.88, respectively, which is fairly significant. The authors are aware though that more research is needed to confirm the hypothesis.

Comment 7: Figure 8 is very misleading as it is averaged over a very large area. Nevertheless why is the difference between SEBAL and HTESSSEL only attributed to incoming solar radiation. Are the 2 models using the same assumptions for albedo and surface temperature ? This is probably not the case and could explain the differences in Rn without any problems.

Response:

* We choose to average the net radiation over the whole test area to show that the underestimation of Rn-RACMO as compared to SEBAL was significant and could be observed in more than just one gridcell.

* The remark of the referee is true: the difference in net radiation may be also due to albedo or surface temperature. However, we think that attributing differences in net radiation to differences in incoming short wave radiation is very plausible as it is very difficult for climate models to predict cloud thickness correctly. Nevertheless, for the purpose of comparing SEBAL and HTESSEL lambda/Rn we considered it important to have similar Rn. By correcting the Rs-downward we could easily achieve this goal.

Comment 8: Why is there no detailed presentation of all the fluxes (observed, SEBAL and HTESSEL) at the 2 towers ? I would guess that the discrepancies of the 2 estimations dwarf the issue of scales when comparing point observations with area averages. This is worth showing and discussing.

Response:

* The authors think that the issue of spatial scale differences is certainly an interesting topic to investigate. However, it is outside the scope of the paper.

Comment 9: As alluded to earlier the big advantage of remote sensing derived products is their spatial coherence. Why is there no discussion of the weekly correlations of the flux maps between HTESSEL and SEBAL. This could for instance show that the lacking irrigation and groundwater recharge progressively leads to a degradation of the spatial correlation during summer.

Response:

* We agree that this would certainly be an interesting exercise; however, we worry about the assumption that the systematic error of SEBAL is 0, on both small spatial scales and on small temporal scales. The error distributions in SEBAL predictions are established to the best of our knowledge. Scaling results down is very risky and could lead to seemingly consistent discussions based on quicksand. Comment 10:The choice of sensitivity experiments performed with HTESSEL is surprising. The matrix (table 3) does not contain a simulation in which only the number of levels in the soil changes. There is sufficient literature to demonstrate that this is not without consequence on the annual cycle of evaporation and it needs to be documented for HTESSEL here.

* We did evaluate the effect of discretisation on lambda/Rn, however, the effect was negligible and therefore it was not shown. In the revised paper, however, we included some remarks in Section 6.4.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6293, 2009.