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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,

The core of our reply to reviewer#1 is that we feel that what the referee considers as "flaws" can also be interpreted as a difference in perspective, which we regard as acceptable. Clearly pointing out these differences could then remove the flaws and constitute a contribution to scientific discussion.

Comment 1a: My main criticism is the implicit assumption in the text that the SEBAL fluxes (derived from remote sensing) are accurate enough to constitute "truth" for the evaluation. The SEBAL fluxes may take some inputs from remote sensing, but this doesn't mean that they're intrinsically accurate; on the contrary, by not explicitly accounting for water stress impacts on evaporation (apparently using instead, at least in part, some kind of surface temperature - NDVI metric for water stress), and by making a host of other assumptions, the accuracy of the SEBAL fluxes is limited and probably questionable in many places. In fact, one could argue that the SEBAL flux accuracy shouldn't be expected to be greater necessarily than that of the HTESSEL fluxes being evaluated.

Response:

* SEBAL does in fact account for water stress. Although certain assumptions are being made, the SEBAL algorithm is physically based as it solves the energy balance. Water stress is accounted for by calibration on dry (hot) and wet (cold) pixels.

* The referee is correct in that we assume the SEBAL fluxes to constitute 'truth'. The accuracy of the SEBAL data is clearly stated in the paper. We are aware that the explanation of the SEBAL algorithm in the discussion paper is limited. This might be the cause of the skepticism of the referee. Therefore, we extended the discussion on the SEBAL algorithm in the revised paper.

* SEBAL has been tested at different spatial and temporal scales and under different climatic conditions. We refer to Bastiaanssen et al. (2005) for a detailed description of the accuracy of SEBAL. This article summarizes the validation studies that have been reported in the academic society. A large professional community has shown confidence in the SEBAL algorithm by using the algorithm to quantify turbulent heat fluxes to support e.g. the evaluation of water rights in Idaho (ref.1) , or the use of satellite techniques to monitor and control the evaporation quotas of counties for water saving in the Hai Basin in China (ref. 2)

* What we are interested in is not so much the local predictive quality in terms of grid cell error, but the regional predictive quality, i.e. the overall predictive error for the whole region.

Comment 1b: The authors provide Figure 4 as a way of demonstrating the validity of the SEBAL fluxes. The evaporation fluxes from SEBAL are compared in the right panel of Figure 4 to measured fluxes at some meteorological towers, and at first glance, the two sets of fluxes do look highly correlated. Evaporation, though, almost certainly increases with net radiation, so all we're seeing here (I think) is the ability of SEBAL to capture the net radiation, which is inherently easier and is, in any case, already shown in the left panel. A *true* test of SEBAL against ground observations would involve constructing a scatter plot of lambdaE/Rn values, not E values, for Figure 4. I'm guessing that such a scatter plot wouldn't look so good.

Response:

* The reason for showing figure 4 is to show the ability of the researcher to correctly apply the SEBAL algorithm. Hence, the intention of the exercise is not to evaluate the performance of the SEBAL algorithm; that has been reported extensively in the literature (e.g. Bastiaanssen et al., 2005).

* We plotted the of lambdaE/Rn of SEBAL and the corrected lambdaE/Rn of the towers. We found a satifying correlation between tower data and SEBAL estimates for Bugac and a rather poor correlation for Matra: i.e. a regression coefficient of 1.24 and 1.41 and a coefficient of determination, R2 of 0.14 and 0.56 for Matra and Bugac, respectively. In the revised paper we added remarks on this issue.

Comment 2: The lambdaE/Rn ratio is actually used later in the work (e.g., Fig. 5), showing that the authors recognize this ratio as an important diagnostic. This, however, brings up a troubling issue. I'm sure that this is not true, but at least the appearance is there that the authors sometimes use E rather than lambdaE/Rn to make their statistics look better. I'm speaking now (in addition to Figure 4) of the final row of Table 4. Why is

the correlation coefficient of E shown, rather than that of lambdaE/Rn? Again, you get excess correlation with E values just because E scales with the net radiation – it's not a true test of the model's ability to partition the radiation between latent and sensible heat. Again, I'm sure the authors don't mean to do this, but the table, as written, comes off looking misleading.

Response:

* The reviewer seems to be unaware that we scaled the (3-hourly) model net radiation (Rn) using the (weekly) SEBAL Rn. This implies that the effect of Rn on evaporative flux differences is small. In that light, the suggestion that we mislead the reader is odd. However, for consistency and readability, we changed the correlation coefficients of E to lambdaE/Rn in the revised paper. As expected, the conclusions remain the same.

Comment 3: I'm especially confused by the statistics discussed in Table 2. The percentiles may match, but do the fluxes match in the *same geographical locations*? Isn't that the appropriate statistical measure of success? Couldn't you get the right percentile values with the low and high values from SEBAL in different locations than those of HTESSEL? The percentiles-based statistical analysis comes off looking strange and ineffective. Again, why don't the authors don't plot the SEBAL seasonally-averaged lambdaE/Rn (not E) at each grid cell against the corresponding HTESSEL values in a scatter plot and then compute the correlation between the values?

Response:

* In figure 9 the evaporative fraction is compared at grid cell level (and we added the correlation coefficients in the revised paper in table 4). However, as stated before, we are mainly interested in the regional predictive quality, i.e. the overall predictive error over the test region and not in prediction capacity at grid cell level. Percentile-based statistics is quite appropriate for our objective.

Comment 4: I'm a little surprised by the use of TRMM data to correct the RACMO data.

TRMM is focused on tropical precipitation, and its accuracy over land, relative to that over the ocean, is small. Can the authors comment on the accuracy of TRMM data at 47 degrees north? Isn't there a gauge-based dataset available? Is the TRMM data used some kind of merged product?

Response:

* The TRMM data is indeed a merged product; it is a calibrated dataset that uses different sources. However, because climate models are known to have difficulties to predict precipitation correctly, TRMM is almost certainly more accurate than precipitation data from RACMO. Furthermore, TRMM precipitation itself is bias-corrected based on ground observations from 35 meteostations. As such we combine the spatial structure in the TRMM data with the reliable precipitation volumes of the gauge data.

* The standard deviation of the TRMM data as compared to the meteo station is provided as a measure of the TRMM accuracy (85 mm). We added a remark on the accuracy in the revised paper and insert the accuracy information in figure 6.

Comment 5: To summarize some of my main concerns, there are, I think, at least four reasons for differences between the SEBAL fluxes and the HTESSEL fluxes: (a) the SEBAL fluxes are themselves estimates and are likely to be inaccurate; (b) the HTESSEL model probably has sub-optimal parameterizations; (c) the forcing used for HTESSEL may be off during the evaluation period; (d) the initial conditions for the HTESSEL runs may be inappropriate (only two configurations of initial conditions are examined here).

Response:

(a) SEBAL fluxes are indeed derived quantities. Nevertheless, their value, accuracy and applicability are very well documented in the literature. See further our response to comment 1.

(b) This is precisely the motivation of this research. The objective of the paper is to

evaluate the parameterization of HTESSEL. We formulated different model versions based on our expert knowledge of the system and tested which version of the model performs better, i.e. arrive at an improved version by model evaluation.

(c) We have matched the atmospheric forcing (i.e. net radiation and precipitation) between SEBAL and HTESSEL as good as possible using well-documented adjustment procedures with the purpose of ruling out this source of discrepancy.

(d) We have quantified the effect of different estimates of initial values on the lambda E/Rn distribution by first using more realistic initial conditions represented by the RACMO initial conditions and, then using the more theoretical initial conditions that are obtained by cycling the model through 2005 until the equilibrium state is reached.

ref 1: See the work by Rick Allen, author of the FAO56 computing crop water requirements, in Idaho: http://www.idwr.idaho.gov/GeographicInfo/METRIC/et.htm

ref 2: see the link below which provides the link to a World Bank document on this work: http://www.scidev.net/en/news/china-urged-to-rethink-water-monitoring.html

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