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Hydrology and Earth System Sciences Discussions

Interactive comment on "Remotely sensed latent heat fluxes for improving model predictions of soil moisture: a case study" *by* J. M. Schuurmans et al.

Anonymous Referee #2

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The paper deals with the use of thermal infrared remote sensing to improve a hydrological model. Surface temperature images were converted into ET map using the SEBAL algorithms. I have several major comments on the paper :

1) Normalisation in Equation 3 is really not understandable. If we develop it we arrive to the following correction term ETact/ETpots*(delta ETpotaverage). So the correction is the strongest when ETPots is the lowest. The term can be very important when ETpot is lower than the Delta. This is not really justified. A single bias correction using the

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Delta value would be better justified. It is not clear in the text what means a 2.5 – 2.74 mm bias correction?

2) Computation of the reference ET by Eq 4 needs more explanations? Is Feddes relationship suitable to be implemented in every layers? What are the assumptions on the root distribution? Why not using the soil water storage variation to estimate ET? There are much less hypothesis behind such calculation (the only one is to ignore the water flows at the bottom of the measured profile) whereas with the proposed error model error are cumulated and may be strong and very difficult to determine.

3) In scen 2 the proposed approximation need more explanation. Can we really make extrapolation with erroneous value (i.e FRnew>1) to provide water potential in the wet domain (h>h3). The fact of having FRnew>1 reveal computation inconsistency in both ET et ETp. There is no reason to link such inconsistencies to the soil moisture.

4) In scen 3 and 4: I don't understand what was done. Why not applying Feddes curve with FR new or somewhere between Fr new and Frm (to take into account that both estimate are characterized with errors).

5) The impact of the paper is very low since the evaluation of the RS data assimilation cannot be evaluated. However, an evaluation could be made at the measurement station level with at least the ASTER data considering their high resolution (25 m). Why this was not done instead of invoking problems in representativity (6191-8)

6) In discussion, the disagreement observed on the forest can be also a weakness of SEBAL. As a matter of fact SEBAL discriminate water stress conditions with the measurement of the surface temperature. Due to a strong roughness, the super temperature signature of forest water stress is very low and so, more difficult to detect. This explains why, water stress may not be observed with the SEBAL method.

7) HOW Etpot is estimated by SEBAL?

8) Figures are too small.

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9) I am not a native English speaking, but I found that English should be improved. Many editorial improvements should be made as well.

a. 6188 L24 what means VMC

b. 6188 L16 is the unit being mm or mmd-1? I don't understand the meaning of such numbers

c. 6189 L5-10 not clear

d. 6189 L21-22 not clear

e. 6191 : the notion of reduction is not clear (difference between dates or between method???). As we have no information of the water storage at both dates, all comments on temporal reduction are very difficult to follow.

f. Fig 3 what means h3l and h3h

g. Fig 4 . Units are missing for the ET

h. Fig 5 what means left and right in the legend

i. Fig 7 root zone storage of what?

For me the assimilation process is really the major weakness of the paper. In its present form, It should be clarified and improved. Scen 2 has no scientific meaning . The author must demonstrate that FRnew variations above 1 is related to soil moisture. Scen 3 and Scen 4 are really not clear. The paper cannot be published whithout justifying all hypothesis in the assimilation scheme.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6179, 2010.