

Interactive comment on “FAO-56 dual approach combined with multi-sensor remote sensing for regional evapotranspiration estimations” by R. Amri et al.

Anonymous Referee #2

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The paper "FAO-56 dual approach combined with multi-sensor remote sensing for regional evapotranspiration estimations" by Amri et al. describes an approach to better estimate the ET from vegetated surfaces by including soil moisture information in the FAO-56 method (via a new evaporation component) and to compare it against ISBA-Ags model output over a site in Tunisia.

This paper is a slightly different model to Er-Raki et al. (2010) and deserves publication. In the current form, this paper requires major revisions, as there are points that will need clarification or more detailed discussion.

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Major comments:

1. The results section is not much longer than the discussion/conclusion. Out of 8 pages of text, only 1 page has any results. The remainder is occupied by the introduction/background, site/model description and conclusions. This is not sufficient. At the very least I would expect a study on the improvement over the Er-Raki et al. (2010) method. This is not given. All the reader is presented with is a comparison of two models. There is no validation reference other than the land surface model, nor is the said comparison against previous model versions presented. This is necessary in order to understand the level of improvement that may be achieved using the extended methodology, as presented in this paper.
2. The motivation as to why the Merlin et al (2011) methodology was chosen to be included is not clear. Er-Raki et al. (2010) presented a very well working method, using simply NDVI. The authors need to make a stronger point to include a surface soil moisture product, as well. In particular, as they point out towards the end, that this product only provides 2-3cm of the top soil moisture.
3. (p. 8119, l. 10-18) I am not clear of your motivation. This is not a strong argumentation to introduce a new index into the model. I think that a better argument could be formed around the more reliable availability of microwave products as opposed to optical ones.
4. (p. 8119, l. 26-29) : the issue there is with optical data products is that they may saturate. This should not be a problem in semi-arid environments, but the general implications should be discussed.
5. Introduction : Overall, the introduction lacks some depth. There is no clear distinction at this point that would suggest why the original method by Er-Raki et al. (2010) should be improved. What is also lacking is a broader discussion of other papers that have used the FAO-56 method (or similar PM methods) with remote sensing data. A brief list with suggested references can be found at the end of this review.

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6. (p. 8122, l. 7-18) : Be more specific about how those parameters were derived.
7. (p. 8124, l. 21) : what is the error term in detail, and how did you quantify it for this study?
8. (p. 8126, l. 15) : how did you arrive at those values, as they differ from Er-Raki et al. (2010).
9. (p. 8129) : at this point, I would expect a discussion on the scale discrepancies between the remotely sensed data product and the surface conditions/model. There is some hinting at it, when talking about ISBA only representing an average of all contained vegetation types, but using 40km and 1km-resolution data products and a model at probably a different resolution again, raises some questions about scaling of data products, that, at the very least, needs to be discussed.
10. (p. 8129, l. 19-25) : The relatively high coefficient of determination is - in my view - partially due to not correcting for the dynamics in the models. There is a strong seasonal correlation, however, the more important question is whether the diurnal or even monthly changes are well captured. That cannot be fully answered with those statistics. Also, for me, the SWI products do not compare well after 1997, with the ERS SWI essentially losing all sensitivity. There is no real discussion around any type of actual reference values. Even if there are no in-situ measurements available, Meteo France did some studies comparing ERS & ASCAT with model and in-situ observations (Albergel et al., 2008, Rudiger et al., 2009). At the least you need to discuss the accuracy of either model or remotely sensed product.
11. (p. 8130, l. 6-8) : there is no discussion about the obvious outliers in Fig. 5. Where, when, and why do they occur?
12. (p. 8131, l. 2-3) : I am not entirely sure whether the root zone product is used at all in this approach (please explain this in more detail with Eq. 7). But if it is then it is not a surprise that FAO-56 method is not working all that well in 2000, given that the ERS

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SWI is fairly flat and not showing much dynamical range.

13. Fig. 7 : there are large areas with very low ET on the plot. How are those explained? They correlate with the low fractional coverage areas in Fig. 3. Were those not considered at all, or blanked out?

Minor comments:

1. (p. 8118, l. 16-17) : Is this a recent development due to climate change or is the statement meant in a broader general term?
2. (p. 8118, l. 20) : rephrase to "[...] rainfed agriculture in water-limited environments"
3. (p. 8118, l. 23-25) : are you saying that ISBA is the best land surface model there is? I would be careful with strong statements like this!
4. (p. 8119, l. 20) : don't use "..."
5. (p. 8120, l. 2) : reference Paloscia et al (2008) is not in the reference list
6. (p. 8120, l. 1-6) : there are many more (recent) publications both for soil moisture retrieval models and ERS-Scat validation.
7. (p. 8120, l. 6) : not clear whether you are using the old or new SWI (Albergel et al., 2008)
8. (p. 8120, l. 21) : are those the mean daily extremes or absolute extremes?
9. (p. 8121, l. 21) : here the observation depth is 5cm, later, it is "2 to 3cm" (p. 8129, l. 16-17).
10. (p. 8123, l. 12) : is there a peer-reviewed reference to this data set?
11. (p. 8128, l. 9ff) : move the paragraph describing the model into Section 3. Why did you not use the diffusive layer model version of ISBA?
12. (p. 8130, l. 3) : what units are the ISBA pixels "0.5 x 0.5"?

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13. (p. 8130, l. 20-21) : why would the models be more consistent during this season than others?

14. (p. 8131, l. 23) : the dual approach was not proposed here, but by Er-Raki (2007, 2010).

Figures:

Combine Fig. 1 & 2.

Figure 3: there are areas that have low fractions on all three plots (SW and NE corners, how can that be?)

Figure 6: y-axis should be in mm/d (I assume that the values are monthly averages of total daily ET?!)

Overall, the figure captions need to be more descriptive.

References :

Campos et al., J. Hydrol., 494, 1-9, 2013.

Prasad et al., J. Agromet., 15(1), 23-30, 2013.

Cammalleri et al., Int. J. Appl. Earth Obs. and GIS, 21, 149-158, 2013.

Sanchez et al., Agric. Water Mgt., 98(1), 69-78, 2013.

Kamble et al., Remote Sensing, 15(4), 1588-1602, 2013.

Torres et al., Remote Sensing in Hydrology, in: IAHS Publication 352, 401-405.

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