

Interactive comment on “Performance of METRIC in estimating hourly and daily evapotranspiration fluxes over an irrigated field in Saudi Arabia” by Rangaswamy Madugundu et al.

Anonymous Referee #2

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General comments

The study presents an evaluation of the METRIC model on a alfalfa field located in Saudi Arabia. Although the work is not particularly original, the validation could be of particular interest for HESS readers because of the study area located in Saudi Arabia that is not well known. Nevertheless, I have some major concerns: (1) the reliability of the results: there are many discrepancies in the provided statistical values between the abstract, text, figures and tables; (2) the methodology lacks important information and (3) the results discussion should be considerably strengthened.

Specific comments

C1

*The introduction is poorly structured: L.15-25: The paragraph is described in vague terms. The conclusion “the ET values measured by the EC system need to be adjusted, through an appropriate method, to improve their accuracy” is not clear to me. What the authors want to explain? That the energy closure of EC system is not always satisfied? I am not sure that an entire paragraph should be devoted to this point. Twine et al. (2000) should be cited then. Twine, T.E., Kustas, W.P., Norman, J.M., Cook, D.R., Houser, P.R., Meyers, T.P., Prueger, J.H., Starks, P.J., Wesely, M.L., 2000. Correcting eddy-covariance flux underestimates over a grassland. *Agric. For. Meteorol.* 103, 279–300. doi:10.1016/S0168-1923(00)00123-4

The state of the art concerning the RS approaches to monitor spatialized ET is not sufficiently detailed. The FAO-56 approach is an interesting alternative to thermal based approach and thermal based approaches are usually separated into image-based method (named contextual) and pixel to pixel based where the energy balance is solved independently from one pixel to another. The cited article Kalma et al., 2008 together with Courault et al., 2005 could certainly help to improve the introduction.

Courault, D., Seguin, B., Olioso, A., 2005. Review on estimation of evapotranspiration from remote sensing data: From empirical to numerical modeling approaches. *Irrig. Drain. Syst.* 19, 223–249. doi:10.1007/s10795-005-5186-0

The objectives are not clearly stated.

*The study area description should be strengthened. Please provide some details on the typical annual cycle of alfalfa crop in the region (in the 2.1 part for instance) and on the soil type

*LANDSAT8 LST: please provide some details on the split windows algorithm and give the proper references of the software.

*P7 L1-7: give some detail on the Footprint analysis approach

*P.7 L23: What is the “EC flux tower measured temperature (TEC)” ? Is it derive from

C2

the upward longwave component measured by the CNR4 ?

*The discussion on the results should be strengthened: - Providing scatterplot only does not help in this objective. A time series, of at least LE, showing both in situ and satellite estimates should be shown and discussed

- Discussing on statistics with such a small sample of data may be uncertain.

- Please organize and strengthen your discussion. For instance, the "Sensible heat flux" part (3.2.3) is very difficult to follow after the first sentence where you provide the statistics of the comparison between EC and metrics:

"The high RMSE value of 72.01 W m⁻² (63.54%) for the HRS might be due to the advection and variability in the canopy density."

Right. Advection may very very strong in hyper arid environment but you could give some references to support your comment.

"Hence, most of the Rn has been partitioned into LE than into H, as introduced by the near surface air temperature difference (ΔT) and the aerodynamic resistance of heat transfer (r_{ah}), i.e. propagation errors."

Not clear to me.

"This was evident in the linear regression analysis (Figure 7), where a good correlation between the HRS and HEC ($R^2 = 0.61$) was observed; however, it was not significant ($P > F = 0.022$), and it was also confirmed with the RMSE 10 of 63.5%."

I don't see the link with the preceding sentence.

"In contrast, Carrasco-Benavides et al. (2013) reported that the METRIC algorithm overestimated the H component by 39 W m⁻² with a mean absolute error of 10%."

In the previous sentence, you were commenting the correlation and the RMSE, this one refers to bias. Not clear.

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* Energy balance (3.2.6): please provide a figure of the EC EB closure in the section 2.2 (and explain if a correction for EB closure has been applied). The discussion on the EB closure at the date of the LANDSAT images acquisition should be put earlier in the results section

*Please check the consistency of the statistical value in the abstract, text, table and figures.

Technical corrections

p.1 L. 29: please replace "can" by "could" p.2 L12-14: should not be placed here just after the listing of in situ approaches to monitor ET (FAO p2. L20: "techniques of energy closure and BR" Bowen ratio is a technique of energy closure here p2. L24: "the accuracy from 79.2% to 95.2%." replace "accuracy" by "closure" p.3 L11-12: already written earlier in the introduction p.3 L22-23: "Due to the high crop water demand combined with the highly erratic rainfall, irrigation is entirely provided" to be replaced by "Due to . . . , irrigation is a pre-requisite for crop growth. It is entirely provided by . . ."

p.4 L6: "The missed data was filled" please provide some details on filling the missing data p.5 L11-12: to be put in the part describing the pre-processing of LANDSAT8 data p.6 L31: ET24 already described above P7 L9: check the numbering for all part. p.7 L26-27: Please reformulate as lysimeters does not provide any measurement of surface temperature

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