

***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 #1

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The manuscript entitled “Performance of METRIC in estimating hourly and daily evapotranspiration fluxes over an irrigated field in Saudi Arabia” by Madugundu et al presents the results of a validation study of the METRIC evapotranspiration and surface heat fluxes retrieval algorithm using Landsat-8 satellite images over an irrigated alfalfa field in Saudi Arabia equipped with eddy covariance system units. Although the goal of the study is not clearly stated, to my knowledge, the study is the first of its kind in Saudi Arabia, applying and validating a remote sensing algorithm for surface energy balance over irrigated fields in hyper-arid environment. As such, the study may be interesting to HESS readership interested in the surface energy balance in such environments and in remote sensing algorithms. However, the manuscript 1) lacks crucial information: a

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clear stated goal, some clear interpretation of discrepancies, graphics and definition of variables, 2) there are apparent inconsistencies between the scores presented in the abstract, text and tables, 3) some methods are not described (eg FTP) and use of correlation for such small sample of data may lead to not meaningful results. I list here detailed comments I have while reading the manuscript, also including the 3 points mentioned above.

- The title should be revised to explicitly mention the use of satellite remote sensing (Landsat).
- Introduction: the goal seems to be stated in p.3 l. 11-15. However, the three successive sentences are not linked by logical reasoning. The first states about crop water management, which is a matter of almost “real-time” data. The second is about lack long-term spatial data (by the way, there is a spatial ET dataset over part of Saudi Arabia for 1992-2014 processed with SEBAL & MODIS (Mahmoud & Alazba, 2016, J Asian Earth Sciences, 124, 269-283)), which seems not to be connected to the first sentence and do not justify the developments presented in the third sentence. A clear statement of the objective of the study with its motivation and a stress on novelty is needed at the end of the introduction.
- section 2.2: Are the EC data corrected as suggested in the introduction p.2 l.15-25? If not, this need justification. If it is, the method should be described.
- section 2.6 and 2.3: the method for footprint analysis should be described and properly referenced, a name of a program can also be written.
- section 2.6: the use of some coefficients for comparison with such a small sample of observations may lead to not significant conclusions.
- section 3.1: the “tower measured temperature”, T_{EC} , is not defined. What is the height of measurement? If it is not a surface temperature, there is no reason to find an agreement with Landsat land surface temperature, because those relates to differ-

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ent heights. Moreover, the explanation given for the discrepancies is not clear to me. Revisit of the text is probably needed here.

- section 3.2.2: the explanation given in p 8, l.29-31 is not clear. I do not see a constant relation between the two variables from Figure 6 for LAI>4.2, neither is obvious the scatter for LAI>4, as there are only 2 data.
- section 3.2.5 should contain all the statistical results obtained for ET hourly and daily. It is stated in the abstract that hourly ET was overestimated and daily ET underestimated. It should be written in the section.
- there is an inconsistency between the statistical scores for ET given in the abstract, in section 3.2.5, in Table 5, and conclusion: this needs to be clarified.
- the location of section 3.2.6 at the end of the paper is awkward, this should be moved in section 2.2 and clearly linked to the rest of the manuscript.
- Abstract: is there any reason for daily ET to underestimate, while hourly ET is overestimated by METRIC ?
- Figure 4,5 7 and 8 are already included in figure 9, they could be removed.
- A graphic for daily ET comparison is lacking in the manuscript.
- Table 3: why is Zm changing from date to date? Is a correction done to account for this variation in the model?

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