

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.

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Author Response to Anonymous Referee #1

Referee comment (RC); Author Response (AR)

RC-1: Lack crucial information: a clear stated goal, some clear interpretation of discrepancies, graphs, and definition of variables.

AR-1: As suggested, crucial information such as (i) clear statement of goal, (ii) strengthening of results and discussion part with-out any discrepancies, and (ii) presentation, graphs, and definition of variables, has been modified.

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RC-2: There are apparent inconsistencies between the scores presented in the abstract, text and tables.

AR-2: Agreed, there were an inconsistency between the scores presented in the abstract, text and tables due to oversight at the time of compilation/manuscript preparation. The discrepancies have been rectified.

RC-3: Some methods are not described (eg. FTP) and use of correlation for small samples of data may lead to not meaningful results.

AR-3: Methods section will be modified as per referee comments. A detailed description on FTP method will be provided in the revised manuscript. As the referee raised that “the use of correlation for small samples of data may lead to not meaningful results”. Results obtained with the use of correlation procedure will be given less importance. Alternatively, Mann-Whitney U-test and/or Kruskal-Wallis H test, which are often used in applications involving small size samples, will be used as described in Gisondi et al. (2004) and McCune and Grace (2002).

RC-4: - The title should be revised to explicitly mention the use of satellite remote sensing (Landsat-8).

AR-4: As suggested, the title has been modified, “Performance of METRIC Model in estimating Evapotranspiration fluxes over an irrigated field in Saudi Arabia with the use of Landsat-8 images”.

RC-5: - 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 “realtime” data. The second is about lack long-term spatial data (by the way, there is a spatial ET dataset over part of Saudi Arabia processed for 1992-2014 processed with SEBAL and 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 in the third sentence. A clear state-

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ment of the objective of the study with its motivation and a stress on novelty is needed at the end of the introduction.

AR-5: As suggested, the goal of the study has been stated clearly in the revised manuscript. The sentences were logically linked. A clear statement of the objective of the study with its motivation is added.

RC-6: Section 2.2: Are the EC data corrected as suggested in the introduction p.2 L. 15-25? If not, this needs justification. If it is, the method should be described.

AR-6: Yes, the EC data was corrected as stated in the introduction. As suggested, text on methods/techniques used in each step of EC data correction has been added in the revised manuscript.

RC-7: 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.

AR-7: As suggested, detailed description on “footprint analysis model” used for the study has been added.

RC-8: Section 2.6: the use of some coefficients for comparison with such a small sample of observation may lead not to ignificant conclusions.

AR-8: Results obtained with the use of correlation procedure will be given less importance. Alternatively, MRPP procedures (Mann-Whitney U-test and/or Kruskal-Wallis H test), which are often used in applications involving small size samples will be used as described in Gisondi et al. (2004) and McCune and Grace (2002).

RC-9: 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 surface, because those relates to different heights. Moreover, the explanation given for the discrepancies is not clear to me. Revise of the text is properly needed here.

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AR-9: The term “tower measured temperature”, T_{EC} , has been defined. The height of the measurement is 3.74 m from the soil surface. It is a surface temperature and can be used in the comparative assessment of Landsat estimated surface temperature. The inconsistencies in the explanation has been removed and the text has been modified.

RC-10: Section 3.3.2: the explanation given in p 8, L 29-30 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.

AR-10: The explanation pertaining to LAI has been revised with clear statements.

RC-11: 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 the daily ET underestimated. It should be written in this section.

AR-11: As suggested, all the statistical results obtained for ET (hourly and daily) is stated and the section has been modified accordingly.

RC-12: There is an inconsistency between the statistical scores for ET given in the abstract, in section 3.2.5, in Table, and conclusion: this needs to be clarified.

AR-12: All the datasets, obtained results and their analysis has been reviewed. Inconsistency among statistical scores across the manuscript has been corrected and the concerned sections/parts have been modified accordingly.

RC-13: The location of section 3.6 at the end of the paper is awkward, this should be moved to section 2.2 and clearly linked to the rest of the manuscript.

AR-13: As suggested, section 3.6 has been merged with section 2.2 and linked to the rest of the manuscript.

RC-14: Abstract: is there any reason for daily ET to underestimate, while hourly ET is overestimated by MERIC?

AR-14: There is a fluctuation in EC and Landsat estimated Sensible Heat flux, it might

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be due to the advection in case of hourly ET and variability in the canopy density with respect to studied footprint". Advection may vary strongly in hyper-arid environments.

RC-15: Figure 4,5,7 and 8 are already included in Figure 9, they could be removed.

AR-15: As suggested, Figure 4, 5, 7 and 8 has been removed in the revised manuscript.

RC-16: A graphic for daily ET comparison is lacking in the manuscript.

AR-16: Graphical representation of daily ET (EC measured and METRIC estimated) will be provided with the revised manuscript.

RC-17: Table 3: why is Zm changing from date to date? Is a correction done to account for this variation in the model?

AR-17: Depending on the height of crop, the Zm was varied. Yes, the correction was done with the use of an internal component of Eddypro software.

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