

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.

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 using 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 statement 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 (page 3; line 10 to 18).

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 (Page 3; Line 12 to 14 and Line 19 to 21).

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 (page 4, subheading Footprint analysis).

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

AR-9: The term “tower measured temperature”, T_{EC} , has been defined (page 10; Line 23). The height of the measurement is 3.67 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 (Page 2; line 7).

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 (Page 12;Line 6-11).

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 (Page 13; Line 29).

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 removed and placed as section 2.4 as and linked to the rest of the manuscript (page 5; Line 20 – 30).

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

AR-14: There is a fluctuation in EC and Landsat estimated Sensible Heat flux, it might 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 (Page 13; Line 30 – 34).

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) has been provided as Figure 8 in the revised manuscript.

RC-17: Table 3: why is Z_m 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 Z_m was varied. Yes, the correction was done with the use of an internal component of Eddypro software.

Author Response to Anonymous Referee #2

Referee comment (RC); Author Response (AR)

General comments:

RC-1: The reliability of results: there are many discrepancies in the provided statistical values between the abstract, text, figures and tables.

AR-1: Agreed, there were discrepancies in provided statistical values among the abstract, figures and tables. Those discrepancies have been rectified and the corresponding section of the manuscript has been revised.

RC-2: The methodology lacks important information.

AR-2: Detailed description of methods used for the study has been incorporated.

RC-3: The results discussion should be considerably strengthened.

AR-3: The results and discussion sections have been modified considerably.

Specific comments:

RC-4: - The introduction is poorly structured:L 15-25:The paragraph is described in vague terms. The conclusion “the ET values measured by the EC systems need to be adjusted through, through an appropriate method, to improve their accuracy” is not clear to me. What the author want to explain? 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.

AR-4: Introduction and conclusion sections have been modified. The explanation on Energy closure of EC system modified and the Reviewer suggested references were incorporated in the revised manuscript (Page 2; Line 20 to 30).

RC-5: - The state of 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-basedmethod (named contextual) and pixl to pixel based where the energy balance is solved independently from one pixel to another. The sited article Kalma et al., 2008 together with Courault et al. 2005 could be certinly help to improve the introduction.

AR-5: The image-based method has been followed for the monitoring of spatialized ET. As suggested, reference of Courault et al. (2005) is added along with Kalma et al. (2008) in page 2 line 15.

RC-6: The objectives are not clearly stated.

AR-6: The goal and objectives of the study have been stated clearly in the revised manuscript (Page 3; Line 10 – 19).

RC-7: 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 of instance) and on th soil type.

AR-7: Description of the study area, the typical annual cycle of alfalfa crop in the region and soil type of experimental plot has been added (Page 3; L29-33).

RC-8: Landsat8 LST: Please provide some details on the split window algorithm and give the proper references of the software.

AR-8: A detailed explanation on “split window algorithm” has been provided with the proper reference to the software. ENVI (Ver. 5.1) software has been used for the execution of split window model in Page 6.

RC-9: P7 L1-7: give some detail on the Footprint analysis approach

AR-9: A detailed text on Footprint analysis approach is added to the revised manuscript on page 4.

RC-10: P7 L23: what is the “EC flux tower measured temperature (TEC)?” is it derived from upward longwave component measured by the CNR4?

AR-10: Explanation pertaining to TEC and its method of measurement is provided in the revised manuscript (Page 10). Yes, the TEC is the product of a series of computations from upward longwave component measured by the CNR4.

RC-11: The discussion on the results should be strengthened: - Providing scatter plot only doesnot help in this objective. A time-series, of at least LE, showing both insitu and satellite estimates should be shown and discussed.

AR-11: Discussion and results sections has been strengthened. A time-series of LE showing both in-situ and satellite (landsat8) estimates has been provided and discussed accordingly (Page 13).

RC-12: Discusing on statistics with such small sample data may be uncertain.

AR-12: Results obtained with the use of correlation procedure will be given less importance. In addition, 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 Gisoni et al. (2004) and McCune and Grace (2002).

RC-13: Please organize and strengthen your discussion. For instance, the “Sesible 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.

AR-13: Discussion and results sections on sensible heat flux has been strengthened (Page 12; Line 13).

RC-14: 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 vary strong in hyper arid environment but you could give some references to support your comment.

AR-14: As suggested, appropriate references were provided to support the given statements (reference provided)

RC-15: 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 (r_{ah}), i.e. propagation errors”. Not clear to me

AR-15: The statement has been removed.

RC-16: “This was evident in the linear regression analysis (Figure 7), with the RMSE 10 of 63.5%”. I don’t see the link with the preceding sentence.” In contrast, Carrasco-enavids et al. (2013) error of 10%”. In the previous sentence, you are commenting the correlation and the RMSE, this one refers to bias, Not clear.

AR-16: As suggested, appropriate references were provided to support the given statements (page 12; Line 25).

RC-17: Energy balance (3.2.6): Please provide a figure of EC EB closure in the section 2.2 (and explain if a correlation 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.

AR-17: More explanation is provided on Energy Balance Closure. Discrepancies in the statement has been removed. The EB closure and the date of the Landsat image acquisition have been provided prior to the results section (Page 5; Table 2 and Figure 2).

RC-18: Please check the consistency of the statistical value in the abstract, table and figures.

AR-18: Agreed, there were discrepancies in provided statistical values among the abstract, figures and tables. Those discrepancies have been rectified and the corresponding section of the manuscript has been revised.

RC-19: Technical corrections.

AR-19: All suggested technical corrections has been incorporated in the revised manuscript.