

Interactive comment on “Assessment of actual evapotranspiration over a semi-arid heterogeneous land surface by means of coupled low resolution remote sensing data with energy balance model: comparison to extra Large Aperture Scintillometer measurements” by Sameh Saadi et al.

Anonymous Referee #1

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Interactive comment on “Assessment of actual evapotranspiration over a semi-arid heterogeneous land surface by means of coupled low resolution remote sensing data with energy balance model: comparison to extra Large Aperture Scintillometer measurements” MS No.: hess-2017-454. by Sameh Saadi et al.

The Authors present an extensive work (reinforced by experimental data) aimed to
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assess the operational use of the Soil Plant Atmosphere and Remote Sensing Evapotranspiration (SPARSE) model and its accuracy by a comparison to the Scintillometric technique. I think that Authors address relevant scientific questions within the scope of HESS. Furthermore the paper is generally well organized and well written and therefore the paper could be taken into account for the final publication after a moderate revision. Particularly, The Authors should improve the part of “Results and discussion” (pag. 16-20) with a better description of the validation of SPARSE model carried out with by comparing H and AE estimations with flux station and XLAS scintillometer (see comments n°7, 11 and 12). My comments and questions are as follow:

1. Lines 33-44: The Authors corroborated “the good correspondence between instantaneous H estimates and large aperture scintillometer H measurements” reporting RMSE values expressed in $W m^{-2}$. As stated by the Authors (Line 418) “For hydrological applications, daily ET is usually required...” and in my opinion this means that for hydrological purposes the accuracy of daily evapotranspiration should be expressed in millimeters for day (mmd^{-1}). Therefore in the abstract and through the paper this aspect should be considered and also critically analyzed. From my calculations the accuracy obtained by SPARSE model application should be around $1.6 mmd^{-1}$. Is this value “acceptable” ?
2. Lines 87-88: Is “irrigation requirements” (generally expressed in mmd^{-1}) a prerogative only “of RS-based SWB models” ? Please, clarify.
3. Line 108: “. . .at the beginning of the process”. Please clarify.
4. Lines 111-112: “. . .the lack of information about the actual irrigation scheduling adopted by the farmers is the critical limitation for SWB modeling”. I believe that various SWB models (Swap, Cropsyst, FAO56, AcquaCrop) are able to consider both scheduled by farmer irrigation (as input) or predicted irrigation (as output). Please, clarify or modify.
5. Line 123: Insert “. . .” in dual-source models.

6. Lines 152-154: Clarify that the “layer” approach of SPARSE is essentially a “dual-source” scheme.
7. Line 187: The Authors should explain (also under a theoretical point of view) the choice to install Scintillometer at a 20 m height. About the experimental setup it is strange the absence of a “net radiometer” that, on the basis of the footprint analysis, could be installed in the average prevalent source area of footprint. The Authors could explain this fact.
8. Line 280: The terms “incoming solar radiation” and “incoming atmospheric radiation” are correct but could generate a misunderstanding. Please use the more classical “shortwave” and “longwave” terminology in eq. (9) and explain how RS data are generally used to solve balance equation of radiation (eq.9).
9. Line 367: About the “Temporal interpolation of albedo and NDVI” some brief details could be considered.
10. Line 455: Which method has been used to evaluate the “potential conditions”, please clarify.
11. Lines 491-492: The Authors reported that . . .”An overestimation of about 15% is found between estimated and measured daily available energy. . . .and the coefficientswere applied to remove this bias”. If I well understand the above procedure (remove of bias) is a sort of calibration of the output of modeled on the basis of observed flux station. Please clarify.
12. Lines 526-527: About the estimation of sensible heat flux the authors reported that “This result is of great interest considering that the SPARSE model was run with no prior calibration”, but I feel a sort of contradiction with the bias removing procedure described in the above comment. Please clarify. Moreover I think that the Authors should describe the accuracy of model prior and after the bias correction.
13. Line 545: (Figure 7). Looking at the scatterplot it is clear a more dispersion for H

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value greater than 150. Is there an explanation of this ?

14. Line 604: The Authors reported that “Daily observed and modeled ET over the whole study period were both in the range of 0-4 mm mm.day-1 which is consistent with the land use present in the XLAS pat”. In my opinion this is a prosy comment, Trouble if not.
15. Line 616-617: The Authors reported that “Some points with little to null ET were recorded from May to July 2013 which can be explained by the very dry conditions and scattered vegetation cover with a considerable amount of bare soil”. Why this behavior was not observed in the same period of 2014 ?
16. Line 863: Please check the (Minacapilli and Ciralo, 2007) reference.

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