Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-437-RC2, 2016

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Feasibility analysis of using inverse modeling for estimating field-scale evapotranspiration in maize and soybean fields from soil water content monitoring networks" *by* Foad Foolad et al.

Anonymous Referee #2

Received and published: 20 September 2016

The manuscript describes an exploration into using ET derived using soil hydraulic parameters that are themselves inversely estimated from soil moisture measurements. The goal of the study is to validate additional data sources for LSMs. The manuscript is fairly well written, although further improvements can be made. While it is an interesting and required study, I do have a few concerns that I expect the authors to address before the manuscript can be accepted for publication.

P6, L114-119: Mention the instrument height above canopy for the EC tower. This would serve as a reference to validate your claim of the footprint size.

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P7, L138-139: The reference to integration of CRNP data into the NOAH LSM seems extraneous here, and would be better deleted.

P7, L141-142: No numbers are given for the footprint size of the EC tower. So there's no way for the reader to decide if this assumption is valid or not. Further, with the assumption made, a discussion on the implications of this assumption later in the manuscript would be a good addition.

P8, L163: Please provide references to the Beer's law.

P8, L167: It may be better to mention that the LAI was described in the previous or study area section, rather than "above".

P8, L168: A brief description of how the Feddes model makes use of the potential transpiration and the root density distribution is necessary. Further, no details of the root density used in the study are given, which should be rectified.

P10, L199-205: What were the objective functions and methodology used to optimize these parameters? No description of any sort is provided, which makes it very difficult to assess the applicability.

P11, L223: R-squared has a name. It is called the Coefficient of Determination. Also, while the other metrics are described in equations, R-squared is not.

P12, L230: What about R-squared?

P12, L236: This may be a matter of semantics, but I feel that the subsection is better titled as "Vadoze Zone Inverse Modeling Results". You are performing inverse modeling of the vadose zone, not modeling of the inverse vadose zone.

P12, L238/239/250: Figures 4 and 7 are interchanged. Fig. 4 shows the annual precipitation, and fig 7 shows the temporal evolution of daily SWC.

P12, L239: Not so clear. It may be good to mention that the large standard deviation values show this. Also, I was surprised to see that the upper layers had smaller

SD values than the deeper layers! As the authors themselves mention elsewhere, the soil moisture variability is expected to reduce with depth. Any discussion on this phenomenon would be welcome.

P13, L272-273: Based on the numbers in Table 3, I am not sure the data are “fairly well matched”. R-squared < 0.1 in the validation period (and < 0.4) in the calibration period), along with a negative NSE, tells me that the model and observation were not behaving alike. Maybe addition of distribution-level metrics could help bring out the relationship (if any) between the two better.

Also, here, and through the rest of the discussion, the authors use terms such as “fairly well matched” or “performed well” or similar language. These are highly subjective terms, and no analyses of numbers are provided to support these statements. It is necessary to establish at the beginning of the section what the authors consider as a “good” or “fairly good” etc., performance means in terms of absolute numbers. While the performance metrics are provided in the tables, no discussion is made regarding them and the reasoning for considering a particular statistic good.

P14, L282: How do these soil hydraulic parameters obtained from the inverse estimation compare with the textures used in the optimization? Further, while you mention earlier in the text that 6 different soil textures were used in the optimization, you omit mentioning which textures they are.

P14, L289: Provide a reference or hyperlink to the Web Soil Survey Data.

P15, L315: The infiltration rate in fine textured soil is lower, leading to higher surface runoff, as the authors mention. However, the water holding capacity of such soils is higher than coarse soils, leading to higher stored volume. I think a better argument here may be that the plant/root would have to overcome higher pressures to extract water from the fine soil, thus leading to lower ET.

P16, L330: Do you mean Figures 11 and 12 here? Figure 11 is never discussed in the

entire manuscript.

P16, L330-334: generally, the phenomenon of roots extracting water from deeper layers is seen in more mature vegetation such as trees, and not in seasonal agricultural crops. Also, even accepting that the plants may be drawing from layers deeper than the model domain, the phenomenon should not be so apparent in the clayey soils (TP4). A clayey soil restricts root penetration, and usually a shallow root depth is seen in such soils.

P16, L337: Clear solution to what?

Figures and Tables: I feel that, overall, the number of figures and tables can be reduced.

As mentioned earlier, Figures 4 and 7 are interchanged.

Figures 5 and 6: Keep any one of these two. No extra information is extracted by having two figures showing the same information here.

Figure 10: This can be merged with fig. 1.

Figure 11: This figure is never discussed in the text. Figure 12: This could be merged into fig. 11 as another panel. Also, in the text, this figure is discussed after fig. 13.

Table 1: These numbers can be discussed in the text instead of adding a single row table. As mentioned in an earlier comment, almost none of the numbers from the tables are discussed in context.

Table 3: Can be merged with tab. 2.

Table 7: There is no need for this table. The numbers can be mentioned in figs. 11 and 12. That would also make those figures easier to interpret.

Based on the above comments, I recommend that the authors be given an opportunity to make major revisions in the manuscript before resubmission.

Technical comments:

P4, L75: Should read as "... hyper-resolution LSM grid cells..." P5, L93: Check the spelling of the name "Simunek". P7, L135: "The CRNP measurement depth..." P7, L147: "... explained in detail by ..." P9, L176: "... GDD approximately 60-70..." P10, L195: The abbreviation TP has not been established earlier. P10, L198: the parameter "l" should be in lower case. P13, L262: "... criteria at TP locations..." P15, L302: "... inverse VZM modeling..." VZM already includes model. P16, L333: "VZM model" Same as above. References: Ensure uniform formatting of all the bibliography. Some end in page numbers, some in years, some in journal names, and some in volumes/issues. Table 4, Column 8: Use lower case "l" for tortuosity. Table 5, Column 6: Hectares in Field.

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