Anonymous Referee #2

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.

Thank you for the very insightful comments.

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.

According to Suyker 2004, the height of EC tower is 6 meter during the growing season and 3 meter before and after the growing season when nothing planting. We will add these details.

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

Thank you we will delete it.

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.

Thank you we will add that to the modified manuscript.

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

Thank you we will add that to the modified manuscript.

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

Thank you we will change that.

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.

Thank you we will add that to the modified manuscript. A full sensitivity analysis of the root model parameters is beyond the current scope of the paper and we refer the reviewer to Guswa (2012).

Guswa, A. J. 2012. Canopy vs. Roots: Production and Destruction of Variability in Soil Moisture and Hydrologic Fluxes. Vadose Zone Journal 11:3. doi:10.2136/vzj2011.0159.

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.

We could not optimize all the layers simultaneously because the maximum number of parameters that we can be optimized by the Hydrus-1D model is 15. We have followed the same procedure as Turkeltaub et al. (2015) and Wang et al. (2015, 2016). We used RMSE as our objective function and will clarify this more in the manuscript. Finally, a sensitivity analysis of all 24 parameters will be presented.

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.

Thank you we will change it in the modified manuscript.

P12, L230: What about R-squared?

In fact the primary objective function that was used to find the best sets of soil hydraulic parameters was RMSE and the others just were used to double check the optimization process. In the modified manuscript we will mention that we use RMSE values to choose the best set of the soil parameters. However, you are right and we should (and we will) add and name Coefficient of Determination (R-squared) as one of the objectives functions which were used for more investigation.

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.

Thank you, agreed. We will change it in the modified manuscript.

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.

Thank you, we will correct it in the modified manuscript.

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.

You are right we should say "according to the standard deviation value SWC varies considerably across the site, particularly during the growing especially in the deeper layers". When we say soil moisture variability is expected to reduce with depth we meant soil moisture variability expected to reduce with respect to time in each location alone not soil moisture variability in one location versus the other locations. We will clarify this in the revisions.

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.

In this study we tried to optimize soil hydraulic parameters based on the simulated SWC and observed SWC. RMSE was chosen as the main objective function to select the best sets of soil hydraulic parameters.

Thank you for your suggestion, we will add a section at the beginning and describe performance means in terms of absolute numbers for clarity.

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.

The soil texture data used in the optimization were just the model default and we used 6 sets of them in order to find the best sets of soil hydraulic parameters in the site based on the observed SWC. We will add this description to the text. Since we start with all 6 guesses not sure what this suggestion would accomplish?

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

We will add reference.

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.

Agreed. Thank you for the comment, we will add suggestion to modified manuscript.

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

Yes. We meant Figure 12 and 13, but we will check out how we have missed Figure 11 in the 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.

Those were our initial thoughts, but conversations with the site PI agronomists suggest water extraction up to 2 m, even in clayey soils! This is based on SWC readings from neutron access tubes in the surrounding fields part of the larger University of Nebraska Mead Extension Center. Root water uptake is very complex and we refer the reader to Guswa 2012 for a more in depth discussion. Also in TP4 location we said we expect to have clayey soils and Web Soil Survey Data confirms our results. We note that this conclusion is based on our simulation results and Web Soil Survey Data which only provides information for the upper soil layer not the deeper layers. Clearly investigation in root water uptake is an area that deserves more attention in LSMs, even in homogeneous annual crops. We are investigating spatial root and soil water interaction using hydrogeophysical mapping techniques in Prof. Franz's laboratory.

Guswa, A. J. 2012. Canopy vs. Roots: Production and Destruction of Variability in Soil Moisture and Hydrologic Fluxes. Vadose Zone Journal 11:3. doi:10.2136/vzj2011.0159.

P16, L337: Clear solution to what?

Thank you, we will rephrase it.

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.

Thank you, we will correct that.

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

Thank you, we will remove one of them.

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

Thank you. We will consider this.

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.

Thank you. We will check that out we should have missed it, and will merge Figures 11 and 12 to one figure. We check out and if that is the case we may need to change figures numbers.

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.

Thank you. We will consider this.

Table 3: Can be merged with tab. 2.

Thank you. We will consider this.

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.

Thank you. We will consider this.

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.

Thank you, we will make corrections.