Response to Prof. Y. Shen (SC2)

General comments

Accurate assessment of soil water budget components (SWBCs) is necessary for improving irrigation strategies and optimizing the use of fertilizer in agricultural systems. However, quantitative information of SWBCs is usually challenging to obtain. Soil moisture is a variable that integrates the water balance components of land surface hydrology, and thus over time it can be used to develop a record of antecedent hydrologic fluxes. This paper presents an interest and important study on a soil moisture data-driven method for the water budget components estimation. Overall, the research was well conducted, and the whole manuscript was generally well written. I recommend publication consideration, granted that the minor concerns and questions below are properly addressed.

Response: We thank Prof. Shen for taking the time to review our manuscript and for his generally positive feedback on our study.

Specific comments

1) The abstract is concise and almost complete. The only suggestion is to clarify in line 27-28 that why the cumulative irrigation volumes varied so much among the 6 different plots, i.e., 652-1186mm. Although you mentioned that this phenomenon can be largely attributed to the plastic film mulching at the NT1 in your manuscript (Line 351-353), it should also be stressed in your abstract.

Response: Thanks for pointing this out. We will add and change in the abstract the following sentences for clarification: "Despite the relatively flat topography and consciously uniform irrigation, significant variances were observed among the film-mulched plots (NT2-6) in both the cumulative irrigation volumes (between 652.1 mm at NT3 and 867.3 mm at NT6) and deep drainages (between 170.7 mm at NT3 and 364.7 mm at NT6) during the growing season of 2016. Moreover, the unmulched plots (NT1) has remarkable higher values either in cumulative irrigation volumes (1186.5 mm) or in deep drainages (651.8 mm) compare with other plots."

2) Page 2, Line53-54, ". . .irrigating too early", I guess it should be "irrigating too much"?

Response: Yes, you are right, that was a careless typo. We apologize for this, and we will correct it as suggested in the revision.

3) Page 2, Line 69-70, the citation of the references [(Mcgowan and Williams, 1980) (Koksal et al., 2017)] was not correct due likely to font conversion.

Response: This citation will be corrected in the revision.

4) Page 3, Fig. 1c, the root systems of crop and alfalfa drawn by the authors are not good to indicate their real patterns in the soil profiles. In most cases, root distribution with depth is that of a negative exponential function, i.e., (Wasson et al., 2017).

Response: Thanks for the nice suggestion. We will reorganize this figure in the revision as suggested.

5) Page 5, Line 165-194, there are more than one method are available to do this calculation, i.e., enhanced soil water balance, slope approach (Guderle and Hildebrandt, 2015), what is the reason for choosing the Inverse Soil Water Flow Model in this paper?

Response: Very good question, and yes, there were more than one method available to do this calculation. According to Guderle and Hildebrandt (2015), the regression method based on diurnal fluctuation of soil water contents (M1) and the inverse method based on solving Richards equation (M2) were proven as the two most accuracy and reliable methods among the four tested ones. However, we found that although M1 performs well in the synthetic data generated with soil water flow model, it cannot be easily used in practice because it does not consider the hysteresis effect of soil moisture between different soil layers, which in turn can result in abnormal values of ET. Therefore, we finally chose the inverse model to estimate the evapotranspiration and slow drainage. We will clarify this point in our revision.

6) Page 6, Line 206, what kind of software? You mean the codes that you developed to do this calculation? You don't mention it throughout the manuscript. Please clarify this point.

Response: Yes, we did mean the codes that we developed to do this calculation. It seems this description does not make sense in the manuscript, so that we will remove the statement of software, and change it in the revision as "The drainage rate $q(n)$ assigned to the bottom node n was determined by the relationship as $q(n) = -K(h)$, where h is the local value of the pressure head and $K(h)$ is the hydraulic conductivity corresponding to this pressure head (Odofin *et al.*, 2012)."

7) Page 7, Line 246, the citation is not complete, please double check it.

Response: We will correct this in the coming revision.

8) Page 9, Line 287, why you start this part from NT6 rather than NT1, what is the logic behind this?

Response: Sorry for the confusion that brings to you and to the readers. To clarify the issue, we reorganized this part as: "The average field capacity value (θ_{fc}) of NT1-6 determined from laboratory measurement of soil water release curves was 19.2% (i.e., 20%, 17%, 18%, 19%, 22% and 19% for NT1-6 respectively). After 24 hours of the end of irrigation (June 3, 2016), the soil moisture values for the all the measured horizons (20-100 cm depth) of NT1-6 ranged between 8.9% and 16.9% (13.7-15.7%, 13.7-15.1%, 8.9-14.5%, 9.6-16.9%, 11.7-15.3% and 12.3-14.2% for NT1-6 respectively), lower than the field capacity (Fig.2&5), suggesting that the rapid drainage of water away from the root zone soil (0-100 cm) was terminated during the period, as expected."

9) Page 11, Line 378-381, you mentioned that one of the potential reasons that could result in the different irrigation rates is the mutation of the infiltration rate. If this is the case, how well do the TDR measurements-based estimation of irrigations represent the experimental plots? More related discussions are needed to clarify this point.

Response: Very constructive suggestion. Although the estimated average irrigation amount of the six experimental plots is well consistent with the actual average irrigation amount, we must agree that considerable uncertainties exist in the estimate of the irrigations. According to this suggestion and the comments from other reviewers, a new section (**4.5 Uncertainty analysis**) will be included in the revision to solve this concern.

10) Page 12, Line 416-417, "Information on SWBCs is crucial for irrigation planning at both the field and regional scale (Jalota and Arora, 2002), and the best estimates should be based on models of soil water, because direct measurements are not available in most cases", it is better to move this

sentence to the 2nd paragraph in section 4.4., i.e., Line 429.

Response: Thanks, we will move this statement into the 2nd paragraph of section (4.4) in the revision.

11) Page 15, Line 542; Page 17, Line 619, the references were not organized in the correct style of HESS.

Response: We will reorganize the references in the correct style of HESS.

References:

Guderle M, Hildebrandt A. 2015. Using measured soil water contents to estimate evapotranspiration and root water uptake profiles - a comparative study. Hydrology & Earth System Sciences, 19: 409-425.