

This study investigated the changes of soil physical properties, i.e. soil water repellency, soil compaction, soil water conditions, on crop yield under different tillage managements in a semi-arid region in China. Generally, the successive field experiment was conducted systematically and the soil properties was measurement carefully. The data was reliable and the statically analysis was done properly. The tables and figures are clear. The discussion is the weak part and need to be improved.

There are some concerns as follows:

- (1) The SWR was characterized by the RI, which was calculated from sorptivity of water and ethanol in an infiltration measurement in the current study. The concerns is does the same undisturbed soil core was used for water and ethanol infiltration. If the water and ethanol infiltration measurements were conducted on the same soil core, it is necessary to provide detailed information on which liquid was applied first and what treatment was done to the wetted soil before second liquid was applied (if the wetted soil was air-dried again?). I may guess two different cores were used for the infiltration measurements of water and ethanol, respectively. If so, the difference of the soil pore structure between the two soil cores was purely neglected when calculated the RI by comparing the S_e and S_w . This information should be provided clearly in the M&M part.
- (2) The contact angle was mentioned in the manuscript in Lines 206-207. I am wondering why the SWR did not characterized by the contact angle measurements.
- (3) Lines 179-187, what is the function of these sentences introducing the calculation of pressure head. It seems the pressure head parameter has no relation with the calculation of S_e and S_w .
- (4) Line 218 and 245, it is necessary to give the value of the particle density in the calculation of total porosity and air-filled porosity.
- (5) Line 252, it is necessary to provide the information of how the cumulative evapotranspiration was measured or calculated in the field for the further calculation of WUE in the current study, and the equation of WUE calculation should be provided.
- (6) Soil samples were taken in 0-5, 5-10 and 10-20 cm soil depth. Why the soil depth in Table 1 are 0-10, 10-20 and 20-30?
- (7) Lines 237-242, Here, it is said LLWR could be calculated based on SWR at air-filled porosity of 10% or field capacity, PR (2 MPa) or PWP (-1500 kPa). It should be clear which criteria was used for the LLWR calculation but no using "or". Line 238 and Line 241, what do you meaning by "smaller water content" and "higher water content"? It is quite confusing.
- (8) The biggest concern is that the discussion part is not concise and solid enough to make a good explanation for the large load of information provided by the measured data and statistically analysis. Many sentences is only a repetition of the results information, such as Lines 380-381, Lines 398-402, Lines 409-410, Lines 422-425, Lines 477-479.
- (9) Lines 386, it is ambiguous to use the "improvement of soil pore structure". What is good pore structure and what is bad pore structure?
- (10) Lines 396-397, actually, the SWR is only not linearly correlated with the content of SOC.
- (11) Lines 435-436, Lines 461-462, Another big concern is that preferential flow caused by SWR could increase the spatial variation of SWC. Some place would have higher SWC but at the same time, some place may have lower SWC. The results should be largely

depended on the sampling locations. It should not conclude that the preferential flow will increase the SWC in deeper depth.

- (12) Lines 439-440, it is said here that the crop straw mulching was used under no-tillage. I think this is the main reason for the significantly higher SWC in no-tillage plots shown in Fig. 2, but not the effects of SWR as stated in Lines 449-453.
- (13) Lines 455-457, it is too general and should be moved to the introduction part.
- (14) Lines 489-491, Lines 545-547, this long sentence is confusing.