Responses to Comments

We have listed the comments in black font and our responses in blue font.

Anonymous Referee #1

Thank you very much for your informative, valuable and useful comments, and also for your time and effort in improving our manuscript.

This paper is not suitable for publication.
The authors present once-off gravimetric soil moisture profiles, collected from three regions and within each region at several sites with differing histories of tree based agriculture, with a permanently cropped site as control. They claim to show a dried soil layer (1) develops in response to tree age (2) develops a turning point such that water content increases at a particular age (differing for different species) and that significant correlations were found between the depth of the zone of lowered water content and rooting depth. While some of these claims are not surprising none has been supported by a sound statistical analysis. Even if they had I doubt the content would be of sufficient interest to readers of HESS in its current form.

Reply: Thanks for your comprehensive and reasonable evaluation. A dried soil layer (DSL) has been found for 6 decades in the Loess Plateau of China. Due to the global change combined with the intensive impact of human activities, soil desiccation has attracted more attention in loess region since it may potentially affect many eco-hydrological processes. However, to our knowledge, very little information about the change of DSLs is available especially for deep soil profile. Under this circumstance, we have conducted the present work.

The authors claim this desiccation is problematic (use of terms worst, serious, deterioration etc. abound) yet trees apparently still get older and deeper rooted i.e. they are still growing and are apparently somewhat productive. I would be more compelled had productivity data been analysed as well. Despite the implications in the title very little plant physiological data is presented, other than age and an example of root weight by depth from one site. Furthermore, the absence of soil data or tests on this data is problematic. I do understand the nature of the Loess and its apparent homogeneity, having visited there recently, yet the lack of testing soil characteristics is yet another factor going against this paper. At one point the authors rely on this homogeneity and at another point claim differences soil properties and heterogeneous soil properties can explain some differences.

Reply: (i) We chose some improper words to describe the soil desiccation when writing the manuscript. But we surely know that soil desiccation is just a hydrological phenomenon in water-limited ecosystems. Its real influence on related eco-hydrological processes needs further and long-term research. At present, it is generally thought that DSL has a possibility of negatively

effects, based on some measured data in loess region.

(ii) We readily agree that the heterogeneous of soil properties should be taken into account. We used the pedotransfer function (PTF) for determining field capacity and permanent wilting point at each sampling soil layer, in revised manuscript. This would be a way to solve/conquer the variation of soil textures in the profile, which perplexed us for a long time.

Gravimetric water contents with depth and site may have different relationships with soil water matric potentials, usually used to define agricultural plant available water. This is an issue because it is unclear how gravimetric water contents were used to derive the numerous indices of water content. For example bulk density is used in equation 2 along with field capacity, permanent wilting point and some unknown term T. All of these terms would have to have been derived from some pedotransfer function presumably (which is not clearly cited or reproduced) particularly given the description of the sampling methodology (augering). I doubt such functions were developed for deep soil samples. Had matric potentials been used it might have been possible to calculate water flow directions and inform the hydrology better.

Reply: (i) We clarified this part in revised manuscript, and we used a pedotransfer function (which was selected from five established PTFs) to generate the profile values of bulk density, field capacity, and permanent wilting point at each sampling soil layer, by inputting the data of soil particle composition and soil organic carbon.

(ii) It is impractical to measure matric potential of the soil to a depth of 18 m, although the matric potential data has some advantages.

0.1 Statistical Analyses

The authors claim the status of soil moisture is a result of the agricultural practices. However, the rainfall time series at each site was not evaluated as to whether it could partly or wholly explain the observed soil moisture profiles. This is particularly relevant as claims of change points (also unsupported by any statistical test) may just as likely have been due to variation in rainfall / potential evaporation as any senescence in plant function.

Reply: We have the data but did not present it the manuscript, because these sites at each sub-region are all located in a small watershed, which has the same rainfall characteristics.

The regressions only show correlation coefficients and not any relationships. We have no idea of rooting depths and little idea of variation in root density with depth to gauge whether such strong correlations are meaningful in any way.

Reply: We have presented the vertical distribution of root data along 18 m soil profile, and added more root parameters (i.e., root diameter, length, etc.) in the revised

manuscript.

There was no consideration of potential site factors in the statistical analyses or ANOVA design.

Reply: We think that the impacts of those potential site factors are very small since each sampling site for a sub-region was selected based on a similar environmental condition.

There appeared to be no consideration of the lack of replicates in control sites within each region. While I would expect there to be an effect of changing agricultural practice and tree age with soil moisture status the methods presented here provide me with no confidence in the conclusions drawn.

Reply: In fact, to verify the change of DSL with age, the four sub-regions were deemed as the replicates, to some extent.

There is also no accounting for potential (/cross) correlation in soil moisture that one might expect due to regional correlations in rainfall.

Reply: This correlation was analyzed in the revised manuscript.

0.2 Grammar, spelling and presentation
This paper has numerous grammatical issues, however, the more significant issues with the science require greater attention at this stage.

Reply: We will invite a native English-speaking soil scientist to check the language and to improve the scientific arguments made within this manuscript.

0.3 Figures and Tables
It appears that one soil moisture time series has been repeated in Figure 2a and 2b yet supposed to represent different sites.

Reply: Yes. We used the same data of permanent farmland for two vegetation types, just because all the sampling sites for the two vegetation types are located in the same small watershed.

Based upon Figure 3 I doubt the presence of a "turning point".

Reply: This "turning point" was determined based on the ANOVA.

It is not clear from the text how the curves of field capacity were derived in Figure 5.

Reply: We used the pedotransfer function, which was selected from five established

PTFs, to generate the profile values of bulk density, field capacity, and permanent wilting point at each sampling soil layer, by taking the heterogeneous of soil properties into account.

0.4 In conclusion

Finally, I recommend the authors revisit their aims and objectives, particularly to address an apparent bias in their hypothesis that soil desiccation is necessarily a bad thing. Different species have different moisture regimes over which they are physiologically adapted. Advocating for reduced DSLs in dryland agriculture seems to have little relevance for improving agricultural productivity.

Reply: As suggested by you and the other two reviewers, we have revised our manuscript thoroughly. The aims and objectives of the manuscript were refined.