

Interactive comment on “Changes of deep soil desiccation with plant growth age in the Chinese Loess Plateau” by Y. Q. Wang et al.

Anonymous Referee #1

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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.

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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.

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.

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

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have been due to variation in rainfall / potential evaporation as any senescence in plant function.

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.

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

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.

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

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.

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.

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

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

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.

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