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Discussions

Interactive comment on "Preferential water flow through decayed root channels enhances soil water infiltration: Evaluation in distinct vegetation types under semi-arid conditions" *by* Gao-Lin Wu et al.

Anonymous Referee #2

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The manuscript compares the soil saturated infiltration capacity of different vegetation types (fruit tree, desert shrub and grassland) both in living and "degraded" state and relates the observed variation by size and area of root channels. The results indicate that degraded woody vegetation still provides for enhanced infiltration to deep layers due to root channels still providing for preferential flow to deep layers.

I think the topic is overall relevant, because the decrease of soil water content by introduced woody vegetation is a pressing issue in semiarid areas in general and within the Loess Plateau and surrounding region specifically. I agree that learning how the

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degraded landscape behaves hydrologically may be of importance for understanding potential means for restoration.

However, I find that the manuscript in the current form has substantial shortcomings and I recommend a very major revision of both the presentation and analysis. I think the introduction should give more background in the setting, e.g. the causes of vegetation degradation and depletion of the soil water storage. The methods need to be stated more clearly and the statistical analysis is not state of the art. The discussion is too narrow and e.g. leaves out some important alternative explanation of the observed patterns. Overall, the manuscript is premature for publication and I suggest rejection with invitation to re-submit.

General comments

Introduction

In the introduction the focal term of the manuscript "dried soil layers“ is introduced with a citation of Jipp et al., 1998. I find this citation misleading, as that reference focusses on the deeper soil layers whereas here the surface soil is of importance. Also, it would have been insightful to learn more about how slow depletion of soil water finally leads to vegetation degradation. A number of manuscripts and more recent ones have been published on this, e.g. on the Loess Plateau. This would also help to appreciate the relevance of the work. On the other hand, I am not sure how e.g. the "biomat bacterial layer“ refers to this work. I suggest strongly revising the introduction.

Methods

The experimental work is not well explained, and in the current state it does not allow evaluation of the results.

(1) Most importantly: What were the details of the selection of the measurement locations? How far from stems were the located? Were they randomly selected and what procedure was used for that? Were they (as Fig. 1 suggests) located on top of the

root collar of the perished plant in the decayed areas? If the latter was the case, the differences between living and decayed groups will have to be re-evaluated. This is because the infiltration reservoir would directly connect to the decaying rooting network of one precise stem, and the result is only representative for the exact former locations of the stem and not the more representative area surrounding the stems. âĂÍ(2) Using double ring infiltrometers for assessing infiltration capacity has the disadvantage that introducing the rings causes substantial disturbance which affects the soil properties. I assume the data are worth publishing regardless, but I expect this to be mentioned in the discussion, along with an evaluation why you believe the results can be interpreted regardless. Please give also more details of the infiltration measurement. How deep was the infiltrometer introduced to the ground and how?

(3) Soil water content was measured precisely where? From the methods section it sounds like it was assessed within the infiltration area, but I am note sure whether this makes sense? I am also not sure, why the soil water content is important for interpretation? Also why give the soil water content in gravimetric units, if the soil was collected in cylinders and volumetric soil water content could be assessed?

(4) The explanation of how root channel sizes and area are measured is unclear. Were the assessed within the infiltration area after the experiment or before? Why is the diameter of the stubbles required for root channel diameter, or are roots the origin are the "stubbles“?

(5) When were the measurements conducted? We learn that two different measurements campaigns were merged. Soil properties can change with time and therefore the length of the period between merged datasets is relevant.

(6) Some variables are presented but it is unclear why and how they are supposed to be interpreted. For example, how is the initial infiltration or soil water content of importance for the interpretation?

How were the sites managed? Is there grazing of animals are the fruit trees ap-

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proached for harvest? In other words, are there any land management procedures that affect soil compaction?

The statistical analysis is not state of the art. The overall goal of the manuscript is to reveal differences in infiltration capacity between the groups and explain them with supplemental information, like root channel diameter and bulk density. This would be a classical application for a statistical model, like ANOVA, mixed effects models or structural equation models. This would reduce the statements of the individual correlations and improve the interpretation.

Also, it looks like root channel diameter and root channel area are related? If yes, it is enough to present only one of them.

Results

The results section is extremely short.

It would have been nice to see boxplots of the root channel diameter, bulk density etc. for the different groups.

It is good to see charts of the infiltration rates over time, but this can only be one representative measurement ? It is not stated so in the figure description.

Figure 3: I think the initial infiltration rate is not required? There are no error bars for the steady infiltration rate - is the variation so small that it does not show up? It would be more insightful, to show the results as boxplots.

Figure 4: The figure description is missing. There must be an error with the units of the average root channel diameter, which ranges from 1 to 16 cm.

The reference to Fig. 5 is only given in the discussion (L 233-235), and should be shifted to results. I would expect that the infiltration depth depends also on how much water had to be infiltrated before the steady state was reached in each location. Also here a statistical model would be extremely useful, which can test the influence of

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several variables like vegetation type, vegetation state and total infiltration together on infiltration depth.

Discussion and Conclusion

Please add a discussion on the reliability of the methods, especially the infiltrometer, but also root channels.

I am surprised the authors do not refer or discuss alternative mechanisms to the role of root channel diameter and root area for enhancing infiltration capacity. Alternatively, bulk density (modulated by soil organic carbon or compaction if applicable) could potentially be related to infiltration capacity. Also, depending on the location of the infiltration areas and of Fig. 1 (see also comments above), the stem flow literature in semiarid areas may be relevant, like the double funneling in desert shrubs by Li et al (2009).

Detailed Comments

I think detailed comments would only be useful for a later version of the manuscript.

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