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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Variation of soil hydraulic properties with alpine grassland degradation in the Eastern Tibetan Plateau" *by* T. Pan et al.

T. Pan et al.

pantao@igsnrr.ac.cn

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Comment 1 In the Introduction section, the authors should substantially review the relevant studies in alpine mountainous regions, not just Tibetan Plateau of China. The main findings, discrepancies and weaknesses of previous studies and the motivations of this study should be addressed in detail. Reply: We appreciate and agree with the reviewer very much for the constructive comment. Indeed, we paid most attention to the Tibetan Plateau of China in the introduction section which was not a substantial review for relevant studies in alpine mountainous regions. In the revised manuscript, relevant studies in other similar alpine mountainous regions such as the south of Tibetan Plateau in Nepal, the Alps mountainous area in Europe, the high land in North China etc. were added in the introduction section. At the same time, we revised the descriptions of main findings, discrepancies and weaknesses of previous studies of

this study carefully in the revised manuscript. And hence the motivations of this study were addressed in detail. For specific revisions and changes, please see the revised manuscript appended.

Comment 2 The authors indicated that "large discrepancies still exist in the obtained conclusions and knowledge gap remains". However, in the Discussion section, the authors pointed out several times that most results of this study were consistent with previous studies, (such as P.7, Line 25, "in agreement with", P.8, Line 19, "is consistent with", P.8, Line 31, "The similar", P.9, Line 10, "consistent with"). What are the new and different findings of this study with respect to those in Tibetan Plateau of China, and more important other alpine mountainous regions in the world. What is the reason and mechanism for the differences? The authors should substantially address them and improve the highlights. Reply: We totally agree with the reviewer's comment. As the reviewer pointed out, our description in the introduction and discussion was a little bit contradictive and misleading. For the first "in agreement with" in P.7, Line 25, we would like to give a further explanation that here we mainly talked about the variation of basic soil properties including bulk density, soil organic carbon and etc., not soil hydraulic properties. As for other points the reviewer issued, we substantially compared our findings to previous studies in the revised manuscript and discussed the consistent and inconsistent results. Actually, there were several different findings comparing to existed studies. So the discussion section was carefully revised. We also avoided using suchlike misleading words in the discussion section. Thereafter we pointed out the reasons and mechanism for the different results and improved the highlights. For specific revisions and changes, please see the revised manuscript appended.

Comment 3 The authors only investigated the effects of soil properties on hydraulic properties. I think the role of vegetation characteristics including roots should be included in the analysis. The degradation changed both vegetation and soil characteristics to affect soil hydraulic properties. Reply: We thanked for the informative suggestion. In site selection, we fully considered and investigated the vegetation charac-

teristics of each degradation degrees, like coverage, biomass (both above and underground), species number and etc. We found soil organic carbon, bulk density and soil texture, especially those of the top soil, responded swiftly to the changes of vegetation characteristics and changed consistently with degradation degrees, so changes in soil properties partly contain the changes in vegetation characteristics. As the request of the reviewer, we addressed more about the effect of vegetation characteristics, especially the root activity on soil hydraulic properties carefully and tried to explained the mechanism in detail. For specific revisions and changes, please see the revised manuscript appended.

Comment 4 If the authors also measured soil moisture, it is necessary to compare soil water content among different degraded alpine grassland fields. Reply: Thanks a lot for the reviewers suggestion and reminding. Actually, we have measured soil moisture content of all investigated sites in the summer of 2014 from June 20th to July 20th. So as suggested by the reviewer, the comparison of soil moisture content among different degraded alpine grassland was added in the paper. For specific revisions and changes, please see the revised manuscript appended.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-333/hess-2016-333-AC1-supplement.zip

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