

Dear Editors and Reviewers:

Thank you for your letter and for the reviewers' comments concerning our manuscript entitled "Soil carbon fractions and enzyme activities under different vegetation types on the Loess Plateau of China". Those comments are all valuable and very helpful for revising and improving our paper, as well as the important guiding significance to our researches. We have studied comments carefully and have made correction which we hope meet with approval. Revised portion are marked in blue in the paper. The main corrections in the paper and the responds to the Editor's and reviewer's comments are as following:

**Editor's comments:**

**--The subject matter of the manuscript as well as the hypothesis has little novelty. It is well known that composition of SOC is related to enzymatic activity. It is obvious for the unaltered forest soil to be rich in C and enzyme activity. Moreover, paper entitled "Changes in soil nutrient and enzyme activities under different vegetations in the Loess Plateau area, Northwest China" Wang et al (2012) published in Catena 92 (2012) 186– 195 also describes similar work in the same region. Measuring SOC in top soil (up to 20 cm) for trees is not a sensible option. The author should analyse it for much deeper regions.**

**Response:** We thank the reviewer's appreciation about the subject and hypotheses of our manuscript. We admit that, forest soils are known to be a strong C sink (in terms of C accumulation) as compared to those of the forest steppe and grassland soils, while the C fractions, especially the labile parts, they might exhibit various patterns along vegetation types.

Compared to the work published by Wang et al (2012), the similar point is:

One work was carried out on the Loess Plateau of China, the soil parameters such as nutrients as well as enzyme activities were analyzed, as a way to compare the

difference between treatments.

However, the obvious differences lies in the two studies are:

1) The objective of the study — Wang et al., focused on evaluating the influence of land transformation (from former farmland to forestland) on the soil quality, after 3 decades. However, we emphasized on the relationship between soil C and enzyme activities along vegetation types.

2) In terms of soil C, Wang et al. only examined the content of soil organic matter, however, we investigated the four soil labile fractions (soil microbial biomass carbon, soil easily oxidized organic carbon and soil dissolved organic carbon), with a consideration of the essential role played by soil labile C in terms of soil nutrients turnover and enzyme activity regulation.

We tried our best to improve the manuscript and made some changes in the manuscript. These changes will not influence the content and framework of the paper.

We appreciate for Editors/Reviewers' warm work earnestly, and hope that the correction will meet with approval.

Once again, thank you very much for your comments and suggestions.

Thank you and best regards.

Yours sincerely,

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