1. All the results (soil, microbe and enzyme) may be simply a mixing effect. In other words, they are not affected by soil coarseness, but caused by the dilution of native soil (much higher CNP, microbe and enzyme) with river sand (much lower CNP, microbe and enzyme). The authors should report initial results right after mixing (baseline data), and then compare these data with the data measured in 2015. An appropriate way to interpret the "effect" of soil coarseness on soil properties should account for the effect of mixing.

Response: Thanks for the observation. We agree that dilution by river sand is an important factor of decreasing soil parameters (mentioned in Page 15 Line 16). However, soil coarseness itself is a process of incorporating sand into soil to cause a dilution effect as a result of wind erosion. Besides simply dilution, we also found that there were still biogeochemical processes happened by comparing theoretical dilution with measured parameters (Page 15 Line 20). This information has been mentioned in Page 15 Line 16-19. We did not collect baseline data at the beginning of the mixing which would be one defect of our experiment. We would expect that the baseline data might be similar to the theoretical dilution values where no biogeochemical interactions happened at the very beginning.

2. It is not clear how the authors calculate the "theoretical dilution".

Response: We have clarified the calculation of the "theoretical dilution" (Page 11 Line 4-10. It is weighted average of native soil and river sand for C and N contents as restively high C and N contents in the sand. For other soil parameters, the theoretical values were calculated based on 90% (C10 treatment), 70% (C30), 50% (C50), and 30% (C70) of the measured parameters in control soil (C10).

3. Any soil (and microbe, enzyme) associated with the "transplant" should be accounted for in the budget.

Response: Thanks so much for the observation. There is no soil associated with the transplant which was mentioned in Page 8 Line 5-7.

4. The experimental duration is very short. The soils were sampled in 2015, only one year after plant presence (by transplant in 2014). Moreover, the authors should provide a better description of the experimental design (with a timetable and few

photos for various stages of the site).

Response: We transplanted the plants in July of 2013. And soils were sampled 2 years after plant presence. We have clarified this in Materials and Methods section (Page 8 Line 4). We admit that the experimental duration is still short. We will keep monitoring the plant-soil system to compared short-term and long-term responses. As suggested, we have provided a timetable with a few photos for various stages of the site as Fig. 1.

5. The novelty and uniqueness of this study should be clearly presented.

Response: This has been stated at Page 6 Line 9-14 and Page 18 Line 11-17. Field experiments are strongly manipulated that natural gradients which make it easier to determine the influence caused by treatments by keeping others factors, such as climatic conditions, soil types and level of manipulations constant. Field experiments simulating desertification would help to better understand mechanisms of changes in soil properties as affected by soil coarseness.

6. Plant-related data (such as above- and belowground biomass, species composition) should be presented.

Response: Thanks for the suggestion. In this manuscript, we tested 3 hypothesis: 1) soil coarseness would decrease both soil C and N contents as well as their stocks across soil depths; 2) soil coarseness would decrease microbial C, N, and P as well as the activities of C-, N-, and P-cycling enzymes because of the significant decrease in SOM; 3) soil coarseness would increase soil microbial C and N limitation relative to P as P could be supplied through abiotic processes. To test them, we might think the current data would be enough. Thus, we will not present plant-related data here.

7. The three enzymes were assayed at different buffer pH (5.5, 6.0, 6.5), which is different than the soil pH (7.3). Most studies adjust the buffer pH to soil pH to make the results more reliable.

Response: The authors agree with the reviewer's comment. Buffering pH of enzyme assays to soil pH might be a way to determine enzymatic activities that is close to activities in field conditions. However, buffering the reaction system to the optimal pH (5.5, 6.0, 6.5) can obtain maximum enzymatic activities. Thus, we are measuring the maximum potential which is easier for researchers to compare the activities among various studies.

8. Soil pH (for all treatments and depths) should also be presented.

Response: As suggested by the Reviewer #1, all the data in subsoils has been deleted. Also, the soil pH data for all treatments and depths has been reported by Lü et al. (2016). Thus, we only cited his paper in terms of soil pH (Page 14 Line 1-2).

9. The authors should be careful in statements. For example, "our results also imply that expansion of desertified grassland ecosystems in dry regions of the world due to overgrazing and climate change might weaken the soil C sequestration potential and N retention capacity, which in turn lead to changes in grassland productivity and biodiversity in a long run." The results in this study say nothing about "soil C sequestration potential and N retention capacity", which need sophisticated studies using ^{13}C and ^{15}N isotope tracing.

Response: Thanks for the observation. We have rephrased "soil C sequestration potential and N retention capacity" into "soil C and N stocks" which can be supported by our data (Page 24 Line 19). The sentence now reads "Our results also imply that expansion of desertified grassland ecosystems in dry regions of the world due to overgrazing and climate change would decrease the soil C and N stocks, which in turn lead to changes in grassland productivity and biodiversity in a long run."