

Interactive comment on “Alteration of carbon, nitrogen, and phosphorus stoichiometry and their related enzymes as affected by increased soil coarseness” by Ruzhen Wang et al.

Anonymous Referee #2

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General Comments Wang et al. present an investigation of the impact of soil coarseness on the soil nutrient cycles and microbial activity using a novel field experiment that mixed varying amounts of river sand into native soils. The approach is interesting and the work addresses important research questions regarding the impacts of desertification on plant productivity and soil C sequestration. However, the manuscript needs substantial improvements in presentation and analysis before it is suitable for consideration for publication in Biogeosciences. Additions are needed to the discussion of experimental design and assumptions, water limitation, theoretical dilutions, and implications of the results. However, the methods could be shortened by omitting details where a reference is cited for a technique, and the results could be shortened by high-

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lighting interesting results and their implications rather than describing every test done on every treatment. A thorough editing for English grammar and usage is needed. I've chosen not to provide technical comments because errors are so frequent.

Specific Comments The authors should justify why an experimental approach is needed to better understand the impacts of desertification and why their design is a realistic representation of soil change observed in natural settings. What are the limitations of existing natural gradient or long-term monitoring studies? Do the amendments made represent the range of variability observed in desertified sites? Is it realistic to transplant vegetation of the same composition as a native community to the treated sites, as vegetation would change along with the soil in a naturally desertifying site?

The experiment was conducted at an arid (450 mm MAP) site, but there is no discussion of water limitation of soil processes or even the precipitation patterns observed during the study. The results of the study could have been very different if it had been conducted during a relatively dry or relatively wet period. Soil moisture data would be ideal, but a simple soil water balance model might help to form a discussion of these issues and the differences between treatments in water holding capacity. It is entirely possible that nutrient limitation is rare in these soils and differences in microbial and enzymatic activity between soil coarseness levels is driven by soil moisture differences. Additionally, it would be useful to provide data, if available, on how soils outside the treatment area changed during the study as this will reflect the climate during the period.

The methods for developing theoretical dilutions for comparison with measured values need to be explained more clearly. As I understand it, the theoretical dilution value for, as an example, SOC content in a 50% amendment plot is simply 50% of the measured SOC in the control. This seems completely wrong and oversimplified, because the added sand contains SOC (see pg. 7 line 8). The theoretical dilution should be at least a weighted average the native soil and added sand or perhaps something more detailed based on theoretical relationships between soil texture and properties. It is unclear how

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theoretical dilution comparisons serve to test the hypotheses in the manuscript, so an overall better description of the objectives of this method needs to be provided.

Throughout the manuscript, there needs to be a stronger connection between the analyses performed and the hypotheses tested. There is a lot of listing of results in terms of things like enzyme activity and microbial biomass carbon, and while the connection to larger issues such as nutrient limitation is explained elsewhere in the paper (perhaps five pages previously), reading and understanding the results in the context of broader implications is an onerous task.

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