

## ***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 #1**

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“Alteration of carbon, nitrogen, and phosphorus stoichiometry and their related enzymes as affected by increased soil coarseness”

General comments The paper of Wang and co-authors deals with the effects of increasing soil coarseness achieved via in situ admixture of different sand amounts to the soil in semi-arid grassland. Authors measured elements' composition, microbial biomass, activities of three extracellular enzymes and estimated stoichiometrical relationships of the latter. Additionally, authors proposed comparison of the measured data with the respective theoretical decrease of parameters due to the dilution with the sand. A field manipulation experiment is important for understanding mechanisms caused by desertification, which has a direct application needs and therefore

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interesting for the broad scientific community. However, the integrated set-up and methodological flaws or the lack of detailed explanation, together with the strong need of the writing improvement prevent this paper to be accepted in its current state. Below authors find general comments while specific recommendations for the paper improvement and technical corrections are incorporated directly in the draft file attached. First of all, the following methodological issues are not clear and may affect the final outcome/conclusions: “ There is no control of original soil without any (excessive) manipulations. The latter included withdrawal of soil down to 60 cm, thorough mixing with sand (strong altering of the soil structure per se), removal of the topsoil with the heating up to 105 °C and the subsequent return, transplanting of vegetation. It remained unclear to which extend the used control (C0) with no sand addition was subjected to the listed manipulations. “ A key parameter for a desertification model study and a semi-arid grassland as such, the soil moisture dynamics is not shown or even mentioned. With all the reported soil characteristics, a basic one – the water content, WHC – is not shown. However, availability of water may strongly vary among soil coarseness gradient and, obviously, affect the majority parameters of interest as microbial biomass and activity, pH, nutrients mobilization etc. “ The effects of added sand. On a mass-basis, relatively low contents of C, N and especially P (not reported in the study) could in fact substantially influence the soil elements stoichiometry, and, critically important, the microbial community structure. This was not clarified in the methods and results, or in discussion. “ Secondary but still important methodological issue: some soil samples were frozen for storage purpose and the enzyme activity was measured upon unfreezing. In the draft file, I mention several papers (but based also on my own experience), the freezing/unfreezing could strongly affect enzyme activities and the direction of change is difficult to predict. So, in fact, authors had an additional treatment for the enzyme activities distribution along coarseness gradient. Most important, the frozen soil samples were not identified as the fresh soil was also used for the analysis. “ Finally, the “theoretical dilution approach” should probably be reconsidered. According

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to authors, the "dilution" occurred for the very initial soil properties, which existed at the moment of excavation and mixing with sand in respective treatments. However, after replanting of sites, additional C was introduced which was not accounted in the "dilution". Thus, depending on an amount of "new C", the theoretical dilution values could be higher as the currently presented and probably approach some of the "actual" measured parameters. From this point of view, comparison of "theoretical" and "actual" values could be erroneous. Authors could estimate how much C, N, and especially microbial biomass was introduced with the replanting and correct the dilution (CO at the same time will not change). There are other shortcomings, such as a too detailed results section (all the observations are excessively described) as well as speculative and controversial statements in the discussion. Upon clarification of unclear parts, amendment of Mat&Meth section and results (water content) with the improvement of discussion, the manuscript could be reconsidered for the publication in Biogeosciences.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-483/bg-2016-483-RC1-supplement.pdf>

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