

General comments:

1. 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.

Response: We fully agree with the reviewer's comments. This is actually the defect of our experiment without setting undisturbed soil (without withdrawal and heating) as control of original soil. Still, we can isolate soil coarseness effects because all the soils from C0 to C70 plots subjected to the same manipulations of excavation and sterilization. By knowing this, we have planned to enclose a new adjacent plot to serve as control of original soil in this field experiment. The absolute control would be good for our future research conducted in the experiment site. We have clarified in methods section that the current control soil were subjected to the same manipulation as listed (Page 7 Line 22).

2. 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.

Response: Thanks for the great observation. Indeed, soil moisture and water holding capacity are essential parameters for semi-arid ecosystem. Thus, we have added data of soil moisture and WHC in results section (Page 12 Line 7-12) and discussed their potential effects on soil enzyme activities (Page 21 Line 12-17). For soil moisture, the data were only available for the time of soil sampling. Considering the advice provided, we have decided to measure soil moisture dynamics during the whole growing season in our future work.

3. The effect of sand addition. 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.

Response: We agree with this observation. The river sand contains $1.29 \text{ g kg}^{-1} \text{ C}$ and $0.15 \text{ g kg}^{-1} \text{ N}$ which are 31.9% of SOC and 31.6% of TN for the original untreated soil. Due to the fact that the C and N contents in the sand are not negligible, we recalculated the theoretical values by considering C and N from the sand. This has been mentioned in Page 11 Line 7-11 in methods section and Page 15 Line 6-9 in result section.

4. Secondary but still important methodological issue: some soil samples were frozen for storage purpose and the enzyme activity was measured upon freezing. In the draft file, I mention several papers (but based 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 among coarseness gradient. Most important, the frozen soil samples were not identified as the fresh soil was also used for the analysis.

Response: Thanks so much for the reviewer's observation and we fully agree with this. We did not compare the difference in enzyme activities between fresh soils and frozen soils, which is a defect of our study. In the reference that were provided by reviewer, β -glucosidase and acid phosphomonoesterase activities were also determined in their study from different soils, but the activities showed no response to freezing-thawing cycles in the laboratory experiment (Daou et al., 2016, Soil Biology & Biochemistry 93, 142-149). This was explained in detail in the following specific comments (comment on Page 9 Line 16). But we are not sure if the activities still remain stable in wider soil and ecosystem types. Thus, we will follow the reviewer's advice of determining enzyme activities on fresh soils or clarify the effect of freezing-thawing process during storage on enzyme activities for in future research.

5. Finally, the "theoretical dilution approach" should probably be reconsidered. According 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 would be higher as 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 (C0 at the same time will not change).

Response: Before plant transplantation, the root of plants was washed to get rid of the soils associated with transplantation. This information has been added in Page 8 Line 7-8. In this case, replanting would not introduce C, N and microbial biomass to native soils. However, we have realized that the river sand contain non-negligible C and N which could comprise 31.9% of SOC and 31.6% of TN. We recalculated theoretical values of SOC and TN by considering the C and N contents in the river sand. Please see the details in the specific comments (comment on Page 11 Line 7-11).

6. There are other shortcomings, such as too detailed results section (all the observations are excessively described) as well as speculative and controversial statements in the discussion.

Response: Thanks so much for pointing out these. We have shorted the results section to avoid excessive description. Please see this in the specific comments (comment on Page 11 Line 8, Page 11 Line 10, Page 11 Line 7-22, and Page 12 Line 1-12). The speculative and controversial statements that were pointed in the specific comments were also addressed (see comments on Page 14 Line 21, Page 15 Line 2, Page 15 Line 10, Page 16 Line 2-3, Page 16 Line 7, Page 17 Line 18, etc.).

Specific comments:

Page 1 Line 1: The title is somewhat misleading: the elements’ stoichiometry was actually not measured in the study. Next, “their related enzymes” may be understood as sets or groups of enzyme catalyzing soil C, N and P turnover; however, just three enzymes were tested. I recommend to adjust the title to better reflect the outcome.

Response: We agree with the reviewer's comment. Thus, the title has been modified to "Alteration of soil carbon and nitrogen pools and enzyme activities as affected by increased soil coarseness".

Page 2 Line 2: added "subjected to desertification" after "grasslands".

Response: These words have been added. It now reads "Soil coarseness decreases ecosystem productivity, ecosystem carbon and nitrogen stocks, and soil nutrient contents in sandy grasslands subjected to desertification".

Page 2 Line 6: % of what? Natural (initial) sand content? Please, specify.

Response: It's percentage of soil mass by keeping total mass of admixture constant. To be specified, this has been rephrased into "a field experiment was conducted by mixing native soil with river sand in different mass proportions: 0%, 10%, 30%, 50%, and 70% sand addition".

Page 2 Line 7: Over which period of time? Please, add here briefly.

Response: We have added the period of time. It's "after 4-year mixture or 2-year plant transplantation" (Page 2 Line 7).

Page 2 Line 10: change "by up to" into "down to".

Response: This has been changed in Page 2 Line 10.

Page 2 Line 12: Not clear why exactly these enzymes were chosen?

Response: These three enzymes are commonly measured to represent microbial acquisition of assimilable C, N and P from recalcitrant organic matter (Waring et al., 2014). Related information has been added in Page 2 Line 12 to clarify the reason.

Page 2 Line 15: The enzyme catalyzes decomposition of chitin in soil, which is also a source of C, not just N, for microorganisms. How C from chitin was accounted in stoichiometry to N and P?

Response: The reviewer makes a great observation. The enzyme of N-acetyl-glucosaminidase (NAG) plays a role in the degradation of chitin and other β -1,4-linked glucosamine polymers (Sinsabaugh et al., 2008). It was selected to represent N-acquiring enzyme according to the studies from Sinsabaugh et al. (2008) (Ecology Letters, 11: 1252-1264) and Sinsabaugh et al. (2009) (Nature, 462: 795-798).

Page 2 Line 20: Authors stated they conducted field study. So, they should be aware of the effect of sand additions on plants. Next, microbial immobilization is a time-dependent process. Upon re-mobilization and MB turnover, the MBP should also be plant available. So, in fact, P immobilization in the medium- and long-term should be positive for plant supply with P. Please, explain better the main message here.

Response: Thank you for this observation. We have corrected the main message here into ‘Phosphorus immobilization in microbial biomass might alleviate plant P limitation in nutrient-poor grassland ecosystem as affected by soil coarseness’ (see Page 2, Line 21). And we also corrected this in the text into ‘In this case, plant P limitation might be alleviated due to microbial P immobilization, because microbial biomass turnover and P re-mobilization from MBP would make more P available for plant in the medium and long term’ (see Page 23 Line 5-8).

Page 2 Line 21: The conclusion is too simplistic and elusive. Authors should specifically highlight the key finding, relate it to a mechanism and/or explain its ecological relevance.

Response: Thank you for pointing out this issue. The conclusion has been stated as ‘Soil coarseness is an essential process of decreasing soil C and N storage and enhancing microbial C and N limitation relative to P, which would potentially pose a threat to plant productivity in sandy grasslands suffering from desertification’ (see Page 3 Line 1-3).

Page 4 Line 19: Does it mean it can not be higher? If not, please rephrase.

Response: Thank you for this observation. It has been rephrased to read ‘Microbial biomass generally comprises 1-4% of soil organic C’ (see Page 4 Line 19).

Page 5 Line 5: ‘environments’ is not necessary.

Response: This has been deleted in Page 5 Line 6.

Page 5 Line 6: ‘this kind of essential microbial function’ is not clear, please rephrase.

Response: This has been rephrased to ‘microbial mineralization of SOM’ see Page 5 Line 6-7.

Page 5 Line 9: unnecessary words.

Response: These words have been deleted (Page 5 Line 10).

Page 5 Line 10: In which soil/ecosystem? Why exactly these enzymes? For instance, peptidases also reflect the N turnover, several key enzyme are catalyzing decomposition of cellulose and hemicellulose (main components of plant residues), i.e. cellulose, xylosidase, alfa-glucosidase, etc.

Response: The authors appreciate and fully agree this observation. Indeed, peptidases (e.g. aminopeptidase), cellulase and hemicellulase are also key enzymes for N and C turnover. The authors think the selected enzymes of β -glucosidase (BG), NAG and acid phosphomonoesterase (AP) are commonly measured enzymes (Moorhead et al., 2012; Sinsabaugh et al., 2008, 2009, 2011; Waring et al., 2014). Also, these three enzymes catalyze rate-limiting steps of C, N and P turnover which supply assailable substrates of β -glucose, amino sugars and phosphate to microorganisms (Waring et al., 2014). Thus, we chose to determine the activities of BG, NAG and AP at the beginning of our study. For sure, we will determine the activities of peptidases, cellulase, hemicellulase and other essential enzymes in our future work. Also, we have added the soils/ecosystems in Page 5 Line 11.

Page 5 Line 16: replace 'researches concerning' by 'studies on'

Response: This has been replaced; please see Page 5 Line 18.

Page 6 Line 22: The initial properties of the soil must be included, especially texture, bulk density, C-org, nutrients.

Response: Thanks so much for the observation. The information has been added in Page 7 Line 6-7.

Page 7 Line 4: change 'simulated' into 'simulate'

Response: This has been corrected in Page 7 Line 12.

Page 7 Line 6: How did the bulk density change? Texture? Aggregates structure and stability? Seemingly, some protected (occluded) OM became available due to destructive manipulations. Was this accounted for at later stages? Another important and lacking here information, how the soil moisture changed with the coarseness?

Response: The bulk density significantly increased in C10 treatment as compared to control. For soil texture and aggregate structure, the soil became sandier after sand addition, because the soil particle of size smaller than 0.25 mm (< 0.25 mm, or called

soil microaggregate) decreased with the increasing coarseness density (please see Lü et al., 2016, Solid Earth 7: 549-556). This important information has also been mentioned in Page 17 Line 15. Indeed, destructive manipulation would make occluded SOM more available to microbial degradation, and this has been mentioned in the text (see Page 18 Line 4-6). Also, the information of soil moisture and water holding capacity has been added (Page 12 Line 7-9) and discussed in Page 20 Line 19.

Page 7 Line 8: So, this should mean the total mass (and volume) increase, correct?

Response: Thanks for this observation. But this is not the case. We kept the total mass constant and added different mass proportions of sand. This has been clarified in Page 7 Line 15.

Page 7 Line 8: -ed (Past Tense).

Response: This has been corrected in Page 7 Line 17.

Page 7 Line 10: 'soils of 0-5 cm depth were taken out from all plots...'. Was this done in the control? How much of SOM was lost due to such a manipulation?

Response: Yes, this manipulation was also done in the control soil to keep all the conditions constant across treatments, except for sand mass proportion (Page 7 Line 21-22). However, we did not quantify the loss of SOM as compared to untreated soil right after we did the sterilization. Due to the fact that all soils with and without sand addition were autoclaved at 105 °C, this would not influence our interpretation of the data. Thanks so much for this observation.

Page 7 Line 15-16: replace 'quadrat' by 'area', and replace 'at August' by 'annually (in August)'.

Response: These have been corrected in Page 8 Lines 8-10.

Page 7 Line 20: What was the moisture level between different plots?

Response: For C0 to C70, soil moisture decreased from 10.6% to 6.8% with the increasing intensity of soil coarseness. Please see Page 12 Line 7.

Page 8 Line 1: I guess 'in soils'

Response: This has been corrected in Page 8 Line 17.

Page 9 Line 3: replace 'extracted' by 'amended'

Response: Thanks for the observation. This detailed description has been deleted

as suggested by Reviewer 2 to omit details where a reference was cited for a technique (Page 9 Line 16-22).

Page 9 Line 16: This is not good for comparing the enzyme activities. To my own experience, as well as here (Sorensen et al., 2016. Biogeochemistry, 128, 141-154) and here (Daou et al., 2016. Soil Biology & Biochemistry, 93, 142-149) the freezing affects the enzyme activities and should be considered as a treatment. Therefore, authors should clearly define frozen samples and distinguish them from the fresh (unfrozen) soil. All the observed effects should also be considered through the prism of an additional freezing treatment.

Response: Thanks so much for this constructive suggestion. When we determined the enzyme activities, we actually did not compared activities from frozen soil samples with the fresh ones. Because we froze all the sampled soils in the laboratory for later enzyme assays according to Allison et al. (2009, Soil Biology & Biochemistry, 41, 293-302) who determined β -glucosidase, N-acetyl-glucosaminidase and acid phosphatase, and Creamer et al. (2013, Biogeochemistry, 113, 307-321) who determined N-acetyl-glucosaminidase. Indeed, freezing-thawing cycles would affect activities of some of enzymes. However, according to Daou et al. (2016, Soil Biology & Biochemistry, 93, 142-149), freezing-thawing cycles had no or a weakly significant effect on activities of β -glucosidase and acid phosphomonoesterase of different soils, who simulated freezing-thawing cycles in the laboratory condition. Thanks for the reviewer's observation and we will definitely consider the effects of laboratory storage (e.g. freezing) on soil enzyme activities in our future work. We have rephrased "fresh soil" into "frozen and field moist soil" to be more accurate (Page 10 Line 10).

Page 10 Line 6: -ed (Past Tense)

Response: The 'express' has been replaced by 'expressed' in Page 10 Line 22.

Page 10 Line 10: This is actually not fully correct. The thing is, 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 (how much?) which was not accounted in the "dilution". Depending on an amount of "new C", the theoretical dilution values could be higher

as the current 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 (C0 at the same time will not change).

Response: Thanks so much for the observation. Before replanting, we washed the plant roots to get rid of the soils associated with transplantation (described in Page 8 Line 5-7). In this case, there would be no additional C, N and microbial biomass was introduced with replanting. Due to the fact that C and N contents in the sand was not negligible, we have corrected the theoretical value based on mass proportions of sand and native soil by considering total C and N concentrations in river sand (please see Page 11 Line 4-8, Page 14 Line 16, and Figure 3a,b).

Page 11 Line 6: delete ‘decreased’

Response: This has been deleted in Page 12 Line 3.

Page 11 Line 8: Please, describe only the significant differences (nonsignificant – only those which are really key findings). In such a case, there is no need to use the word “significant” (provide respective references to statistical data, where necessary).

Response: Thanks so much for the suggestion. We checked throughout the manuscript to make sure describe only the significant differences and only nonsignificant results which are really key findings. Also, we deleted the word “significant” and “significantly” and provided respective references to statistical data in the Result section.

Page 11 Line 10: Not necessary, if the above mentioned prerequisite is met.

Response: This has been deleted (Page 12 Line 12). And the above mentioned prerequisite has been met throughout the manuscript.

Page 11 Line 7-22: The whole paragraph should be substantially shortened: there is no need to describe every single difference and number. Leave only key message (result).

Response: This paragraph has been shortened and some of the numbers have been deleted (Page 12 Line 10-20).

Page 12 Line 1-12: Same as above: too detailed description and too much

text/numbers. Enough to say that addition of sand decreased SOC content and stocks as well as TN by these many percent from the control (initial?).

Response: Thanks for the suggestion. We have shortened the text and deleted some of the numbers (Page 12 Line 10-20).

Page 13 Line 14: Many studied parameters could depend on a soil moisture dynamics in sites with increasing sand concentrations. Is such information available? Without such a key background, all other described parameters look secondary (or may directly result from a change in moisture). The same is true for the temperature regime if it changed due to the treatments.

Response: We have soil moisture data when sampling the soils (presented in Page 12 Line 5-8). Also, we have added the information of soil water holding capacity (Page 12 Line 8-9). The information has also been discussed in Page 20 Line 19.

Page 14 Line 9: Please, refer to my comment in the section “Statistical analyses”. The values reported here could vary after the respective correction for the “new C” (from replanting).

Response: We added description of the new calculation in “Statistical analyses” (Page 11 Line 4-10). As mentioned above, there is no new C from replanting.

Page 14 Line 21: “faster decrease”—the statement is misleading: there were no measurements conducted in dynamics. Please, rephrase.

Response: We have rephrased the statement. And it now reads “For microbial biomass C, N, and P, theoretical dilution showed more decrease of these parameters as compared to measurements” (Page 15 Line 8-9).

Page 15 Line 2: “decreased faster”—same as above: rephrase.

Response: This has been corrected and it now reads “However, the acid PME activity showed more decrease in field plots than that of theoretical dilution in C50 and C70 treatments” (Page 15 Line 11-13).

Page 15 Line 6: delete “in values” and replace “laboratory measurements” by “measure parameters”.

Response: These have been corrected, and it now reads “The difference between theoretical dilution and measured parameters”.

Page 15 Line 9: Actually, authors should know this. As the sand admixture was done on a weight basis and the C, N contents were measured, so the calculation is rather simple. Please, do this and state more exactly.

Response: We have done the calculation again by considering the total C and N contents in river sand (Page 11 Line 4-8, Page 14 Line 17-20), and interpreted the new data in Page 16 Line 1-5.

Page 15 Line 10: “might”—This means a very low probability of the observation/event. However, indeed, litter inputs and decomposition increase SOC and TN. Authors, should trust their findings!

Response: Thanks for this advice. After calculation the theoretical C and N contents, we have rewritten this paragraph. Please see Page 16 Line 1-5.

Page 15 Line 11-13: Replace “plan” into “plant”, and “was” into “is”. Add “as compared with the theoretical dilution”. Delete “as their mobility”.

Response: We have replaced “plan” into “plant” and changed “was” into “is” in Page 16 Line 1. And “as compared with the theoretical dilution” has been added in Page 16 Line 6. “as their mobility” has been deleted in Page 16 Line 6.

Page 15 Line 15: Actually, authors should be able to prove this, as they collected soil samples from three soil depths. However, it is not clear, why DOC and other parameters were not measured in deeper sampled layers...

Response: Thanks so much for the reviewer’s observation. We did not measure enzyme activities in deeper soil layers because microbial activities in surface soil are more important in nutrient cycling than that in subsoils and that would be labor intensity measuring all soil layers. Correspondingly, we did not measure available C, N and P concentrations in subsoils which are essential for microbial secretion of extracellular enzymes. However, we would like to consider this suggestion in our future work when doing researches on C and nutrient transportations across soil profile.

Page 15 Line 16: delete “which resulted in higher measured values than theoretical dilution”

Response: These words have been deleted in Page 16 Line 10.

Page 15 Line 18: Change “was not the case for” into “was not accounted for the”.

Response: This has been changed in Page 16 Line 12.

Page 15 Line 20: “soil physiochemical properties”— Exactly! Such as moisture regime...

Response: Thanks for the observation. And the information of soil moisture has been added in Page 12 Line 7-8, Page 20 Line 19.

Page 16 Line 2-3: Well, it is not that straightforward, especially based on the mentioned complexity of the field conditions. Another point, sand as such contained native microbial populations. As the sand was not sterile, they obviously were combined with the native soil microorganism. It is difficult to predict their fate, but under the conditions of higher nutrients supply and plant-derived deposits, they could proliferate and contribute to the less pronounced decrease with the coarseness (Fig. 2d) as compared with the theoretical dilution.

Response: Thanks so much for the suggestion. Even though the sand was not sterilized, the mixture of sand and native soil was sterilized in August 2012 (Fig. 1b). According to your advice, we have rephrased the sentence to make it straightforward (Page 16 Line 18-20).

Page 16 Line 6: I doubt they were conducted on the same site, so in the studied soil, number of other related parameters could change the picture.

Response: Yes, they were not conducted on the same site. And we agree with the reviewer that number of other related parameters could change the picture. Thus, we deleted this sentence (Page 17 Line 1-2).

Page 16 Line 7: There is no need to speculate: it could be other way round. If not measured, then omit as speculation.

Response: Soil bacteria were actually not measured in this study and this sentence was speculation. In this case, we followed the reviewer’s advice to omit the speculation. This statement has been deleted in Page 17 Line 2-4.

Page 16 Line 18: “the decline of soil fine particles”—Was this really decline or dilution?

Response: It was a decrease caused by dilution. And this has been rephrased in

Page 17 Line 15 to read as “We previously found the decrease of soil fine particles mainly as a result of sand dilution during simulated soil coarseness in this field plot”.

Page 16 Line 20: “decline”—Better to use deterioration, degradation...

Response: It has been replaced by “deterioration” (Page 17 Line 18).

Page 17 Line 1: Repetition of the statement 4 lines above. Omit or combine both sentences.

Response: Thanks. The repetition part has been deleted, and it now reads “Moreover, loss of SOM could result from limited stabilizing effects of mineral associations after soil coarseness” (Page 17 Line 21).

Page 17 Line 4: Again, this is just a repetition. Omit.

Response: This has been deleted (Page 18 Line 1-3).

Page 17 Line 8: as compared to what?

Response: As compared to later desertification stages (Page 18 Line 8), the loss of C and N was greater in light and moderate stages.

Page 17 Line: This is not exactly true! Of course, manipulations allow to distinguish between different effects, but probably authors were so far very lucky that they could “precisely control” and usually got “clear trends”. I am afraid, authors confusing field and lab experiments. However, it is indeed worth to compare natural gradient studies and the current manipulation experiment in terms of potential (and actual) drawbacks of the latter. For instance, the level of manipulation such as digging the whole soil out, mixing (destroying of the natural structure), sterilization with heat etc., all these are not common for the natural desertification. Still, we need strongly manipulated experiments to better understand factors/mechanisms. So, I would like to encourage authors to look onto their results with criticism and caution when relating to natural pristine ecosystems.

Response: Thanks so much for this suggestion and it’s really constructive.

According to reviewer’s comment, we deleted original statement and described the difference between field manipulations and natural pristine ecosystems. Also, we emphasized the necessity to conduct field manipulation experiments and the caution when relating them to investigations from natural pristine ecosystems (Page 18 Line

11-17).

Page 17 Line 18: This is misleading: reduction of C stocks due to dilution with sand does not necessarily mean there should be an increased transfer to the atmosphere. Why? The opposite could be hypothesized: with the increasing mineral component (sand) the total surface of minerals should increase and absorb (stabilize) available C. So, in the long-term the C storage capacity should increase.

Response: We agree with the reviewer's point that decrease of C stocks due to dilution does not mean there should be an increased transfer to the atmosphere. In this case, we deleted the original statement in Page 18 Line 19-22.

Page 17 Line 22: "decrease faster"—This is misleading: authors did not measure parameters in dynamics. Rewrite or omit as speculation.

Response: We have reworded this, and it now reads "Our results indicated that soil N stocks of surface soil decreased more than that of soil C" (Page 19 Line 1-2).

Page 18 Line 1: replace "with" by "the"

Response: This has been corrected in Page 19 Line 2.

Page 18 Line 4: The sentence is confusing: "this" means the mentioned study of Zhou et al., or the current study? Next, the explanation of the difference between C and N stocks decrease is not clear at all. Clarify!

Response: Thanks for the observation. "this" means the current study and it has been corrected in Page 19 Line 6. The difference between C and N stocks decrease was clarified in Page 12 Line 15-16 where "Across all soil coarseness intensities in surface soils, soil C and N stocks decreased by as much as 31.8% and 54.0%, respectively". This information has also been added in Page 19 Line 2.

Page 18 Line 7: "might"—this means such a probability is very low, so maybe no need to pay attention to this problem?

Response: As suggested by our plant productivity measurements, soil coarseness indeed decreased plant biomass (but data were not shown in this manuscript). Thus, we rephrased "might" into "would" (Page 19 Line 9).

Page 18 Line 14: Please, add here "discussed below (section 4.4)". Otherwise, there is an impression authors "forgot" about P while discussing C and N in microbial

biomass.

Response: Thanks for mentioning this and related information has been added. It now reads “while the increase of MBP under soil coarseness as discussed below (section 4.4) was not expected” (Page 19 Line 16).

Page 18 Line 19: Flawy statement: it is not possible to inhibit MBC (MBN).

Inhibition occurs for the living organisms. If they die then MBC (MBN) may decrease.

Response: This has been rephrased to “decreasing MBC and MBN” (Page 19 Line 20).

Page 18 Line 19: Do authors mean the “synthesis” or properties of enzymes as chemical compounds/molecules? This difference is important.

Response: It is “synthesis”. This sentence has been rewritten as “factors directly or indirectly decreasing MBC and MBN would also suppress the synthesis of extracellular enzymes of BG, NAG and PME” (Page 19 Line 20).

Page 18 Line 21: delete “element”, and replace “microbial biomass production” by “microorganisms”.

Response: These have been corrected and it now reads “Soil C is essential for microorganism” (Page 20 Line 1).

Page 19 Line 17: The reference is inappropriate: in the referred study, i.e. Wang et al. (2015), term “desiccation” is mentioned just once and with the reference to the study of Zhang et al. (2013). However, there are number of studies about the drought/drying effects on soil microbial populations. So, authors should refer to proper studies over the topic.

Response: Thanks for the observation. We have changed the reference into Alster et al. (2013) which studies drought effect on soil enzyme activities in a grassland ecosystem (Page 20 Line 19).

Page 20 Line 7: “salinity from sand”—This is not clear: the sand was a river sand, so why the salinity should increase? Did authors measure concentrations of salts? Please, explain.

Response: We are sorry that it should be “the increase of alkalinity from sand

addition” (Page 21 Line 14). Because the pH of river sand was 7.5 (Page 7 Line 18), and sand addition increased soil pH from 6.7 (C0) to 7.3 (C70) (Page 14 Line 1).

Page 20 Line 9: This is too simplistic and misleading statement. Different soils have different microbial community structure and behind the terms “fungi” and “bacteria” are enormous number of species, which certainly have specific ecological niches. What is optimal for one group of fungi at any case similar for the other. The same is for the bacteria. But most importantly, the referred studies do not supported the statement of authors. Thus, in the study of Bååth (1998) the contrasting information is reported: “Thus, soil pH appears to have no direct effect on the number of CFUs over the pH range studied here (4 to 8)”. Therefore, I strongly recommend authors to omit such a generalization here and in their future works.

Response: Thanks so much for the detailed comment. We fully agree with the reviewer and such a generalization has been omitted (Page 21 Line 16-17). In our future work, we must be more precise and avoid the generalization like this.

Page 20 Line 10: This is speculation as the community structure was not measured in the study. Delete.

Response: We agree with this. And the speculation has been deleted (Page 21 Line 17-19).

Page 20 Line 18: The sentence is too long. Split.

Response: This sentence has been split (Page 22 Line 2-5).

Page 20 Line 21: In their study, it is very likely the moisture regime changed with the coarseness. It is not clear, if the soil moisture was measured in the experiment. However, water content in a semi-arid ecosystem (and generally) is one of the basic parameter affecting ecosystem (namely, microbial in the current context) functioning. So, it is very surprising nothing is even mentioned about this key factor. Without data on soil moisture content the discussion of other important but secondary to water regime environmental parameters seems to be incorrect.

Response: Thanks so much for the reviewer pointing out this important issue. We have added the data of soil moisture content which was measure right after the soils being sampled (Page 12 Line 7-8). Also, we added and mentioned data of water

holding capacity in both result (Page 12 Line 8-9) and discussion section (Page 20 Line 19).

Page 21 Line 4: “might be”—maybe not?

Response: Thanks. We have reworded “might be” into “could be” (Page 22 Line 10).

Page 21 Line 8: “unchanged”—Flawy expression: rephrase or omit.

Response: This has been rephrased into “unaffected Olsen-P concentration” (Page 22 Line 14).

Page 21 Line 11: Does this mean there was no increase of abiotic P supply? Otherwise it is difficult to assess whether decrease of P fixation on clays could overcompensate the decrease of biotic P release. Is there any information available on the abiotic P supply due to sand increase? What was actually P content of sand used in the study?

Response: Here, “suppression of P fixation” means increase of abiotic P release (fewer fixations by clay). We did not measure P content of the river sand, but the sand should contain very low concentration of Olsen-P. Decrease of clay content would definitely decrease Olsen-P fixed on its surface which would result in increase of abiotic P supply to balance decreased biotic supply. Only in this way, it will keep Olsen-P concentration stable (Fig. 4c). To make this clear, we have rephrased this sentence (Page 22 Line 15-16).

Page 21 Line 16: add “be” after “could”.

Response: The word has been added (Page 23 Line 1).

Page 22 Line 2: add “in” before “contrast”.

Response: The word has been added in Page 23 Line 11.

Page 22 Line 7: The sentence is too long. Split.

Response: The sentence has been split into two. It now reads: “These results were in contrast to findings from Cleveland et al. (2007) who suggested C:N:P ratios of both soils and microorganisms were well-constrained at the global scale. However, our results were consistent with Sinsabaugh et al. (2008) who found ratios of microbial C-, N-, and P-acquisition enzymes were variable and depended more on environmental parameters, such as substrate availability, soil pH and the

stoichiometry of microbial nutrient demand.” (Page 23 Line 11-16).

Page 22 Line 14: Again, there was no comparison with the initial state (before the experiment) provided. What is shown, relative difference between treatments after 4 years of the experiment without any intermediate data. Therefore, it is inappropriate to speak about the rates.

Response: Thanks for the observation. We fully agree with the reviewer’s advice. Thus, we have rephrased “faster” into “more” (Page 24 Line 9).

Page 22 Line 16: Maybe first of all, the soil moisture decreased with the coarseness.

Response: Thanks for the advice. We have added soil moisture data and rephrased it into “resulting from decreases of soil moisture, C pools and fine particles” (Page 24 Line 12).

Page 23 Line 3: The soil depth effects were not clearly discussed and concluded on that. So, seemingly these data could be excluded from the manuscript.

Response: Thanks so much for the comments. We have excluded these data from the manuscript.

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