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Interactive comment on "Organic matter dynamics along a salinity gradient in Siberian steppe soils" *by* Norbert Bischoff et al.

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Dear Referee #2 (R#2),

Thank you for taking your time to go through our manuscript and give us helpful advices and corrections.

R#2: This study aimed to understand the role of salinity in shaping soil organic matter. The study is somewhat confounded because the salinity gradient covaries with a moisture gradient. The saltiest soil is closest to the water table and had the highest moisturecontent while the low salinity soil was far from the water table and had much lowersoil moisture. Consequently, it is not possible to separate out the effects of moistureand salinity on the soil carbon and microbial community. Despite this limitation

themanuscript presents a robust dataset that is, on the whole, well contextualized.The presentation of the data is quite dense and the manuscript is made less comprehensible by the excessive use of abbreviations. The authors should work to simplify theresults where data is sometimes redundantly presented in the text, tables and figures. There also seems to be an excess of supplemental data that is simply an alternatepresentation of the data shown in the tables. Generally, I think the authors could do more to explain why their findings do not match those reported by others. The moisture gradient seems to be the most obvious reason to me yet this is not well discussed in the manuscript.

A: It is true that the salinity gradient covaries with a moisture gradient, which is a broadly occurring situation in the study area. Salt-affected soils are those close to the groundwater table and are thus generally moister than the non-salt-affected soils which occur at a larger distance to the groundwater table. Therefore, this should not be seen as a limitation of the study. It is just the natural association of the soils in the semi-arid steppe. We agree with you that we should discuss this in more detail in the revised manuscript. In particular we are going to include the fact that it is difficult to separate the effect of salinity and moisture on soil OM dynamics and the microbial community. We will discuss that the missing effect of salinity on the soil OM dynamics along the transect could also be explained by the covarying moisture gradient. According to your comment we are going to delete some abbreviations, e.g. SPT. If data is redundantly presented, we will delete redundant data, e.g. the d13C ratios in Table 3 which are similarly presented in Figure 4. In our opinion there is no excess of supplemental data and it is not an alternate presentation of the data shown in the tables. Only Figure S5 and S6 are partially redundant to Figure 5, but we consider these figures as informative as they highlight precisely how the two sugars glucose and arabinose differ between the soil types with respect to plant samples and organic matter fractions. This is not possible to show in a PCA in such detail. In the revised manuscript we are going to explain in more detail why we think that our results differ from those observed in other studies. As was already discussed, this could be due to the covarying moisture gradient, particularly the pronounced aridity in the Kastanozem, which may have led to a smaller OM input and consequently smaller OC stocks. Moreover, soil OM decomposition could have been inhibited and thus the soil OM transformation appears similar to those of the salt-affected soils.

R#2:Page 3 - 31. Suggest start new sentence, i.e. change “OM, while particulate OM” to“OM. In contrast, particulate OM”

A:Thank you for this advice. We are going to change it accordingly in the revised manuscript.

R#2: Page 8 - 5 If the salt interfered with the internal standard peak how can you be sure it did not interfere with any of the other peaks?

A: We are very sure that the salt did not interfere with any of the other PLFA peaks, as the peaks appeared clear and with a characteristic shape. Moreover, the shape and the appearance of the peaks were similar in the salt-affected and non-salt-affected soils. This was not the case for the internal standard peak, which clearly differed in the topsoils of the salt-affected soils by a clear overlap with another unspecific peak.

R#2: Figure 3 is not referred to in the results section

A: By accident, on p.10 l. 36 we referred to “Table 3” instead of “Figure 3”. We are going to change this in the revised manuscript.

R#2: Note to self salty soils have more clay and more moisture – these are factors thatstabilize C

A: To argue that a higher clay content may contribute to a larger C stabilization in the salt-affected soils is a good point. We are going to integrate this in the revised manuscript. To discuss soil moisture as a factor that stabilizes C along the transect is, in our opinion, difficult, because the “available” soil moisture (i.e. water potential) is possibly similar along the transect, either due to a low osmotic potential (Solonchaks) or a low matric potential (Kastanozems). Thus, we would not like to integrate this into

the discussion.

R#2: Page 10 - 25 Can you write out SPT this is not used frequently enough to warrant abbreviation

A: Yes, we are going to write it out in the revised manuscript.

R#2: Page 10 -27 Can you just refer to the loss as mobilized C, I think that would make itless confusing. I had to reread the methods to understand this part of the results.

A:We think it would not be correct to refer to the loss simply as mobilized C, since the mass loss in the salt-affected soils is largely due to the dissolution of C-free salts. Hence, in these soils we observe a large mass loss but only a minor loss of mobilizable C. In the Kastanozem, however, less total soil mass was lost during the density fractionation but this was associated with a larger portion of mobilized OC. Since we think that this differentiation is important, we would like to keep it in our revised manuscript.

R#2: Page 10- 32 I think this is a sentence for the discussion.

A: This is what we explained in the previous comment. We agree with you, that this sentence fits better into the discussion. Hence, we are going to move it in the revised manuscript.

R#2:Page 10 – 37 What does B.P. stand for ?Before Present?

A: Yes, B.P. means “Before Present”.

R#2: Figure 4 is also not referred to in the results- only the tables. Perhaps the data should not be redundantly presented in both locations?

A: Indeed, figure 4 is referred to in the results (p. 10 l. 23). However, we agree that the data on d13C is somewhat redundant. In the revised manuscript we are going to delete d13C values from Table 3 and refer only to Figure 4.

R#2: Figure 5 – Is there a need to show the grey dots in each panel?

A: The PCA on neutral sugars was applied on the entire data set, i.e. neutral sugar data of all three soil types and all three fractions was analyzed in one PCA. This resulted in the biplot shown in Figure 5. To highlight differences between the soils we split the biplot into three panels and indicated the fractions of each soil by different colors. The biplot shows all considered data (i.e. the entire data set); this includes the grey dots which do not belong to the particular soil type of a panel.

R#2: Page 11 – 21-33 Have you considered doing a PerMANOVA to determine if these differences in sugar composition are significant?

A: PerMANOVA is a robust tool to test multivariate data on statistical significance. However, a minimum sample size is required to obtain reliable results. As mentioned in the statistics section of the Material & Methods part, we only have 3Âň–4 field replicates (i.e. 3–4 soil profiles per soil type). One might argue that we consider more than 3–4 samples per soil type in the PCA, but this is because the soil profiles were sampled in horizons and data of each horizon is also considered in the PCA. As these horizon samples are nested within the soil profile they cannot be treated as independent samples. If so, they would be referred to as "pseudo-replicates" and application of statistical hypothesis testing on such data would result in underestimation of p-values. Therefore we decided to refrain from a PerMANOVA and analyze the data descriptively.

R#2: Page 11 – 35 this sentence is confusing " The relative contribution of PLFA observed within the PLFA profiles"

A: In the revised manuscript we are going to change the sentence to: "The relative proportion of PLFA on the entire data set was as follows:"

R#2:Page 12 – 5 As with sugar composition you should be able to test statistically if thesites and soil profiles are statistically distinct in terms of microbial community structure.

A: Please refer to our previous comment regarding statistical hypothesis testing on

neutral sugar data.

R#2: Pag 12- 20 Given the high CV for these soil types I'm not sure that soil type is such a great predictor of carbon stock.

A: Soil type is, for sure, not the best predictor of C stocks but it is, as a single variable, possibly more precise than solely predictors such as temperature, moisture, parent material, or clay content. Anyway, this was not the reason why we discuss C stocks of soil types in that part of the manuscript. In this section we aim to compare our measured data on C stocks to data of previous studies investigating similar soils. This is particularly important as our data is different from what was found in other studies. We therefore would like to keep this part in the revised manuscript.

R#2: Page 13 – 14 How are you sure the soils are not affected by erosion?

A:This is explained on p.4 l.30. Sample locations were plane with <0.5° slope inclination. Thus, the probability of erosion is reduced to a minimum.

R#2: Page 13-15 – Could reduced decomposition due to salt stress and anaerobic conditionsfrom the high moisture content be contributing to the higher organic matter content inthe Non-sodic and sodic Solonchaks?

A: We would expect that a reduced decomposition due to salt stress would result in an accumulation of particulate OM. This was not the case in the studied soils: salt-affected and non-salt-affected soils contained similar proportions of particulate OM. Moreover, the analysis of C isotopes and neutral sugars indicated a comparable degree of OM alteration between the soils, as already discussed in the manuscript, while we would expect a smaller OM alteration if decomposition would be reduced in the salt-affected soils. Higher OC stocks in the salt-affected soils were particularly found in the topsoils. Anaerobic conditions in the topsoil are very unlikely as the gleyic properties of the soils show that anaerobic conditions can be primarily expected in the subsoil, but not in the topsoil. For example, in the four soil profiles next to the lake, Fe and Mn mottling reaches on average 84 $\pm$ 16 cm soil depth, thus indicating the maximum average ground water level during flooding. In the subsoil differences between OC stocks were smaller. Thus, we do not consider anaerobic conditions as a factor explaining the high OC stocks in the Solonchaks.

R#2: Page 13 – 30 Could you remind us what your second hypothesis was?

A: In the revised manuscript we are going to repeat the second hypothesis at the beginning of that paragraph.

R#2: Page 13 – it seems that the water availability to plants and microbes might be similar inthe dry salt free Kastanozem and the wetter but salty Solonchaks (i.e. similar osmotic pressure). This could explain why above ground biomass was similar and explain the similarities in soil C.

A: This is a good point and was already noted in the manuscript (p. 12 l. 37 – p. 13 l. 5). However, we think we should discuss this issue more intensively in the revised manuscript. This was already noted by Referee #1 and we are going to revise the discussion in the revised manuscript with particular focus on that point.

R#2: Page 15-19 this lets us assume?

A:We are going to change that in the revised MS to "This led us to the conclusion. . ."

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