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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 #1 (R#1),

Thank you for taking your time to go through our manuscript and give critical comments and advices. We agree with you that we partially overinterpreted the results (deriving from PLFA measurements the functionality and resilience of the microbial community) and that we should discuss some data more carefully. After discussing your comments we decided to add a fourth hypothesis to the existing three: (i) soil OC stocks decrease with increasing salinity, (ii) the proportion and stability of particulate OM is larger in salt-affected than in non-salt-affected soils, (iii) sodicity reduces the proportion and stability of mineral-associated OM, and (iv) fungi : bacteria ratios, as derived from

PLFA measurements, decrease along the salinity gradient. By that, we connect our objectives with the hypotheses, as you suggested. Moreover, we are not going to relate the PLFA data directly to the proportion and stability of particulate OM, as this in fact would result in an overinterpretation. Setting up a fourth hypothesis allows for a separate discussion of the microbial community data. We are going to discuss the possibility that a functionally diverse fungal community contributed to the progressive decomposition of particulate OM. In addition, we will clearly state that a similar water stress along the salinity gradient could be responsible that we have not found a different alteration of OM along the transect. In the introduction we will remove the explanation on Solonetz soils, but focus on salinity and sodicity, as these were the main issues of our study. By that we follow many of your helpful advices. We are going to await the decision of the editor and, if positive, work on the manuscript revision.

General comments

R#1: Considering the increasing extent of salt-affected soils, this MS deals with an important and timely issue. Understanding the carbon dynamics in salt-affected soils is aninteresting topic within the scope of Biogeosciences. The biggest problem of the presented study is that the salinity gradient was confoundedby a large difference in soil moisture between the saline sites and the non-saline site.This is discussed by the authors (p. 12 l 37-p.13 l 2), but the importance is under stated. Soil moisture has an enormous impact not only on plant productivity, but ondecomposition processes, which are inhibited strongly by lack of water (Manzoni et al.,2012). The possibility cannot be excluded that the alteration of OM was found to besimilar at saline and non-saline sites, because decomposition was inhibited by lack ofwater at the non-saline site, especially if the non-saline site is drier than the saline sitesthroughout the course of the year. It is therefore not possible to conclude that microbialactivity was resistant to salinity in the studied soils, since it could have been inhibitedby low water availability at all sites, caused by different mechanisms. As a result, amajor revision of the discussion is needed. As a suggestion, it could make sense touse water potential as a parameter, to allow for an easier comparison between sitesand distinguish between the effects of salinity and moisture.

Authors (A):We agree that the salinity gradient was possibly confounded by a difference in soil moisture. Hence, the interpretation of the data is currently not straightforward. After evaluating the data for the first time, we were aware of the problem and intended to calculate the water potential of the soils, as you have suggested. Thereby, we faced the problem, that we could not measure the matric potential directly by use of soil water retention curves, since we had no undisturbed soil cores of the studied soils. Thus, we had to use Pedo-Transfer-Functions (PTF′s) which estimate the matric potential via soil parameters, such as soil texture, bulk density, organic carbon content, and actual soil water content. Such a PTF was proposed in Vereecken et al. (1989) with the Van Genuchten model. Another possibility is to calculate the model parameters for the Van Genuchten model via the software "RETC". Both, the use of the PTF′s in Vereecken et al. (1989) and the use of "RETC" have not yielded plausible results for our soils. This might be due to the fact, that the PTF′s were empirically developed for temperate soils without influence of salinity. As a consequence, we cannot calculate the matric potential for the soils in our study and, thus, neither the water potential. However, this is not limiting the significance of our study, since (i) it is a natural phenomenon that salinity co-varies with soil moisture in the study area, thus, our transect represents the occurring natural conditions, (ii) the soil moisture measurement given in Table 2 represents just a "snapshot" at the moment of soil sampling and not a mean value during a longer period of time. To draw conclusions about the possible effect of the matric potential or water potential, respectively, on processes like soil OM decomposition, we would need to measure these parameters for a longer period of time. Nevertheless, we agree with you that the effect of soil moisture is a critical aspect in the manuscript and should be discussed more extensively. In the revised manuscript we intend to revise the discussion thoroughly, particularly with respect to the effect of matric potential vs. osmotic potential and the overall water potential on soil OM decomposition. In particular we are going to state that it is possible that we have not found differences between the soils with respect to soil OM decomposition, because of a similar water stress/water potential in all soils.

R#1: Another serious issue is that the dataset is very limited, to the extent that statistical hypothesis testing was not possible. Effectively, the number of independent samplesalong the salinity gradient is only 3.

A: The number of independent samples along the salinity gradient was 3 or 4 (for the Non-sodic Solonchaks), respectively. Thus, statistical hypothesis testing was not possible, as noted on p. 8 l. 34Âň-36 of the manuscript. However, this does not mean that the dataset is limited. We decided to conduct an in-depth analysis by measuring many soil parameters per soil profile and relate them to each other in order to reveal processes which take place within the soil. This was done in many previous studies (Fierer et al., 2003; Kemmitt et al., 2008; Shen and Bartha, 1996). By that, we actually obtained a very large and detailed data set. For example, only by use of isotopic data (13C, 14C) and neutral sugar measurements in combination with PLFA we could reveal that POM was not distinctly altered in the studied soils, maybe due to a functionally diverse and resilient microbial community, which is capable of decomposing POM at a similar rate in salt-affected and non-salt-affected soils. As you have mentioned in the previous comment, in the revised manuscriptwe will add to this explanation, that it is possible that a lower soil moisture in the non-salt-affected soils has led to similar POM decomposition in the salt-affected and non-salt-affected soils.

R#1: The manuscript is generally well written, if a bit lengthy in some areas (Results) and underdeveloped in others (discussion). However, there are some sentences that containclumsy English structures.

A: In the revised manuscript we are going to shorten the results section (e.g. the part about soil mineralogy and by generally not repeating the numbers from the tables too extensively). On the other hand, we are going to work on the discussion section including more detail and discussing also controversial positions, such as the fact that soil moisture could have a crucial impact on soil OM decomposition along the transect. Sentences that contain clumsy English structures are going to be revised.

SpeciïňĄc comments

R#1: p2 l17-19: While you measured the microbial community composition, I do not understand how you derive from the results that the functioning and capability to decomposeof the community was virtually unaffected by salt. This seems like an overinterpretationof the data.

A: As mentioned above, we agree that this could be an overinterpretation of the data. We are going to soften this conclusion in the revised manuscript.

R#1: p3 l 6-7: This is a bit confusing, since Na+ is also a water-soluble salt. Another issue:Here you refer to Solonchaks and Solonetzes, but later in the MS you switch to sodicand non-sodic Solonchak. Naming should be consistent.

A: Na+, as such, is not a water-soluble salt but a monovalent cation. To make the sentence clearer, we may change it in the revised manuscript to: "Solonchaks contain high loads of water-soluble salts in general, while Solonetzes are particularly characterized by Na+ as the dominant cation on the exchange sites, irrespective of the quantity of salts." Here we distinguish between Solonchak and Solonetz to explain the difference between non-sodic and sodic. But, we agree with you, that we could shorten the explanation regarding "Solonetz" in the revised manuscript, as this particular soil type was not part of our transect.

R#1: p 3 l 136: which previous studies?

A:Thank you for this attentive note. Previous studies are for example Mavi et al. (2012), Setia et al. (2013, 2014). We are going to add this to the revised manuscript.

R#1: p 4 l 4: What is the expectation for the third objective?

A: After your comment about the "overinterpretation" of the PLFA data (microbial community composition / functioning), we decided to attenuate the conclusion on the results of the third objective. Moreover, we came to the conclusion that it is not straightforward to relate the PLFA data to the results of soil OC stocks and quantities and properties of functionally different OM fractions. Thus, we will restate our third objective to "(iii) analyse changes of the microbial community composition". This objective will be kept quite general, as to our knowledge there are no studies which have determined microbial community compositions in Solonchaks or Solonetzes so far (which we stated on p. 3 l. 34-36). We are going to include this in the revised manuscript.

R#1: p 4 l. 13-16: As a suggestion, the focus of the MS would become clearer, if the hypotheses would follow your stated objectives above.

A: In the revised manuscript we are going to integrate your suggestion. We will set up three objectives and add a fourth hypothesis regarding the microbial community composition.

R#1: p. 8. l. 26: Was plant biomass the only response variable that was tested?

A: Yes, plant biomass was the only response variable that was tested, because this was the only parameter for which we had a sufficient number of samples/replicates. This was because it is a parameter which is easy to measure without the need of lots of time and money.

R#1: p.8. l.37: By "involved the consideration of several response variables", do you mean multivariate statistics? It is an unclear sentence.

A:In the revised manuscript we will change the sentence to: "Data of PLFA and neutral sugars were analyzed by PCA in order to consider multiple response variables. Confidence regions (68%) for the group centroids of the independent factor variables were added to the biplots."

R#1: p.9 l. 27-36: This section is never clearly brought up in the discussion and I am not sure if these results contribute important information.

A:We determined the soil mineralogical composition principally because of two reasons: (i) to characterize the mineralogical composition of water-soluble salt minerals in the salt-affected soils, and (ii) to determine the clay mineralogical composition particularly with respect to expandable clay minerals, such as smectite, as these affect the physical properties of sodic soils crucially (see p. 6 l.4-5). The mineralogical characterization of the water-soluble salt minerals is primarily descriptive, but informative and important as we study salt-affected soils. The clay mineralogical composition turned out to be similar between the soils and therefore cannot explain differences between the soils later on in the discussion. Thus, we may move this section to the Supplements in the revised manuscript.

R#1: p. 10. l. 21: What could be the reason for decreasing _13C ratios? Leaching? This is missing a discussion. Could also be linked to the 14C increase with depth.

A: In our opinion, decreasing d13C ratios cannot be caused by leaching as the net-movement of water in the salt-affected soils is upwards. Decreasing d13C ratios, and as such increasing 14C activities, with depth could be related to a faster soil OM turnover. In the Solonchaks of our study this could occur due to the water stress in the topsoil (osmotic stress and matric stress) while the subsoil is generally wetter due to the proximity to the groundwater and a lower salt content. Hence, the conditions for microbes to process soil OM could be better in the subsoil than in the topsoil. This would explain the observed pattern in the Solonchak, but not the increase of 14C activity in the Kastanozem. Since this is very speculative, we decided to leave it out from the discussion. But we may add it to the revised manuscript with the advice that this asks for further investigation in future studies.

R#1: p. 12 l.9: I don't see any differences in community composition between soil types.Consider changing the wording of "less pronounced".

A: Indeed, there are differences in the microbial community composition between the soil types. Please consider the confidence regions in Fig. 6a with a larger variability on

PC2 for the salt-affected soils. This corresponds to a larger variability of fungal PLFA in the salt-affected soils. Though the differences are small, they are existent and should be mentioned. However, we are going to change the wording to “small” instead of “less pronounced” in the revised manuscript.

R#1: p. 13 l.16-18: Again, since the Kastanozem was so dry, I would be careful to talk about a lack of inhibition by salinity. Were the OC stocks actually large compared with whatwould be expected in a steppe soil? Bring this statement into context with data fromother studies.

A: As mentioned in a previous response to one of your comments, in the revised manuscript we are going to include a more intensive discussion on the fact that the very dry conditions in the Kastanozem could have led to a similar water stress in the Kastanozems and Solonchaks, with the respective consequences on soil OM input and soil OM decomposition. So far this was only little discussed in the manuscript (p. 12 l. 37-39, p. 13 l. 1-5). As already mentioned in the manuscript, the OC stocks of Solonchaks were large when compared to data from other studies, while the Kastanozems of the transect revealed smaller OC stocks than previously observed in other studies (see chapter “Discussion: Soil OC stocks along the salinity gradient”).

Technical comments:

R#1: p. 4. l. 22: Upslope of the lake?

A: “to about 5m above the lake”

R#1: p.9. l. 18: lowest EC1:5. Also in other places in the MS “small” should be replaced by“low”, and “large” by “high”.

A: Thank you for this correction. We are going to correct this in the revised manuscript.

R#1: p.10 l.23: Consider changing the order of Figure 3 and 4, so that it matches the first appearences in the text

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Discussion paper

A:In the revised MS, we are going to change the order of Figure 3 and 4.

R#1: p. 10 l.36: Did you mean Fig. 3?

A: Correct. We are going to change this in the revised MS.

R#1: p.15 l. 19: This led us to the conclusion.

A: Thanks for the correction. We are going to integrate this in the revised MS.

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