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

Anonymous Referee #1

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Bischoff et al. studied soils from a salinity gradient in the Siberian steppe. This salinity gradient covered three different soil types of different salinity and sodicity levels. Along these gradients the authors set out to evaluate three hypotheses: (i) SOC stocks decrease with salinity, (ii) the particulate OM pool is larger and more stable in salt affected soils, (iii) high Na+ concentrations reduce the proportion and stability of mineral-associated OM. To evaluate these hypotheses the authors used samples from different soil horizons and measured SOC and δ13C in the bulk samples as well as in the light and heavy fraction after fractionation. In addition, 14C ages of the fractions were measured, as well as a range of sugars. Microbial community composition was estimated by measuring PLFA patterns. Based on their results, the authors rejected all three of their hypotheses. SOC stocks increased with salinity and the particulate OM fraction was not affected by salinity. Moreover the proportion and stability of mineral-bound OM

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Discussion paper

was not affected either.

General comments

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 an interesting topic within the scope of Biogeosciences.

The biggest problem of the presented study is that the salinity gradient was confounded by 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 understated. Soil moisture has an enormous impact not only on plant productivity, but on decomposition 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 be similar at saline and non-saline sites, because decomposition was inhibited by lack of water at the non-saline site, especially if the non-saline site is drier than the saline sites throughout the course of the year. It is therefore not possible to conclude that microbial activity was resistant to salinity in the studied soils, since it could have been inhibited by low water availability at all sites, caused by different mechanisms. As a result, a major revision of the discussion is needed. As a suggestion, it could make sense to use water potential as a parameter, to allow for an easier comparison between sites and distinguish between the effects of salinity and moisture.

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 samples along the salinity gradient is only 3.

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 contain clumsy English structures.

Specific comments

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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 decompose of the community was virtually unaffected by salt. This seems like an overinterpretation of the data.

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 sodic and non-sodic Solonchak. Naming should be consistent.

p 3 l 136: which previous studies?

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

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.

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

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

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.

p. 10. l. 21: What could be the reason for decreasing δ13C ratios? Leaching? This is missing a discussion. Could also be linked to the 14C increase with depth.

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

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 what would be expected in a steppe soil? Bring this statement into context with data from other studies.

p.13 l. 34-37: How does the salinity in your study compare to that in Peinemann et al. (2005)?

Technical comments:

p. 4. l. 22: Upslope of the lake?

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

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

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

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

References

Manzoni, S., Schimel, J.P. and Porporato, A.: Responses of soil microbial communities to water stress: results from a meta-analysis, Ecology, 93 (4), 930-938, 2012.

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