

Interactive comment on “Limited protection of macro-aggregate occluded organic carbon in Siberian steppe soils” by Norbert Bischoff et al.

Anonymous Referee #2

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Page 2 Abstract

As a matter of fact, an abstract gives the paper (highly concentrated) consequently, all comments and recommendations given below apply also for the abstract.

Page 3

Line 7: please add a reference to the statement “As the stabilization.for maintaining soil fertility.

Line 9-11: in my understanding Lützow et al. do not point out that mineral-ass. and physical disconnection are the main ones, they rather want to strengthen that recalcitrance is not that important than thought

Line 11 13: delete this sentence, as your investigation is not done in the dry steppe,

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rather substitute by own data (Bischoff 2016). (And to your knowledge, Kalinina et al. (2014) found comparable C ratios in aggregate and clay fractions for dry steppe soils)

Line 16: is “primary particle” the right term? What about “detached” or “isolated”?

Line 22-23: I would add as explanation for the importance of aggregate C the very pronounced crumble structure (at least in Chernozems the best I have ever seen)

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Line 1: what is meant by “complicate reliable assessment”? Please explain more detailed.

Line 5: please explain why Siberian steppe soils need special attention? Are results of the same soils but different regions not transferable? They must!

Line 15: delete “agricultural”

Line 18-20: first hypothesis is not consistently deduced from the literature! The authors state themselves that increases and decreases were found (page 3)

Line 20-21: also inconsistent: the authors refer to the opposite (page 3, line 29). The second part “land-use duration” and “intensity” (what is exactly meant by this term?) is not derived from knowledge from the literature (missing state of the art)

Line 24-25: is the approach of getting of getting pools from fittings decay models an appropriate one? Please explain to those who are not familiar with it, add references

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Line1-2: As stated, the Kulanda steppe is semi-arid. How can FS be part of this steppe (forest steppe in semi-arid steppe)?

Line 4: hopefully with comparable grain sizes within each chronosequence, please confirm

Line 5 (Tab. 1): I. missing data on grain size distribution, please add, so that any grain

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size effect on analyzed process can be excluded. II. The term "soil type" is not used in WRB, please correct. III Replace the term "Typical Steppe" by "Semi-arid Steppe" and introduce abbreviation (throughout the text). IV here 30yr in line 12 ten years, what is correct?

Line 7: use one term throughout the text for "more arid typical steppe, you introduced before "semi-arid" (much more consistent) and be stay thereafter

Line 8: please clarify, how you identified sites

Line 9: I. Why did you resign to include a natural plot? All plots of second chronosequence have a management history, hence, no discussion on land use change can be done. Additionally, can you prove that the first plot (FS) has no cropping history at all, although cropped sites are that near and land use change seems to be distributed all over the investigation area , is crucial point (see discussion). II. The forage plot also has a time of cultivation, please add, otherwise infeasible. III What exactly makes the forage site to an intensively used one?

Line 13: abbreviation "TS" not introduced, should be exchanged by SAS (see above)

Line 19: "meanwhile"?, I guess since 1983

Line 16: 30 yr fallow means sampling year was 2013, correct? Please add more information on sampling design

Line 22: "which are attributed to erosion" is a speculation, delete. E.g. position top hill vs. slope toe might also be feasible

Line 24: and if erosion is the case you have mixed material at plot 19 yr (autochthonous and from above) which makes plot 10 yr unfeasible. Strictly argued: Plot 10 yr has to be deleted, but what is left?

Line 25: How you prove that key profiles are representative?

Line 26: I. delete "genetic" II. arable 30yr plot not introduced before

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Line28: despite not all sites were investigated by a key profile, all plots have to be analyzed in respect of grain size distribution (see above). II. Why EC was measured? Delete, if you do not refer to somewhere. III: what means "composite"? Mixed samples?

Line 30: Tab S1 not required, coordinates can be integrated in Tab 1

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Line 2: which samples are "all samples"? Those from the profiles? And if so, why is always only one data set per soil is given, and not those per horizon? And from what horizon were the given data?

Line 3: why was the residual water measured? Nowhere appearing again

Line 18: statement on amount of samples is redundant

Line 20-25: it is not clear how the quantification was done. II. Fig "1 is not required, because 1) it does not help to understand the quantification and 2) does not appear again

Line 27: how was WHC determined?

Line 31: replace "sampling" by "filling"

Fig. 1: not introduced. II. site photos not meaningful (delete). III. profile too small. IV. Map not meaningful (medium scale is missing) + scale not stated + missing north arrow

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Line7: what is meant by "increasing duration "? A reached equilibrium after 5 yr cropping?

Line 11: see comment on this issue above. In addition: at the top plot you might have include sub soil material as top soil was eroded. However, any erosion process includes addition from elsewhere or losses from top and addition from the sub soil, pro-

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cesses completely destroying investigation approach and hence have to be completely avoided (make sure). II. In addition, erosion statement was only given for 10 yr above, but not for 1 yr, as firstly described here.

Line 7 + 12: it might be interesting to point out the differences in C/N of both chronosequences?

Line 14 and following: it is not clear when the measurements were done, after incubation? It hampers a reviewing with regard to content

Line 16 (Fig S2): if the figure shall only show which is stated in the sentence beginning in line 14, the figure is not required (delete)

Line 17-19: I do not understand the relationship between the two sentences given here

Line 17 (Fig. 2): Figure does not show different scales, as written in the heading

Line 19-21 please interpret Fig 2 more correctly, what about TS?

Line 14-21: these few lines are supported by four figures/tables. I propose just to keep Fig. 3.

Line 23: advice to Fig. 3 needed but not to Fig 2 (delete)

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Line 10: what is meant by "the sites", please indicate more precisely

Line 30 (Fig S3): not meaningful, delete

Line 32 (Fig S4): not meaningful, delete

Page 11 and 12 Discussion on limited protection macro-aggregate C

As already noted above I recommend a more careful discussion on this aspect. All plot of the second sequence have a cropping history. Thus, it might be possible that macro-aggregate occluded C was lost than, never built up again (Kalinina et al., 2011,

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found during self-restoration of post-agrogenic Chernozems an increase in C, however in relations to other fractions increase of aggregate C was less existing) land thus, you find no differences. A cropping history can in all probability also not be excluded for the first plot of the first chronosequence. This means your chronosequences lack of proven uncropped former stage. So, once again this aspect has to be included into the discussion.

Page 12 and 13 Discussion on effect of management, soil and sites on mineralization

The discussion has to be done more tentatively, as differences in fast soil pools of grassland and cropland was not significant (see Fig. 4 and page 10, line 11). In this respect, statement beginning in line 28 is too offensive (just trends), the same is true for line 31 (or was this a literature statement, then add references). On the other hand the statement "our results support." (page 13, line 5) is too general (what results are explicitly meant?). In addition be again be careful in discussion LUC from grass to crop (see comments for page 11, 12). At least, it is nice to see homogenous effects upon ploughing by your data, however, this is an old story.

Page 13-14 Conclusion

I again recommend a more careful writing many statement are not underlined by significant data, and again see comments for page 11, 12.

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