

## ***Interactive comment on* “Centennial black carbon turnover observed in a Russian steppe soil” by K. Hammes et al.**

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### **General comments**

The presented study makes use of the unique availability of a 100 yr old preserved soil core to study the turnover time of black carbon. The black carbon stocks in two similar soil cores that were sampled recently were lower than that of the preserved core. The authors show with a model and a sensitivity analysis that this observation can be explained by assuming turnover times of black carbon between 212 and 541 years. This turnover is much faster than what is usually assumed in models of soil organic matter dynamics to explain the ages of soil carbon up to several thousand years. Hence, this study is a unique contribution to show the limits of our current understanding of soil carbon stabilization and the role of soil carbon in the biogeochemical cycles and its

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response to global change.

Despite its high relevance and uniqueness I have two major concerns. Other possibilities that might also explain the observed differences in black carbon stocks must be ruled out or at least discussed in more detail.

1. The role of transport of black carbon out of the soil profile.

The depth profile differs considerably between the three soil cores. Why is this? The authors should discuss the reasons for the differences across the soil depth and validate or argue that transport out of the profile is not a major concern.

2. The spatial variability of black carbon stocks.

How large is the spatial variability in soil carbon stocks? This issue is discussed only vaguely in the methods section. Is there data that could quantify the spatial variability? If not, the authors should statistically compare measures of variability of the recent cores with difference across time. This seems to be straightforward with Fig. 2.

Further, I obtained slightly different turnover times. This is probably due to I did not correctly understand the scenarios of the best estimate and the min/max estimates. Suggestions to improve the description are given with the specific comments.

### **Specific comments**

The abstract is well written, concise and presents the major message of the paper. It should be stated that other factors such as transport and spatial variability are no major concerns.

In the following P=page L=line

P663 L4 "these studies" It is not clear which studies are referred to. State this more precisely such as "These incubation studies ...".

P661 L10-17 In my opinion not all the detail is required.

P664 L9 "exactly the same location" A repeated destructive sampling at "exactly" the

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same location samples a disturbed soil. In the cited Torn et al. 2002 I read that the location was narrowed down to "inside the microplateau of preserve 1" having an area of a few hectare. Do you have an estimate of spatial variability of C stocks and BC stocks within this area? It would be nice to report it at appropriate paragraphs. Maybe this is not an issue with the steppe soil. However, with my experience on Central European forest soils spatial variability within a few meters often is an issue.

P665 L14,15 "fairly representative": State this in more detail. If it is a summary of the following paragraph move it to the end of the paragraph.

P665 L18 "little spatial variation in the soil parent material": The reference to table 1 on Line 21 is not intuitive to the reader. Better present additionally a graph of the soil profile depth bulk densities and organic carbon with all soil cores together. Or quantify the variation as coefficient of variation or an adequate statistical measure.

P665 L24ff: I cannot judge the validity of the black carbon analysis based on my experience. The editor either should trust the author's experience, which I can support. Or the editor should inquire with other referees.

P666 L19ff: The model is correct but described in a too concise way. I could follow the derivation only after a more detailed explanation. Maybe an appendix or online supplement is a good solution.

P667 L12: I do not understand the numbers given for b.

When trying to follow the described sensitivity analysis I obtained the following differing results. This may be due to that I did not correctly understand the scenarios of the best estimates and the min/max estimates. When I decrease bulk density for  $S_0=2.5\text{kgC/m}^2$  by 10 percent so that initial stocks are only  $2.25\text{kgC/m}^2$ , I get for  $S=1.9\text{kg/m}^2$   $b=0.84$ . I suggest that the authors give used numbers for the scenarios in a table similar to the following one:

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scenario	t	f	S0	b	tau	b	tau
				S=1.9		S=2.0	
best estimate	100	0.1	2.5	0.76	322.4	0.8	397.9
min	94	0.1	2.75	0.69	223.4	0.73	260.4
max	102	0	2.25	0.84	603.2	0.89	866.0

P668 L2: Why is the BC data presented as supplement and not with table 1? When you refer to the supplement state what the reader can expect to find. "Table of BC stocks with depth given in suppl. ..."

Fig. 1: It is hard to compare the inset graphs with the outer graphs. If not additional graph or measures of spatial variability is given, this is a severe drawback. Why is Fig1C missing the inset data of the 2004 soil? The integrated carbon stocks across the profile for the two recent cores are similar, however the depth distribution is not. Why is this? Only the differences in depth between the 1997 core and the preserved are discussed in the text (P668 26ff).

A legend of the fills or annotations directly in the graph would be helpful.

P668 L20: In addition to the presented numbers refer to Fig 2.

Fig 2: What do the error bars represent: standard error, 95percent confidence intervals? Is it correct that the error bars do not represent replicates at each plot but just the uncertainty of the analysis? It seems that you could easily compare the variability between the three recent ones with the difference across time.

P668 26ff: You give a lot of citations of differences of BC with depth but no hypothesis or explanations about reasons. Possible explanations would be helpful to rule out the concerns referring to transport of BC out of the profile. I read in the cited Lapenis 2008 paper that the soils became leached and I read about a strongly increased carbonate leaching. Can the BC be leached out of the profile together with the carbonate or is it just destabilized? On the other hand, if I assume a downward transport of BC and carbonate with increased precipitation then I would expect an opposite (downward) shift

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BC stocks in the profile. Did the intensity of bioturbation change with the management change?

P669 L15ff: You write about quality of BC, but in your text you write mostly about the degree of condensation. In the context of this paper I recommend to write more about recalcitrance or degradability which is associated with the condensation and the chemical analysis. I found it hard to translate the BXCA labels to the context of quality and recalcitrance.

Fig. 3: A legend of the fills or annotations "1900" and "1997" directly in the graph would be really helpful. In the caption or legend indicate that B6CA is a slowly degradable fraction of BC.

P669 L24ff: You state an increase of proportion of B6CA, but Fig 3 shows this only for the upper 50 centimetres and the opposite below.

P669 L25: "is evident". With current labelling of Fig.4 I had to assemble several pieces from the text together to follow your thoughts.

Fig. 4: Give a legend of the fills that matches the other description of the profiles. Do the light columns represent 1900 soil and the dark columns the 1997 or 2004 soil? On the x-Axis indicate the gradient of condensation and degradability. Part of the Y-axis label is clipped.

P670 L2ff: I obtain different results. See my comments on P667 L12. If the scenarios are described more clearly, they do not need to be repeated here.

P672 L3-8: What are the implications of calcium loss for the presented results and the conclusion? Is this an alternative for the decay or does it explain the observation of a fast decay? Will BC at locations without calcium loss have longer turnover times?

P673 L2: Before other explanations of the observed difference in BC stocks are ruled out, I would recommend to put the conclusion with more caution such as "it is likely that BC ..." or "based on the assumptions that the decay of BC is the single cause for

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the observed decrease of BC stocks ..."

### Technical corrections

The DOI: 10.1029/2007GB003077 was not found by www.doi.org given with reference with Lapenis, A. G., Lawrence, G. B., Baily, S., Aparin, B. F., Shiklomanov, A. I., Speranskaya, N. A., Torn, M. S., and Calef, M.: Climatically driven loss of calcium in steppe soil as a sink for atmospheric carbon, Glob. Biogeochem. Cy., doi:10.1029/2007GB003077, in press, 2008.

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