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Interactive comment on "Strong warming of subarctic forest soil deteriorated soil structure via carbon loss – Indications from organic matter fractionation" *by* Christopher Poeplau et al.

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In large this is a good and interesting paper. However, there are some shortcomings with regard to these types of studies; The authors themselves have discussed problems connected to interpretation of results from experiments using geothermically warmed soils in a global warming / climate change context, concerns that I also share. Though I am not convinced that the study is bringing us much forward in questions regarding the fate of carbon in subarctic forest soils in future warmer climates, I still see the relevance with regard to the fate of soil carbon in geothermically warmed Andesitic soil. I believe the most important results in this study are the relative changes in proportion of C in

the different fraction with increasing temperature and this should be more in focus than the loss of C and deterioration of soil structure. I also think that the differences between the two ecosystems (grassland and forest) should be better communicated in the tittle.

Answer: We thank the reviewer for the valuable comments and suggestions. The title has been adjusted to i) narrow the focus to Andosols and ii) include grasslands. Regarding the focus, we see that loss of C, fraction distribution and soil structure are closely coupled in this case and we tried to make exactly this point in the study.

More specific comments

1. Does the paper address relevant scientific questions within the scope of SOIL? yes

2.Does the paper present novel concepts, ideas, tools, or data? the paper presents new date, but is not particularly novel in concept, ideas and tools.

3. Does the paper address soils within a multidisciplinary context? Yes, in a using geothermic warming of soils as a proxy for warming of soils in climate change scenario. But does not address the ecosystem changes/ vegetation as much it ought

4. Is the paper of broad international interest? Yes, but not as broad as the title suggests, these are Andesitic soil and thermal warming of soils do have some limitations with regard to interpretation in a global change context.

5. Are clear objectives and/or hypotheses put forward? Yes, three objectives are stated. 1.advance our understanding of the temperature response of different SOC fractions representing kinetic pools 2.assess the role of the ecosystem type in the temperature response of SOC 3.investigate potential links between SOC loss and so il structure changes.

6.Are the scientific methods valid and clear outlined to be reproduced? I have some questions with regard to sampling and interpretation of the term soil structure, see below.

7.Is the soil type/classification adequately described? fairly, general information on soil type/classification at the experimental site is given. but I cannot see that the information that the soil type/classification provides is actually used in the interpretation of the results. Though only the upper 30 cm is used in this study it would have given valuable information if this was related to soil horizons.

Answer: The whole study design was not related to genetic horizons, but to fixed depth increments, which is a standard e.g. when different ecosystems are compared.

8.Are analyses and assumptions valid? see comments below

9.Are the presented results sufficient to support the interpretations and associated discussion? see comments below

10.Is the discussion relevant and backed up? see comments below

11.Are accurate conclusions reached based on the presented results and discussion? Yes

12.Do the authors give proper credit to related and relevant work and clearly indicate their own original contribution? Yes

13.Does the title clearly reflect the contents of the paper and is it informative? See comments below

14.Does the abstract provide a concise and complete summary, including quantitative results? Yes

15. Is the overall presentation well structured? yes

16.Is the paper written concisely and to the point? OK

17.Is the language fluent, precise, and grammatically correct? long cumbersome sentences make particularly the discussion, but also elsewhere. You are really not making it easy for the reader when you write sentences like this e.g.: Page 9 lines 30 to 33

" Thereby, topsoil and subsoil samples scattered approximately around the same regression line, indicating that especially the amount of young SOC, may it be driven by warming intensity or the location within the soil profile, was a very good predictor for the amount of stable aggregates in the soil." Or page 11 line 24-26: "Together with the fact that also more labile SOC was present in the forest topsoil, which responded more sensitive to warming than the soil, it seems likely that amount and fraction distribution of SOC drove the ecosystem specific warming response in the topsoil."

Answer: We agree that some sentences, especially in the discussion, were too long and reworked those. The mentioned sentences now read as follows: "Topsoil and subsoil samples scattered approximately around the same regression line. This indicates that the abundance of young and coarse SOC per se, rather than the degree of soil warming, is driving the amount of stable aggregates in the soil." And "Therefore it seems likely that amount and fraction distribution of SOC drove the ecosystem specific warming response in the topsoil." We further changed the sentence "In consequence of SOC loss, total pore space decreased strongly as indicated by poured bulk density used as a proxy for bulk density in undisturbed samples. The latter was unfortunately not determined in the present study." Into "In consequence of SOC loss, total pore space decreased strongly as indicated by poured bulk density. Poured bulk density was used as a proxy for in situ bulk density in the undisturbed soil, which was unfortunately not determined in the present study." And "While the authors did not explicitly link that to changes in soil structure, an increase in bulk density with associated decrease in pore space might have fostered that change." Into "An increase in bulk density with associated decrease in pore space might have fostered this physiological response, although this was not explicitly mentioned by the authors."

18.Are the figures and tables useful and all necessary? Yes (see point 20)

19.Are mathematical formulae, symbols, abbreviations, and units correctly defined and used according to the author guidelines? I believe so

20.Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? 1.most of the text is relevant and to the point, but I am unsure if the “poured bulk density - soil structure” part contributes in a meaningful way.D eleting it and giving more focus to the warming effects on fractionation -ecosystem comparisons stable soil C would benefit the paper.

Answer: We do not necessarily agree on that point, and for example reviewer 2 was especially in favour of that part. We therefore decided to keep the focus as it is, although we clearly acknowledge that the proxies used are not enough to fully understand the SOC effect on soil structure in the studied soil.

2.Most of the figures are nice and useful. Figure 5 must have the wrong Y-axis tittle – subsoil not topsoil SOC. However, I wonder if it would be possible to add the warming (some sort of colour code) to Figure6. It would be interesting to see if there were any systematic also with regard to temperature.

Answer: Figure 5 has been changed. In Figure 6, we think that giving the different warming intensities would over-complicate the figure. It already consists of 4 panels and thus a high information density. In the plots before, we show how warming and SOC are correlated, so this would be redundant information to some degree.

3.I miss a table showing general soil properties such as pH, oxide extraction of some sort (Fe, Al, Si), sand silt clay%. I am not too keen on table 2 and 3 which only show summary of statistics, they are much more valuable when they are connected to measured properties, merge or delete?

Answer: Soil pH, texture and initial SOC contents are given in the text describing the experiment. Fe, Al and Si contents were not measured. Also, some more information, e.g. on soil temperature profiles, are given in the mentioned publication which is a pure site description publication. Therefore, we think that an additional table is not necessary. We also think that both, table 2 and 3 are necessary and would like to keep them.

21.Are the number and quality of references appropriate? Yes

22. Is the amount and quality of supplementary material appropriate and of added value? as far as I can see yes

I have a couple of more specific comments that I think the authors need to address to improve the paper

1)I think the use of the term “soil structure” is used wrong. Soil structure has not been investigated in this study. What has been studied is stable aggregate (SA) (63-2000μm) from the fractionation procedure and the carbon (C) connected to this fraction. The natural soil bulk density (BD) was not measured – which could have given some indication of soil structure and the “poured bulk density” does not replace the measurement of the natural BD, though it does give a relative difference between dried soil material in the fraction less than 2 mm. I therefore suggest a change in tittle. e.g.: Âń Strong warming of subarctic forest soil reduces stabilisation of carbon in soil aggregates – Indications from organic matter fractionation”. I would also suggest including“ subarctic andesitic forest soil” in the tittle as these soils normally show both chemical and physical properties that are markedly different from “subarctic forest soils” formed on other parent materials.

Answer: We agree that using the word soil structure in the title is misleading, or leads to different expectations. We also agree that the title was too brought, since we investigated a very specific soil type. Therefore we changed the title to: ‘Strong warming of a subarctic Andosol depleted soil carbon and aggregation under forest and grassland cover’.

2) Sampling method and comparisons, sampling should be better explained, and it would also be nice to know how many samples (N=) were behind each temperature/location? In studies like this sampling method and a good description of these are crucial. Many sophisticated analyses in the laboratory will never compensate for errors, flaws and inaccuracy in sampling– or description of sampling. I am afraid that

the lack attention to the sampling procedure maymake the results of this study none reproducible. Personal experience with similar sampling schemes suggests that a soil of this nature (Silandic Andosol with a silt loam texture, 60 % silt) is easily compressed during sampling. How were the depth intervals determined? If sampling was done as I anticipate by extracting a 30 cm cylindrical core and then splitting it into 0-10, 10-20 and 20-30 samplesthis could be a real challenge. The comparisons between the different layers could be based on pure artefacts - please convince me of the opposite

Answer: The amount of samples has been introduced in the section 2.1, which was renamed from study site to study site and experimental design. However, we also added the number of warming intensities and replicates to the section 2.2 and the sentence now reads as follows:'In late April 2018, i.e. almost exactly 10 years after the warming was initiated, mineral soils of all permanent forest plots (six warming intensities, five replicates each) were sampled.' Regarding the sampling and potential artefacts: We used a thin auger that is not necessarily suited for volumetric sampling, but has the advantage that soil compaction is relatively low. However, as the reviewer mentioned correctly, this kind of soil is easily compressed. This is especially true for the warmer treatments that are less stabile due to less SOC and less aggregates. Changes in soil structure and structural stability were thus already noticed during sampling. The procedure was the following: The auger was always driven into the soil to a depth of 30cm. When extracting the auger with the respective soil core, the soil core was several centimeters shorter than that $\sim$27 cm. In this case we split the core into intervals of 9cm, assuming that compaction happened linearly. In this way, we avoided strong artefacts related to compaction, because 0-9, 9-18 and 18-27 cm in a compacted core should come relatively close to 0-10, 10-20 and 20-30 cm in an uncompacted core. To clarify this, we added the following sentences: 'In case of soil compaction within the auger, the increment depth was adjusted linearly. For example, a compaction of three cm over the whole soil core resulted in a sampling of 0-9, 9-18 and 18-27 cm increments.'

This brings me to my next point – if the warming of the soil has caused changes in the

soil density particularly in the "top soil" this would cause the sampling at the warmer place to go deeper into the subsoil extracting soils that naturally (before the shift in geothermal flow) had a lower content of C. This would then be compared to the lower layer of the "none" warmed soil and we would wrongly conclude that the warming has caused loss of carbon?

Answer: Again, we agree with the reviewer that this is a problem to a certain extent: Shifts in SOC contents lead to shifts in bulk density, which should –in an optimal world- be considered before sampling. However, the difficulty is that such changes in bulk density are usually not known before sampling, so how can depth increments be properly adjusted? The only way is then to use a defined volume (e.g. a metal frame) and conduct already the sampling by equivalent soil mass (the approach that is mentioned below). Accounting for equivalent soil mass in the field is extremely elaborate and therefore rarely done. More importantly, it is based on fine soil (<2mm) of course, because this is where the carbon is. In this young, volcanic soil, the rock fragment fraction is extremely variable and partly very high (especially in the grassland soil >10cm). A sampling based on equivalent FINE soil mass is therefore not possible in this soil, and also bulk density values (that have been measured before) are hard to interpret. Nevertheless, the reviewer brought up an important point that is ignored in many studies measuring gradients in SOC contents (or even stocks). We have one argument that the observed loss of SOC is mostly really related to the warming effect: Along the soil profile of grassland and forest soils, the strongest gradients in SOC content with depth occur in the upper cm. The deeper in the profile, the less steep the gradient. This means, that a bulk density related sampling bias would show up most extreme in the topsoil (i.e. the relative loss of SOC should be higher in 0-10cm than in 20-30cm). However, in our case the opposite was the case: Relative losses were more pronounced in the subsoil. We now address this issue in the following sentences in the discussion: Also, those structural changes did most likely lead to a certain sampling bias and thus a slight overestimation of SOC losses: A sampling of fixed depth increments ignores the fact that depth increments change with changes in bulk density. Therefore, the

depth increments sampled in the higher warming intensities do not exactly match the depth increments sampled in the lower warming intensities. However, this effect is expected to be more pronounced in the topsoil, were the SOC depth gradient is largest and thus a shift in reference soil depth would have the strongest impact on bulk SOC content. However, relative losses in SOC were even more pronounced in the subsoil, indicating that the sampling bias was might have been small. However, it should be mentioned that a mass-based instead of a depth-based sampling (Don et al. 2020) or at least an a-posteriori soil mass correction (Ellert and Bettany et al. 1993) would be indispensable to accurately estimate SOC stock changes."

Bringing me to my third point –as one of the main objectives of the study clearly is to assess losses of C and also quantify the losses, we need to be sure we that what we compare are comparable. Studies like this should be done by comparing equivalent mass (See Ellert, B. H. and J. R. Bettany (1995). "Calculation of organic matter and nutrients stored in soils under contrasting management regimes." Canadian Journal of Soil Science 75(4): 529-538 or others more recent paper). Also the 10-20 cm was not analysed, understandable from a resource point of view (many time-consuming and expensive analysis), but a simple analysis of SOC + weighing of the total dry sample would have added valuable information particularly for interpretation in a climate change context.

Answer: It is correct that for calculating SOC stocks in 0-30 cm, we would need the full picture: bulk densities and SOC contents in all three depth increments. We could then also apply the mass correction as suggested by Ellert and Bettany. However, the study was not designed to measure stocks, which has its own difficulties in this specific soil, but contents. Rock fragments were not determined, total soil mass was not measured. We also think that relative loss in SOC content comes close to a relative loss of soil mass corrected SOC stock (because the differences in bulk density and total fine soil mass will be vanished by mass correction).

The most important results in this study is the relative change in proportion of C in the

different fraction with increasing temperature and this should be more in focus than the loss of C

Answer: The manuscript has three clearly formulated objectives and we think that each of these aspects is important. Also, in the discussion we argue that the change in C fraction distribution and structure change (i.e. aggregate breakdown) is directly related to C loss, it is thus hard not to focus on C loss as such, although it is only based on contents.

All analysis were done on the fraction < 2mm, but it would have been interesting to know the proportion of the coarse fraction.

Answer: This is correct and we think the same, therefore a comprehensive aggregate fractionation (including aggregates >2mm) is ongoing, at least for the grassland. Here, we used the chance to apply the very same fractionation procedure as used in an earlier publication in the grassland soil also to the forest soil. Only in this way, a direct ecosystem comparison is possible.

3) The study appears to focus on loss of C from the soil with warming, however there is little information in change in input of C to the system. Warming of the soil may have had an influence on the forest growth/productivity and litterinput. Some more information on the vegetation would have been appreciated. In this study the litter /O horizonis removed – it would have been nice to at least know how thick it was at the different locations? Several papers have been written with data from this experimental area– surly some information could have been extracted from these not just giving references and leaving the reader to find out for themselves. Additional information on mineralogy/alternatively selective extraction of different oxideswould also aidthe discussion on C stabilization mechanism. Also, DOC and pH normally are correlated – it would have been nice to have some pH measurements to go with the top and subsoil samples. As this warming is by geothermal heating, I am naturally curious to how this is affects the the top- and subsoil, are there any effects on soil moisture, any gradient between the

two layers.

Answer: Yes, there was also a vegetation response, which could partly drive SOC losses. However, estimating C inputs to the soil is extremely complicated in this setting, because not only productivity, but also biomass turnover needs to be estimated. Those studies are underway in both ecosystems and are the focus of two PhD projects. Concerning this, we have added the following sentences, using unpublished data from the database (section 4.1): “In fact, root biomass in 0-10 cm decreased in both ecosystems (data not shown), leading to weak positive correlations (R^2=0.37 for forest and R^2=0.29 for grasslands) of SOC and root biomass. Also aboveground plant litter tended to decline in both ecosystems. This suggests that SOC losses were partly driven by decreasing C input with warming and not by increased microbial activity alone. However, a clear picture on absolute C inputs in the experimental plots is not available yet, since it needs to consider NPP and biomass turnover at the same time.” Much of the data that is needed to understand the whole picture better is still in the pipeline and unpublished. As for mineralogy and oxides, we must admit that those have never been measured so far. Surely this would be interesting, but in our view there will be only minor changes in mineralogy with warming, so it might not be the most important parameter to explain the responses in this study. Regarding soil pH: We agree that an increased pH could also explain that observation. We have now added the following sentence: “However, also soil pH is acknowledged to affect DOC formation (Kalbitz et al., 2000), which might be another possible explanation for the observed increase in the proportion of DOC: in both ecosystems, soil pH increased by up to 0.5 units in the highest warming intensity (Sigurdsson et al., 2016).”. Soil moisture (only in 0-5 cm) has been measured in both ecosystem between April and August 2016. Interestingly, the difference between treatments was small and not related to warming. Data are presented in Sigurdsson et al. 2016 Journal of Icelandic agricultural sicences. We do not have information on a potential depth gradient.

4) The use of the term “topsoil” and “sub soil” when this refers to 0-10cm (topsoil) and

20 -30 (sub soil) is ill-conceived. I normal soil terminology both these layers refer to topsoil. Readers with an interest in C subsoil – non surface layers will perhaps be misled. Why not simply us “Upper” and “Lower” or even better were there any genetic differences A horizon – B horizon?

Answer: We agree that the term subsoil is potentially misleading when talking about soil within the top 30 cm of soil. However, the investigated soils, especially in the grassland, were very shallow and of course there are soils with shallow subsoils. We think that this is a matter of definition and define top and subsoil already in the abstract. Therefore we do not see that a change from topsoil to subsoil into “Upper soil” and “Lower soil” or “Upper” and “Lower” would improve the manuscript. Also, we did not sample along genetic horizons, especially not representatively, therefore we cannot change the fixed depth increments into pedogenetic horizons. By the way, pedogenetic horizons are also very hard to determine in these young Andosols.

5)Ecosystem comparison: I believe there should have been more focus on differences in input of C. You observe differences between the ecosystems only in the topsoil – ascribing this to the fact that the forest was planted on former unmanaged grassland. However, you also find that the forest soil has a more pronounced depletion of Cin the subsoil. Could also part of the explanation also be due to the fact that warming was geothermal – from beneath. In a situation where global warming (air warming) is the case the differences in the topsoil would be equally reflected in the subsoil.

Answer: Warming is coming from below, that is correct, and it might to some extent explain the relatively stronger response. This was already part of the discussion (section 4.1). But why or how could it explain that there is a distinct ecosystem response in the topsoil but not in the subsoil? For us, it is more likely that this is due to the fact that forest and grassland subsoils were generally very similar (in SOC and fraction distribution) and so responded also similarly to warming.

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