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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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Dear Christopher, Páll and Bjarni It was a pleasure to me to find a manuscript which deals with a very similar fraction- ation approach and which presents an Icelandic study. Hence, it is very similar to my research which I did in Iceland some years ago. My memories from the southern part of Iceland came back... Therefore, I read the manuscript of your study and would like to give you my comments.

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Discussion paper

1) It is ingenious to use a natural heat source and the resulting warming gradient to study ecosystem changes. This is likely possible on a volcanic island like Iceland. But on volcanic islands, soil properties differ from other soil types of non-volcanic regions in the boreal ecosystem. How much are your results applicable to the rest of the boreal ecosystem? What could be the limitations?

Answer: Each case study has its limitations and should not be extrapolated to whole biomes. This is true when talking about absolute rates of change, while the direction of change or certain response mechanisms might well be more generic. The soil of this study is a quite specific one, and we agree that we have not accounted for this enough in the previous version. We now mention the word Andosol in the title to clarify the focus.

2) You mentioned that the worst scenario in the IPCC report predicts a temperature increase of 11°C for the region North of 60°N. Your temperature sequence ends at +17.5°C. Can you link your results to the different IPCC scenarios with regards to the maximum predicted temperature increases in the IPCC report? Can you make any assumption, how the SOC change would look like at +11°C (IPCC report) based on your temperature sequence (5.8°C, 17.5°C). By the way, does the referenced temperature increase (+11°C) corresponds to the air temperature? In this case, how do you link the increase of air temperature to the increase of soil temperature? Does soil temperature increase in the same way and with the same slope gradient in the future and therefore does your soil warming gradient corresponds to the assumed soil temperature increase in the future for boreal ecosystems? Please clarify this in the introduction part.

Answer: This comment includes a few important remarks at the same time. Of course, soil warming is not equal to air warming. The difference between soil temperature and air temperature is site specific (surface energy balance) and therefore it is hard to predict how much air temperature increase is necessary to increase the average soil temperature e.g. by 5.8°C at a specific site. Also it is clear that the source of warming may have a strong effect on the overall ecosystem response. However, based on the

most pessimistic IPCC extreme scenarios, we think that at least a soil warming of 5.8°C is within this (maximum) realistic range. We have now been clearer in the discussion when we talk about warming if we refer to air temperature or soil temperature (also because one reviewer requested that). However, the introduction is not the place to discuss shortcomings of geothermal warming and also not how we can link our results with any IPCC scenario or how soil temperature will increase in the future. There is a lot of uncertainty involved, especially regarding the long-term effects of soil warming. And this is the reason why we mention (in the introduction) that long-term warming experiments are needed. Also, the warming response of bulk SOC was linear, so it is easy to derive a number for any of the suggested warming scenarios, if you wish so - with high uncertainties: only one soil, soil warming not air warming, warming from below, very abrupt warming vs. long term gradual warming... Thus, we do not want to speculate too much and go too far into this direction and see the strength of this experiment more in the fact that we have strong and long soil warming to study mechanisms.

3) Based on Comment 2, could you make any statements about the impact of temperature increase on SOC regarding the different IPCC scenarios (e.g. SOC change according to the smallest temperature increase).

Answer: For mentioned reasons (in answers 1 and 2): No. Recent papers (e.g. Crowther et al.) tried to predict changes in SOC until the end of the century or until 2050 based on short-term warming experiments. We think that this is problematic for several reasons and do not dare to directly conclude from our still relatively short-term warming experiments what will happen under global change in the long run.

4) In your study the temperature increased within 10 years. The modelled increase of air temperature will however change within several decades. What would you think, is the different time scale irrelevant concerning the change of SOC and the soil processes which control the SOC?

Answer: This is a good comment and it concerns basically all climate change manipulation experiments (temperature, CO2 conc., drought, ozone, UV, etc.) that have been conducted globally. No, it is surely not irrelevant. We might see an overshoot-reaction here, or things might level off soon. We have indications from different ForHot papers that such responses can occur for when the 10 year warming is compared to >50 year warming in the same grassland type. This is actually the focus of a Nature Ecology and Evolution paper on that exact aspect soon where a large range of ecosystem parameters are compared (Walker et al., in press). The main message is: Be careful with linear extrapolations of short-term warming experiments. The doubts that you mention in your last 4 comments are all correct, but I think at the same time it is pretty clear that our experiment cannot solve the equation for all the boreal zone, all IPCC scenarios and until the end of the century. To clarify this, we added the following sentence to the discussion (p8, l.37ff: "However, the transferability of the results in this study to the SOC response to global warming is still rather limited and can only slightly reduce given uncertainties: i) we studied soil temperature, not air temperature increase, ii) the warming occurred abruptly and not gradually, iii) we studied an Andosol. Extrapolations to larger areas or longer time periods should thus be done carefully and were not intended with this study."

5) Is the vegetation (grassland or forest ecosystem) also changing during the warming within these 10 years? If there is also a change in the vegetation composition or the supply of OM to the soil phases, might these changes also be responsible for the changes in your SOC results? In this case, the increase of the temperature is not the only independent parameter that changes.

Answer: Yes, there were some changes in vegetation, while those were most pronounced in the most extreme warming intensities >15°C: For example, in the forest many trees died after this extreme and abrupt soil warming event. However, the changes in SOC we see are very gradual (along the temperature rise, so this is not reflected in the SOC data). Also, there is more understorey herbaceous vegetation

now in the warmest treatment due to the canopy reduction, so the input of C might not even be smaller. However, for both ecosystems we actually detected a decrease in root biomass and (although less pronounced) also in litter biomass. There are ongoing studies on if the turnover rates of those roots also change, but those are still unpublished. It is thus quite realistic, that changes in SOC are also partly driven by decreased C input, although biomass turnover is not comprehensively measured yet. So yes, temperature increase is the independent parameter that changed in the very beginning, but then there is a whole chain of mechanisms that could lead to changes in SOC and SOC fraction distribution. We don't go into too much detail here, because that was not the focus of the study and data availability is partly scattered yet. However, we added the following sentences (in 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."

6) Discussing the change of soil structure and SOC content (within SOC fraction), you might also need to have a look at the soil mineralogy. Did you analyse the volcanic clay minerals? Volcanic clay minerals and the abundance or change of metal-humus complexes, allophane content, ferrihydrite content can also explain the changes within more resistant SOC fractions.

Answer: This would surely be an interesting thing to do, especially for a better characterization of the soil, but was not done in this study. We acknowledge that those Andosol-specific characteristics are important, but we think that i) temperature changes were not high enough to change soil mineralogy substantially and ii) mineralogy is more related to SOC stabilization mechanisms in the finest fractions (including microaggre-

gates), while here we observe the strongest changes in the larger fractions. We believe that the response that we observe (i.e. a break-down of large micro- and macroaggregates) is more controlled by biotic than by abiotic drivers. Organo-mineral interactions and aggregates in Andosols are known to be particularly strong and almost impossible to disintegrate, but what we see here is a break-down of aggregates due to warming. It seems unrealistic, that this related to the specific mineralogical features of an Andosol. But we agree that further studies should include analyses that can be used to quantify metal-humus complexes, allophane content, ferrihydrite. The plan is to get your friend prof. Ólafur Arnalds involved in the near future to add those aspects to a further study.

7) What do you suggest to use as a further analysis technique to characterize the stable SOC fraction (rSOC) or the <63 microns fraction? I ask you, because I used the same fractionation technique and later, however, read that the wet oxidation step is questioned. Spontaneously, I would measure the SOC and the volcanic clay minerals in the <63 microns fraction to get an idea about the characteristics between the SOC and the soil mineralogy. Do you have any further ideas which approaches can be used when the SOC of volcanic soils is fractioned and characterized?

Answer: There are different ways of wet oxidation and the one used in this study (with NaOCl) was found to be related (in previous studies) to the amount of extractable Fe and Al in the soil, so directly to soil mineralogy, which makes sense. It seems to be those organo-metal complexes that resist that oxidation step and that form the most stable SOC. The derived fraction rSOC is mostly found to be the one that shows the least response to any change or novelty, therefore we think that wet oxidation (especially with NaOCl) gives meaningful results to some extent. But, indeed it remains a mystery why this is not the case for warming (or at least in this experiment): rSOC shows pretty much the same response as the silt and clay fraction as a whole. If this is related to the type of soil or the type of treatment (warming) remains open. Adding mineralogy data to fraction data could always be helpful, but not sure if it will help to solve that specific question. Thermo-stability, as assessed using techniques like Rock-Eval

can also be promising to determine the amount of persistent SOC- with its own methodological difficulties. We have slightly changed the section on this fraction in section 4.1: "This has been observed before and questions the notion that this oxidation-resistant pool can be linked to a centennially persistent or even inert SOC pool (Lutfalla et al., 2014;Poeplau et al., 2019;Poeplau et al., 2017;Zimmermann et al., 2007). At the same time, NaOCl-resistant SOC has often been described as substantially older and thus slower cycling as bulk SOC (Helfrich et al., 2007) and was also found to correlate to the abundance of Al and Fe-oxides in the soil (Mikutta et al., 2005). Thus, the strong warming response of this fraction is somewhat in contrast to the slow responses observed to other treatments, such as C3-C4 vegetation changes (Poeplau et al. 2018)." Additional comment from Bjarni: We recently got some 14C analyses from the topsoil and subsoil and also from the <63 microns fraction in the grassland sites. It unexpectedly showed only minor age differences... For me this seems to indicate that in our soil/ecosystem we are having a very high turnover of SOC, which is also in line with what we are finding... Again, this will be addressed in more detail in future paper, where we will look more closely into those dynamics and also involving the mineralogy.

Statistics 8) How many replicates do you have per category (e.g. Fig. 4)? Is it n=5? Can you mention the number of samples per category and the number of samples within the two ecosystem datasets in section 2? It might be also useful to mention it in the capture of Table 1.

Answer: It is 5 and we have now entered another sentence on that in the soil sampling section (although it was already mentioned before in the experimental design section). Also in the table capture of Tab.1 we included it now, thank you.

9) Mention also p-values in the text when giving the regression values (e.g. page 6 line 31, Figure 6). I remember the reviews of my manuscript

Answer: Done.

10) In Figure 4 and Figure 5, what does the intervals along the regression lines indi-

cate? Is it the 95% range of the regression value or the 95% range of the modelled value?

Answer: It is the 95% confidence interval of the regression model- this information was added to all Figure captions.

11) Changes of ecosystem processes are not always linear. Figure 4 and Figure 5 show distributions of values which could be modelled more accurate with a non-linear function. For example, the patterns of the contents of bulk SOC, SA, POM or rSOC show different slopes along the temperature gradient and asymptotic properties. Did you tested other types of functions to explain the patterns of SOC changes? There might be also a non-linear correlation in Figure 6 D.

Answer: We admit that it was a bit oversimplified to assume linearity in all cases. We now tried logarithmic fits as a second option and indeed, it did fit better in several cases. Figure 4 and 5 were adjusted and so was the Statistics section. However, in Figure 6, a linear fit was much better – the optical non-linearity might originate from the three points at the lower left corner, while there is a wealth of observations much higher than those. We therefore did not adjust Figure 6.

12) In the case of a non-linear pattern, is it useful to show only one absolute change value (g C kg-1 fraction°C-1)?

Answer: We agree that using this one value is again oversimplified in case of a clear non-linear response. However, i) the values used in the abstract (and directly in the text) refer to linear responses of topsoil and subsoil bulk SOC and ii) the values given in table 1 (for the fractions) are used as measures to directly compare the response of different fractions. In this sense, we think it is useful to give also these values. The only other chance we see is to use the difference between the unwarmed reference and the highest warming, but we think that a linear fit is a more fair comparison, since it does not overemphasize those two values. We have now added the following sentence to the table heading of table 1: “Although this was not the best model in all cases, we

used this value as a proxy to compare the warming response among fractions."

13) In the case of a non-linear pattern, when do you expect the highest rates of change? Is this in the beginning of the warming or at the end? What does this mean for the change of the boreal ecosystem and at what time do you expect the highest changes (in the next 10-20 years or in 80-100 years)?

Answer: This is a different question: We looked at the response to a temperature gradient; you are now referring to the temporal pattern. This is a different story and very uncertain (as mentioned earlier). The dataset investigated here cannot be used to answer this important question, but we might have indications that the loss actually levels off after the first years (mentioned in the discussion) – and the paper on this issue involving many different ecosystem processes and characteristics is in press in Nature Ecology and Evolution (Walker et al.)

14) I offer you to read the reviews of my manuscript. The study deals also about SOC fractions in volcanic soils in Iceland and some comments might be also useful for the revision of your manuscript. https://www.soil-journal.net/5/223/2019/soil-5-223-2019-discussion.html

Answer: Thank you.

15) In this journal, square brackets are not used to note any units (e.g. [°C]). It uses parenthesis (°C).

Answer: Thank you, we have changed it everywhere.

16) I guess that Figure 5 shows the scatterplots for the SOC contents in the subsoil (20-30 cm). Please change the title in the Y-axis.

Answer: This is correct and was changed accordingly.

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