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Comment on bg-2021-338

Emma Sayer (Referee)

Referee comment on "Soil carbon loss in warmed subarctic grasslands is rapid and restricted to topsoil" by Niel Verbrigghe et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-338-RC1, 2022

The manuscript presents a study of SOC stocks to 30-cm depth in a unique long-term soil warming study based on natural geothermal gradients. The manuscript presents differences in SOC stocks along warming gradients at sites that had been warmed for 5-10 years (medium-term) or 5-55 years (long-term) at the time of the study. The key finding is that C stocks in the topsoil declined with increasing temperature after 5-10 years of warming, but not after 50-55 years, and subsoil C stocks (long-term site only) did not differ with temperature.

I read the manuscript with great interest, as data on changes in subsoil C stocks are rare, as are such long-term climate change experiments. The topic is certainly appropriate for Biogeosciences and although the overarching research questions and hypotheses are interesting, I am not entirely convinced that the present study address them in full. First, the chosen soil sampling depth needs to be justified, as many researchers would not consider 10-30 cm depth as 'subsoil'. Second the 10-30 cm depth was only sampled at the site with long-term warming. So it is entirely unknown whether the deeper soils experienced similar short-term effects to the topsoil. In addition, there are a few key points that should be considered in the interests of clarity and scientific rigour:

First, the language of the present manuscript is somewhat misleading because 'SOC losses' are referred to throughout, but actual C loss was not measured. The space-for-time approach used in the study demonstrates differences in SOC stocks between plots that have been warmed to varying degrees for different lengths of time, which is not the same as measuring C losses. The language in the text should be edited to reflect this.

Second, the reasoning behind the hypothesis of long-term warming needs a justification – is 50 years sufficient for a new equilibrium to be reached? And why would you expect the new equilibrium to be reached at lower SOC content? Theoretically, maintenance of SOC stocks might also be predicted over the longer term if, e.g. acclimation of microbial communities and C turnover rates, increased plant productivity, or declining nutrient

availability with long-term warming eventually compensate for initial losses… A rationale for this hypothesis could be provided by drawing on previous research from the site (currently discussed on lines 76-85).

Third, the introduction states clearly that the processes involved in SOC formation and mineralisation are rarely studied below 20-30 cm depth, which sounds like a justification to study subsoils below 30-cm depth - so why was only 0-30 cm considered in this study? The justification for the split between 0-10 and 10-30 cm is given, but it does not explain why the 10-30 cm increment should be considered as subsoil, nor whether it is likely to be representative of subsoil at greater depths. Indeed, subsoils are often considered as being below 30-cm depth.

Finally, I'm not entirely convinced by the argument that C inputs did not increase with warming. If I understand correctly, the evidence for the lack of changes in C inputs (presented in figure B3 and B5) is based on measurements and samples collected after the first 5 years of warming. So how do you know there were no short-term changes in C inputs during the first 5 years? The major losses in the first 5 years could be an artefact of the sudden increase in soil temperature – what precludes a similar short-term increase in plant inputs? At the long-term site, I also wondered whether gradual change in temperature and growing season length over the last 50 years could have partially compensated for early C losses?

Data analyses

The data analysis section should clarify how the data were handled and what was considered a replicate. In the methods section, the transects are referred to as replicates but the 6 plots per transects represent different temperatures. I therefore assume that the models are based on plot-level data. At the very least, transect or sampling plot should be included as a random effect in the models (ideally plot nested within transect to account for the experimental design). Including location in the models could help deal with the high variability. In addition, figure 1 shows regression lines but no regressions are described in the analyses.

I note that these issues do not detract from the interesting and potentially important findings on the differences between soil depths and long- vs. short-term warming. However, the presentation ad discussion of the results should be revised to ensure the main messages are accurate and the limitations of the study are clear.

With kind regards

E.J. Sayer

Additional minor comments by line:

L15 – suggest replacing “lead to increased” with “increase”

L17 – what is meant by “sign”? Do you mean whether the feedback is positive or negative? This could be rephrased to make it clearer.

L20 – omit soil before SOC

L22 – extrapolations of responses from, or model parametrisation based on, short-term experiments

L27: above it

L57: lose SOC (not ‘loose’)

Line 67 states: “Even grasslands that had been warmed at least 55 years exhibited no larger SOC loss than that observed after 5 years of soil warming.” How were SOC losses over 55 years assessed without analysing samples from 55 years ago? Or does this instead mean that the SOC stocks in the long-term warmed transects were similar to those in the medium-term transects?

L81-83: you present no data for microbial communities in this study, so upon what basis do you infer there is no evidence for physiological adaptations or compositional shifts?

L103-106: The differences to the other studies referred to here could be partly explained by differences in sampling depth, e.g. Lin et al. considered soils to 60 cm depth, Soong et al considered soils to 1-m depth and even Jia et al. considered 30-40 cm. In addition, I believe that all of these studies focussed on early or short-term changes in SOC, which were not considered for the subsoil in this work.

L165 states that medium-term warmed soils were too shallow to sample deeper than 10

cm and yet lysimeters were placed at 30-40 cm depth (L177). If there were at least some sites with deeper soils in the medium-term transects, why were they not sampled?

L130: incorrect spelling of Agrostis

Figure 1: There seems to be a bias towards more plots at the lower end of the warming gradient, whereas at the hotter end of the gradient, it looks like there are only 3 plots – It would be useful to give an indication of the spread of the data along the warming gradient. If most soil C loss occurred during the first 5 years of warming, why does the regression include data from both sampling times? It also looks like not all plots were sampled at both times – is this correct?

Figure 2: The smoothing lines are misleading, because they imply "no change" between 10 and 50 years, for which there is no evidence

Figure 3: Are these the data for topsoil C fractions? Please clarify in the legend. Are there fractionation data for the subsoils?