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Reply on RC2

Niel Verbrigghe et al.

Author 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-AC2, 2022

General comments

I was attracted by the observation system set up to study the effects of global warming on SOC stocks and globally by the results obtained.

Beyond its originality, the observation system built offers above all a serious opportunity to study the response of C stocks in grassland soils both in the medium and long term, in a solid manner. Indeed, the observations were carried out in situ on local warming gradients applied naturally, i.e. by fully preserving the components of the ecosystems in place. This makes it possible to integrate in situ the combined responses of all these components on the soil C stock. It is rare and very valuable to have such observations.
For this reason and in order to improve the readability of the document, a more explicit synthetic description of the device (distribution of the plots around the hot spots, number of Institute plots...) should be presented in the main text (from line 30). Finally, a reference to appendix A must be added in the main text.

We thank the reviewer for the positive feedback on our paper, and the thorough reading of the manuscript. The set-up of the two experimental sites is thoroughly described in Sigurdsson et al. (2016). As the reviewer suggested, we added a more specific reference in the introduction at line 32-33: "To address both these challenges, we determined SOC stock changes along natural geothermal gradients at the ForHot research site in Iceland, which is extensively described in Sigurdsson et al. (2016).". Also a reference to appendix A was made in the main text on line 34-35: "An elaboration on the choice of these two soil layer depths is provided in the material and methods section in appendix A."

I would like to emphasise that the observation set-up created by the authors shows remarkable rigour. Many precautions were taken in the design of the soil samples on transects distributed around the geothermal points. In addition, a great deal of expertise was deployed to verify and characterise the heating gradients obtained at the soil surface and at depth. The measurement methods used are not only well documented but also show a high level of rigour in the acquisition of field data. I particularly appreciated the efforts made to estimate the SOC stocks by taking into account the evolution of the apparent soil densities

under the effect of warming (corrected SOC stocks).

We thank the reviewer for the appreciation of our work.

Thanks to all these investments, the results are mostly convincing. They clearly show an ephemeral SOC loss under warming from the upper soil layer. On the other hand, I am less attracted and convinced by the results and conclusions concerning the SOC dynamics in deep soils. Indeed, only soils under long-term warming could be sampled, which weakens the level of investigation on the response of deep SOC to warming. As a result, these deep SOC results and their interpretations are less robust and original than the previous ones, although 3 paragraphs are dedicated to them against only one previously (L79-83). A better balance between these two axes of results should be found, which would reinforce in this paper the place given to the temporal dynamics of the SOC response to warming, for which the results obtained in this original device are clearly convincing.

We agree with the reviewer that the mechanism(s) behind the stable SOC stocks in subsoils is (are) not that clear as those behind the SOC stock loss in topsoil. For the latter, Walker et al. (2018) provide a well-founded mechanistical explanation which makes a thorough elaboration on the subject unnecessary. For subsoil, there is more speculation and three (non mutually exclusive) mechanisms were listed in the manuscript. The conclusion sentence of the subsoil paragraph (ln 112-114) emphasises this: "Further research is needed to unravel the drivers of these contrasting subsoil SOC responses to warming among experiments, which may be related to differences in soil properties, aggregate dynamics or rooting depths."

The ephemeral SOC loss under warming from the upper soil layer has been observed here on a natural warming device. These results are in line with other results showing an ephemeral increase of soil respiration but obtained under experimental (i.e. artificial) warming conditions. It is interesting to note that these studies, although distinct in terms of the ecosystem studied (artic grassland vs. temperate forest), share similar soil warming gradients and lead to similar durations of increase in C loss fluxes (5-6 years) before this positive response to warming disappears. Among the possible explanations already proposed in the literature to address this change in response to warming over time, several are mentioned here and sometimes tested in coherence with data collected on site when available. Nevertheless, one explanation proposed in the literature is totally absent, although it would offer an additional interpretation here that would be particularly consistent with the idea put forward by the authors regarding "the interplay between soil microbial biomass and activity" (L77). This is the positive temperature effect on enzyme inactivation which results in a reduction of the catalytic power of enzymes under warming (Alvarez et al. 2018). By accelerating the inactivation of enzymes produced by microbes, warming leads to a reduction of the enzymatic breakdown of soil organic C in the long term. Moreover, when taking into account this temperature-dependent process, which constrains the dynamics of the enzyme pool, simulation shows an ephemeral positive response of enzymatic activity, which is followed by a disappearance of the positive long-term effect of warming or even a negative effect. Considering this theory would also contribute to the discussion on the response of the soil microbial biomass (negative) and CUE reported by the authors. Finally, this theory would bring an additional element to the discussion on the carbon-climate feedbacks of warming.

Thank you for pointing out this interesting mechanism. It could indeed play a role in attenuating the warming-induced increase of microbial soil organic C decomposition. We

included a sentence describing the mechanism on line XXX: "Additionally, soil warming might reduce the catalytic power of microbial enzymes and lower SOC decomposition (Alvarez et al., 2018).

Concerning data analysis I have some comments regarding the statistical models used to analyse the effect of warming on variables. In particular, I found it difficult to digest the construction made in figure 1 and thus to adhere completely to the interpretations made in terms of warming effect. My first concern was that the data used to construct the relationship combine the two sampling dates for each field plot, i.e. two different warming durations for each field plot (confounding effects). Reading the last paragraph of Appendix A, which briefly describes the mixed model used, reassured me a bit. Nevertheless, a more explicit presentation of this model with statistical results obtained and interpretation for at least the SOC stock variable would be necessary in the appendix (with reference to appendix in the main text and the caption of Fig1).

We agree with the reviewer that a more explicit description of the statistical model used should be present in the manuscript, which we added in appendix A. We edited the mixed model slightly, as reviewer 1 suggested, by including also plot transect as random factor. A reference to the model description in the main text and in the caption of figure 1 was also added.

Technical comments

L50 - I did not understand where the number in brackets "or 8.8 °C-1 " came from or its unit. Please clarify.

We thank the reviewer for pointing this out. The value between brackets provides the percentual SOC stock loss per degree Celsius, so a % sign was lacking. The error was corrected.

Appendix A - L163 – "we adopted a regression approach" Please specify briefly the purpose.

The reviewer is right that the purpose of the sentence is unclear. After consideration, we removed the whole sentence from the experimental design paragraph, as all necessary information about the statistical analysis can be found later in the appendix.

Appendix A - L185 – "for a some weeks" remove "a"

The error was corrected.

Fig 2a legend. SOC stocks are expressed as "kg ha-1". Replace by ton ha-1 ?

The error was corrected.

Fig 2 legend. please add 0-10 cm to the axis units

'Topsoil' was added to the axis units.

Fig 2 - It is surprising to see only 2 data (points) for the 4.1-5.1°C warming, which are also well above the curve for this warming category. Are there any missing dots or is there a problem with the legend colors?

The reviewer is right that this can be confusing. This conceptual figure is built as is stated in the figure legend: 'Soils are divided in four warming categories for representation. The

colours on the heatmap and the smoother lines are based on a linear regression equation per sampling event.'

Hence, the smoother lines are based on the linear regression of all data points, and not only on the points within a warming category. They are only drawn to visualise that no additional reduction of SOC stocks was observed after 5 years of warming, and are not intended to represent the statistical model. For this, we refer to figure 1.

Figure B1 – In the caption, rather than "Reduction of…" I guess that it would be more appropriate "Uncorrected carbon stock…"

We thank the reviewer for the suggestion. The caption was edited accordingly.

L70 - please move up the reference to fig3 from line 71 to line 70

The figure reference was moved.

Fig 3: The x-axis shows soil warming range from 0 to 18°C. This is different from the soil warming range 0-6.4°C. Please clarify and give explanation in the text.

We agree with the reviewer that using the full warming range for soil aggregates instead of soil warming <6.4°C can be confusing. However, as the aggregate data is meant to support a mechanistical theory about aggregate breakup reducing the physical protection of SOC, we do not think this is problematic. To indicate this we added the following to the figure caption: "The full soil warming range is used here, to make optimal use of the smaller sample number for aggregate data".

L72, 73 and elsewhere – It would be more explicit to replace "soil C %" by "C concentration" in soil fractions.

The suggestion of the reviewer was incorporated throughout the manuscript.

L70 – The name of fractions should be improved to increase direct readability without increasing words number. ">2 mm" corresponds to 2-8 mm, ">250 µm" corresponds to 2000-250 µm, ">63 µm" corresponds to 250-63 µm.

We thank the reviewer for the suggestion. The manuscript was edited accordingly.

L76 – "emerged from the interplay between soil microbial biomass and activity". Wouldn't it be clearer to talk about a change in the specific activity of the microbial biomass?

We thank the reviewer for the suggestion. A detailed description of the mechanism can be found in Walker et al. (2018). As not only an warming-induced increase in specific activity of the microbial biomass, but also a reduction of the microbial biomass size leads to an eventual new equilibrium of SOC stock, we decided to leave the description as it is now.

L77 – "Warming at the same study site accelerated microbial growth and respiration (Marañón-Jiménez et al., 2018; Walker et al., 2020)" – Please could you clarify here whether these two warming-induced accelerations were observed only in the medium term or also in the long term?

The sentence was extended as following: "Warming at the same study site accelerated microbial growth and respiration both in the medium-term and in the long-term warmed grassland (Marañón-Jiménez et al., 2018; Walker et al., 2020)"

L79 – Reference to fig B1a. Add "a" and "b" in fig B1.

a) and b) were added to the figure panels.

With kind regards