Biogeosciences Discussions

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Reply on RC1

Matthias Volk et al.

Author comment on "Massive warming-induced carbon loss from subalpine grassland soils in an altitudinal transplantation experiment" by Matthias Volk et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-287-AC2, 2022

we suggest to refer to the attached *. docx or *.pdf versions of our response, because here the formatting got seriously damaged in the pasting process

RC1: 'Comment on bg-2021-287', Anonymous Referee #1, 11 Jan 2022 reply

> *We copied the Reviewers comments into this text for convenience of reading. Author responses start with an arrow (**>**), are in italics font and are indented like this paragraph, in order to be easily distinguished from the Reviewers comments.*

In the study “Massive C loss from subalpine grassland soil with seasonal warming larger than 1.5°C in an altitudinal transplantation experiment” Volk et al. examined how warming, fertilization, and water availability influence ecosystem organic carbon stock and C fluxes by using a transplantation approach along an elevational gradient. The findings indicate that warming lead to a decline of the C stock, while fertilization and soil water had no effect. This study is of great importance because it shows that global warming triggers processes that act as a chain reaction and cause further warming, even if the human-made causes of global warming would be stopped. The manuscript is very well written, it is easy to understand, and it has a good structure. All in all, I think this study is very nice - the approach is new and clever, the study is well designed, the topic is more important than ever, and the results are crucial, alarming, and a call to action.

> *we greatly appreciate finding our study so well received!*

Nevertheless, I have a major concern about the method/statistical analysis that needs to be clarified before the manuscript can be published:

If I understood correctly, soil monoliths (0.1 m2 surface area) were taken from different sites (at the height of CS2), put into plastic boxes, and buried in different sites along the transect (within the plastic boxes). Thus, plants and soil organisms have to deal with warmer (or colder) environments, which can mimic global warming (or cooling). I think this is a very smart approach, however, I wonder how the plastic boxes affected the growth of the plant and soil communities:

Regarding the plants: Changes in the environment often result in changes in competitive relationships between plants - for example, fertilization often results in grasses becoming more dominant. Subordinate species can only escape this increased competition by growing in open areas, or they become extinct. However, this is not possible in boxes and I would imagine that warming or fertilization would cause species to die out, leading to a significant change in diversity over the years. In principle, this is not a bad thing, because all communities are equally influenced by the growth in the boxes, however, the question arises then how well the results can be related to "real" processes in nature and whether we can draw the right conclusions from this study. Plant diversity and plant community composition have a strong impact on the carbon cycle, so it would be important in this study to address how plant communities have changed over the years (have there been overall losses of diversity, has composition changed, are patterns the same everywhere or do they vary from site to site? - a few sentences in the method part and/or in the discussion would be great). I noticed that some previous studies have addressed diversity, etc. - so it would be good to cite those and briefly summarize what came out.

> *You are touching an important, but extremely difficult subject here. At least on the functional group level, we can contribute relevant information and suggest to include a statement on the* potential *effects of biodiversity, e.g. based on our previous publications on the subject*

Wüst□Galley, C., Volk, M., and Bassin, S.: Interaction of climate change and nitrogen deposition on subalpine pastures, Journal of Vegetation Science, 32(1), e12946, 2021.

Bassin, S., Volk, M. and Fuhrer, J. (2013) Species composition of subalpine grassland is sensitive to nitrogen deposition, but not to ozone, after seven years of treatment. Ecosystems, 16(6), 1105–1117. https://doi.org/10.1007/s10021-013-9670-3

Blanke, V., Bassin, S., Volk, M. and Fuhrer, J. (2012) Nitrogen deposition effects on subalpine grassland: The role of nutrient limitations and changes in mycorrhizal abundance. Acta Oecologica, 45, 57–65. https://doi.org/10.1016/j.actao.2012.09.002

We suggest not to dig into the subject any deeper, because only measuring the community composition, is standard procedure. But establishing a quantitative cause-effect relationship between composition changes and ecosystem C stock changes in a 100+ vascular plant species system, seems impossible to us. We decided against an in depth discussion of the biodiversity issue, because we feel that that would be highly speculative.

Ideally, plant diversity and/or composition could be used as co-variables (or random effects) in the mixed-effects models to exclude that the changes in carbon budget are triggered by box-induced changes in plant diversity or composition.

> *Earlier you described the assumed box-effect in the context of our climate change experiment as leading to species dying out rather than just migrating to more suitable places. We feel that even if C budget changes* were *triggered by such box-induced diversity changes, we would still have a valid representation of a grassland plant community that has lost a few members due to climate change. For this reason we would rather not introduce additional variables into the statistical model.*

Regarding the soil community: again, box effects could change the community, but I think if the plant community is being discussed/considered, there is no need to also discuss soil community - that would be beyond the scope. However, I wonder how permeable the containers were? Would it be possible that within the 5 years soil organisms could enter through the holes/gaze (or however the containers were made permeable) and affect/change the soil community?

> *Indeed, the containers had 6 mm holes drilled and the bottom was covered with a ca. 3 mm thick drainage fleece. Thus, an immigration of soil organisms is likely. We assume that this had no or little effect on the soil organism community in the monoliths, but we have no data on this.*

In addition to this main issue, I have some minor comments/questions:

I like the introduction; however, the hypotheses are phrased in an unclear way, e.g. the opening sentence of hypothesis 3 “Irrigation mitigates effects of drought due to warming and N deposition reduces …”: drought due to warming AND N deposition or drought due to warming, and N deposition?

> *Thank you for drawing our attention to this missing comma. Suggested improvement:*

3) Irrigation mitigates effects of drought due to warming, and N deposition reduces possible N limitation of microbial activity; both factors thus exhibiting a favorable effect on decomposition and reducing the SOC stock.

L 69: Are there more recent studies that support the statement (the cited study is already 22 years old and it is an important aspect that is addressed here). In general, I noticed that many older studies were cited, although I am sure that there are also many more recent studies on this current topic.

> *You are right, there are more recent studies, despite the expensive free air CO_2 fumigation research has peaked already a while ago. And low productivity grassland received much less attention than agronomic systems or forest ...*

Indeed, the literature reports mostly positive biomass responses to CO_2 enrichment, even though it appears that both duration of fumigation, climate, edaphic factors and nutrient supply strongly influence the response.

For example, the very recent Tansley review concludes that there is a terrestrial C sink resulting from CO_2 fertilization of photosynthesis, but it also states 'However, we frequently have low or medium confidence in the magnitude, and low confidence in how much of the change is attributable to iCO2'.

Walker, A. P., De Kauwe, M. G., Bastos, A., Belmecheri, S., Georgiou, K., Keeling, R. F., ... & Zuidema, P. A. (2021). Integrating the evidence for a terrestrial carbon sink caused by increasing atmospheric CO2. New Phytologist, 229(5), 2413-2445.

Also in the 17-year fertile, temperate grassland experiment (GiFACE), a positive biomass response was found. All plots received a total of ca. 60 kg N ha^{-1} a^{-1}:

Andresen, LC, Yuan, N, Seibert, R, et al. Biomass responses in a temperate European grassland through 17 years of elevated CO2. Glob Change Biol. 2018; 24: 3875– 3885. https://doi.org/10.1111/gcb.13705

On the other hand, the Jasper Ridge experiment, that is outstanding in terms of multifactorial treatments and has no N treatment in the control plots, did not result in a CO_2 main effect in 17 years, but many significant interactions with other environmental factors like fertilization:

Zhu, K., Chiariello, N. R., Tobeck, T., Fukami, T., & Field, C. B. (2016). Nonlinear, interacting responses to climate limit grassland production under global change. Proceedings of the National Academy of Sciences, 113(38), 10589-10594.

In addition, a 2010 meta-analysis finds, that beyond the generally small response to CO_2 there is no significant relationship between CO_2 concentration and response size. This suggests CO_2 saturation of photosynthesis and plant growth to be primarily limited by other resources, such as nutrients and water:

Lee, M., Manning, P., Rist, J., Power, S. A., & Marsh, C. (2010). A global comparison of grassland biomass responses to CO2 and nitrogen enrichment. Philosophical Transactions of the Royal Society B: Biological Sciences, 365(1549), 2047-2056.

This is why we think that rising CO_2 concentrations are not a relevant factor for the productivity of our unfertilized subalpine grassland.

But since the CO_2 issue will not be addressed again in the manuscript, we suggest to simply omit l. 67-69.

L 108: Why were the monoliths 22 cm in depth? Many plants can grow deeper than 22 cm. I understand that the monoliths cannot be taken one meter deep, but is there a specific reason for the size of the monoliths? It seems very random, whereas depth can have an influence (for example, that certain plants can get water from deeper layers, etc.).

> *True, most plants will go deeper to tap resources. When we probed the original sites of the monoliths, we found almost invariably only course gravel or bedrock at depths greater than 20 cm. Some plants could likely extract water from underneath the shallow soil layer, but we could not excavate this material. This is a design deficit in our experiment.*

L 127: did I understand correctly that only the irrigated boxes were fertilized? If so, why? Then it is not a full factorial design, isn't it?

> *Sorry for being unclear. This is described in more detail in the BG 2020 companion paper on this experiment.*

The N treatment was applied as a NH_4^- NO_3^+ in 200 ml water solution. Control plots received water without N. Thus, all plots received this amount of water, equivalent of 2 mm rain per fertilization event (bi-weekly, i.e. ca. 12 times per year).

The irrigation treatment is applied to half of all monoliths.

throughout the text: I find it difficult to label the irrigation treatment as drought. I understand the idea that warmer temperatures and less precipitation can lead to less water availability, but that doesn't mean it's a drought event (or is there data on that?). I wouldn't call it drought treatment (especially since it wasn't water availability of the "dry" plants but the control that was manipulated). Maybe it could be labeled as altered precipitation or water availability.

> *You are right, drought is not a treatment per se in the experiment, but a consequence of the downward transplantation. On the other hand, the supplementary precipitation is a treatment to mitigate drought conditions. Accordingly, we do not refer to the irrigation treatment as 'drought', but as 'irrigation treatment' (e.g. l. 17-19 'In addition, we applied an irrigation treatment …, simulating summer-drought mitigation …').*

regarding the title: The title states a "massive carbon loss", while the abstract states a "14% loss". I am not an absolute carbon cycle expert to assess this percentage accurately, and I am sure that 14% is a lot regarding effects on the climate. Nevertheless, the word "massive" and "14%" compete. Perhaps it should be rephrased, or the 14% is put in relation, that shows that 14% loss is massive (e.g., with XY% loss, global temperature could continue to rise XY°C, or normal is XY% loss over XY years).

> *Not that it is going to happen this way, but if the 14% loss in five years would continue, after 35 years there would be only bare sand left.*

To put our result into perspective, please compare it to the claim of the '4p1000' initiative (https://www.4p1000.org/). This renowned initiative is aiming to save the climate by a 0.4% C content increase per year in agricultural soils. By these standards, we consider the 14% C content decrease in 5 years quite spectacular.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-287/bg-2021-287-AC2-supplement.zip