Response to reviewers; Sigrid van Grinsven et al. 2026

Dear editors and reviewers,

We have now revised and updated our manuscript for SOIL Letters, following the suggestions and feedback from the reviewers and editor. We have included the clean revised manuscript, a file with track changes visible, and this rebuttal letter in which we reply directly to each of the reviewers' comments. We have now also put our data in the Zenodo repositiory, and added the DOI to the manuscript.

Kind regards, also on behalf of the coauthors,

Sigrid van Grinsven

# of words in abtract (max: 200): 200
# of figures in main text: 3
# of words in main text (excluding figure captions; max: 2500): 2493
# of references: 25
# of words in methods section (Appendix A): 1539

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van Grinsven et al. report on carbon stock and carbon dioxide flux differences between wet and dry sites in the proglacial area of the Bachfallenferner glacier in the European Alps. The researchers demonstrated that the wet sites stored more carbon in soil than the dry areas in their space-for-time analysis. They also showed that the wet areas tended to more consistently have CO_2 uptake than the dry areas over their measurement period, regardless of age of the site. Overall, this research demonstrates local hydrological conditions are important for understanding carbon dynamics in proglacial areas and have the potential to store more carbon than would be anticipated by only studying dry sites. I enjoyed reading the article and it is suitable for publication in SOIL pending moderate revisions. My main concerns with the article surround how the total carbon mass in the soils was calculated and whether the study has enough information to justify calling the wet areas carbon dioxide sinks. I also think the paper would benefit from a few additional analyses surrounding the correlations between organic matter content and carbon dioxide fluxes with plant cover. Details are outlined below with associated line numbers.

Re: We want to thank the reviewer for the constructive feedback. We have provided more detailed responses to the comments and suggestions below.

Abstract:

Line 21: "key locations of CO_2 uptake and a significant role in carbon storage in the proglacial valley" – based on the study design I think it can be justified that these areas generally have more uptake of CO_2 than the dry areas in the summer, but because the study does not have fluxes outside of August it may be a bit misleading to say they are key locations of CO_2 uptake generally. Same goes for the title of the article.

Re: Although we agree it would be great to have measurements of more timepoints than just August, we would argue the data collected of just August still has a good predictive value for the uptake of CO2 and storage of soil carbon in the whole active season of these type of proglacial valleys. The reason why we believe this has to do with two factors: 1. The growing season in these high altitude systems is very short (July and August are considered summer, June spring, and September autumn. Outside of these months, there is a partial or full snow cover). Therefore, the sampling campaign of 1 week in August cover a substantial part of the growing season. 2. In addition to CO2 flux, we also measured soil carbon content (Fig. 3). This is a parameter that represents long term, not seasonal, changes, and is therefore not dependent on sampling time.
We hope the reviewer agrees with these points based on this new information. We have added the information on the growing season (summer) in this area to the methods section to clarify this also better to the readers. ("No climate station is available at the Bachfallenferner glacier, but the nearby (23 km distance) Pitztaler glacier has a permanent climate station at 2864 m.a.s.l. The average daytime temperature at this station over the period 2020 – 2025 was -0.6°C, with an average daytime temperature of 8.6°C in the summer months (July, August). A consistent snow cover is generally observed from September/October to June (Geosphere Austria23)."). We have also added a line in the last section of the results & discussion, that highlights that non-growing season and nighttime fluxes are never measured for these kind of systems. (And that hopefully follow up research will be done on this!)

Line 22: "despite their small surface area" – can a rough percent estimate for the area of the wetlands within the proglacial area be given? This will help to give further context to their relative importance in proglacial areas.
Re: We have now done a rough estimation, and added <5% to the abstract. In the methods section, we have explained how we got to this number. As it is just a rough manual approach, we decided to write it in the form of '<X%' in the abstract. This is the text of the methods section: "A rough estimation, based on manual polygon drawing in Google Earth (satellite imagery from June 2025), gave a surface area of ca. 15,000 m2 for wetlands, versus 1,013,000 m2 for the entire proglacial area (including lakes and bedrock covered areas, excluding the glacier), resulting in a rough estimate of 1.5% wetland surface. As this is likely an underestimation, as not all wetlands can be recognized on satellite imagery, we settled on an estimate of 1 - 5%."

Line 22: "Loss-on-ignition" – I think these results should be presented somewhere in the paper, so that the reader can evaluate how many of the sites have high organic matter content. I suggested Table S2 below.

Re: All data on the loss-on-ignition is now available in the open data file that is in the Zenodo database. We have also included it as a file for review to the upload of this revision.

Figures:

Figure 1: Should the yellow border be visible around the entire proglacial area like the orange and blue perimeters? Write out the acronym GLIMS and provide the reference (12 in the reference list). Add an inset map showing where the study site is within Austria.

Re: thank you for noticing that the yellow line is not visible, we had not realized ourselves that it's not visible and therefore confusing. The yellow line is beneath the blue line for the southern part of the *glacier, which makes is what made it disappear. We have now specified this in the legend. We also adapted the GLIMS reference.*

Figures 2/3: I suggest adding mean plant and moss cover for each age in these graphs to clearly demonstrate when vegetation appears in the wet and dry areas in the main text. This is one of your key datasets and you should showcase it!

Re: thank you for the suggestion! We have now adapted Fig. 3 to include the vegatation data.

Results and Discussion:

Line 124: "quantify the surface area of wetlands" – as mentioned in my comments for the abstract (Line 22), can an approximate percentage for wetland area be provided based on field observations?

Re: We have now added: "Our first estimate of the wetland surface area in the Bachfallenferner area is 2 – 5% of the total proglacial area (including lakes and bedrock), but whether this is similar for other proglacial areas, has thus far not been investigated."

Line 132: "plant community is limited" – when looking at Figure B2, it seems like almost all the sites do have some amount of plant cover, which would presumably contribute to CO_2 uptake. I suggest investigating whether there is a correlation between percentage plant and moss cover and carbon dioxide flux/carbon mass. This would either support or refute the hypothesis is this study that CO_2 uptake is microbially derived. For example, if the correlation was not strong in younger sites vs. older sites, then it would support that the microbial community is playing an important role at the younger sites. The correlations may also be different for the dry vs. wet sites. This analysis would also support the discussion starting at line 195.

Re: We have now added information about the plant coverage to Fig. 3, to give a visual overview of the plants and mosses abundance at the measured locations, in a way that it can be compared directly to the soil CO2 flux and carbon stocks. However, the spread in the data (both in the fluxes, as well as in the plant coverage), is too large for a solid correlation between the two, based on the amount of datapoints we have. For future studies, we would like to focus more strongly on this contrast between vegetated and unvegetated sites, and on the role of the plant and microbial communities. We however see, from our current dataset, that many factors seem to affect the CO2 fluxes, as there is no dominating explanatory variable for the CO2 fluxes. We have, prior to the first submission of this manuscript, also tried basic modeling to see if this would help us figure out correlations and possible relationships between variables (such as plant cover, age, soil pH, wetness), but this gave no statistically strong outcomes, and we do not want to manipulate the dataset (by creating smaller subsets) or modeling excersises in a way that forces stronger outcomes. Therefore, we think the contribution of plants versus microbial CO2 uptake and respiration needs to be the subject of a follow-up study.
We did add information on statistical significance in several places in the manuscript.

Line 156: One could also perform fluxes when the chamber is shaded or clip the vegetation prior to measurement in the field.

Re: We have now added this suggestion in the text.

Line 160: This paragraph is related to Figures B3 and B4. I would add a sentence here before discussing the age relationships to draw the attention of the reader to them.

Re: We have added the figure references here (but kept it very short to not exceed the word limit).

Line 172: “linear increase in soil organic carbon content” – does the data from this study (i.e., LOI and age of site) also have this same pattern? Is it different for dry vs. wet soils?

Re: we have added that information now to that line in the text: “We also observed a linear relationship between age and OC content (R2 = 0.40 for dry soil, R2 = 0.35 for wetland, Fig 3) and ...”

Line 184: “significantly higher CO_2 uptake rates” – was this tested statistically? Please provide the statistical summary.

Re: Statistical significance added to text.

Line 193: “Supplemental File 2” – I don’t see the LOI results in the supplementary file (see comment for Table S2).

Re: All data on the loss-on-ignition is now available in the open data file that is in the Zenodo database. The reference to Supplemental file 2 on line 193 should have been to Table S2; we have adapted that now.

Line 195: Figure A should be Figure B1?

Re: corrected

Line 219: “consistent uptake” – keep in mind that this study records consistent uptake spatially but not temporally, especially since, as mentioned in the discussion here, glacial meltwater does not have a consistent flux through time. Also, I am not sure that having consistent uptake means that allochthonous CO_2 production is limited, as the net CO_2 flux was measured in this study. Please provide more details on the reasoning for this discussion point.

Re: We have now adapted this sentence to make the reasoning more clear: “As we measure consistent uptake (95% of wetland chambers showed a negative CO2 flux) of CO2 from our wetlands, we expect allochthonous aquatic CO2 inputs, which would lead to positive CO2 fluxes, to be limited.”

Line 222: As stated above (Line 132), looking at correlations to vegetation cover is missing in this list.

Re: adapted

Appendices:

Line 411: The description of the conversion to $\mu M\ m^{-2}\ hr^{-1}$ is consistent with the text, but in Figure 2 and Table S1 the values are in $mg\ C\ m^{-2}\ hr^{-1}$. Make the units consistent across the text and figures.

Re: adapted

Line 436: Is assuming 1500 kg m^{-3} across wet and dry soil types for all ages of site appropriate? Where does this bulk density value come from? Please provide a reference and more justification. The density of organic soils is typically much lower than 1500 kg m^{-3}. For example, the average bulk density of peat in northern peatlands is 118 kg m^{-3} (Table 1; Loisel *et al.* 2014 Holocene). Even if the sites are not peat forming, I would expect the bulk density to decrease as organic matter from soil formation is incorporated. By using a high bulk density across all the site locations, you may be overestimating the amount of soil carbon mass.

Re: Taking one value of 1500 kg m-3 is definitely a big assumption, and we should have included the reference and more explanation how we got to this number. Thank you for bringing this up. We have added this now to the methods section, but will also explain more here:
Before setting up the field campaign for this study, we have thought long and hard on what would be the best way to take the soil samples. It would have been ideal to use the traditional metal soil rings, that allow for a direct measurement of the bulk density by weighing them afterwards. However, due to the stony nature of many of the sampling sites, this method cannot be used. It would very easily lead to cherry picking of sites with a less stony, higher fine earth fraction, and we wanted to prevent that. And even in most sites with a higher finer fraction, there still were stones and pebbles present that would have made the use of the rings impossible.
Therefore, we instead decided to make an assumption of the bulk density. Our soils are not organic soils, as the vast majority of our samples, including samples in wetlands, had <5% organic matter. We therefore assumed 1500 kg/m3, which is a fairly standard assumption for samples that are not very rich in organic matter (see for example Bockheim and Munroe, 2014, https://doi.org/10.1657/1938-4246-46.4.987). Correcting the density for the organic matter content but not for the other changes that happen during aging of the soil material, also seemed incorrect, as the increasing OM content is only one of many changes that happen to the parent material. This is also supported by the paper by Mavris et al. (2010, https://doi.org/10.1016/j.geoderma.2009.12.019) who did measure bulk denisty in a proglacial chronosequence. In their data, no significant relationship between OM content and bulk denisty in proglacial soils can be found, most likely due to the many other changes (in sand, silt and clay content, for example). We therefore decided to not correct the bulk density assumption to the measured OM content.

Figure B1: Also include pictures of a representative dry area for comparison?

Re: On these photos, dry soil is also visible. We have now included that to the legend: "Wetland locations (in the foreground of the photos) and dry soil (seen in the center (3 yrs and 56 yrs) or background (130 yrs) of the photos) with time since glacial retreat (age) in the Bachfallenferner proglacial area. The right photo (130 yrs) also shows a small pond, with grass, in the centre of the photo."

Fig B2: upper and lower should be left and right. I suggest adding labels to the plots (A and B) and describing each under those labels in the figure caption.

Re: the figure has been adapted, because we have now added the surface cover to Fig. 3. We also adapted the figure caption.

Figure B3/B4: display the equations and the 95% confidence intervals?

Re: added.

Table S1: add the R2 values for each flux.

Re: adapted, R2 values added.

Table S2: Is there a reason the loss-on-ignition results not been listed here? It would allow readers to better understand how the organic carbon stock was calculated. Maybe this is the Supplemental File 2 mentioned in the text? (Line 193).

Re: All data on the loss-on-ignition is now available in the open data file that is in the Zenodo database. The reference to Supplemental file 2 on line 193 should have been to Table S2; we have adapted that now.

Comments to S. van Grinsven et al. Proglacial wetlands: an overlooked CO_2 sink within recently deglaciated landscapes.

This manuscript reports on the effect of moisture status (wetland or dryland) and age-since-deglaciation on either CO_2-drawdown of CO_2 emission from incipient soils. The used language is OK and understandable, but the organization of the manuscript is messy. The separation of work done by the authors that from other studies is not always clear. I assume the measurements are OK, but explanation of the mechanisms explaining the measurements is a bit too haphazard. Some mechanisms are ignored (e.g. effects of weathering and parent material in this versus other studies), some mechanisms are brought forward that strengthen some measurements but at the the same time weaken results elsewhere (e.g. l.202).

At this moment I would not support publication unless after a major restructuring of the paper including cleaning up some reasoning.

Re: Dear reviewer, thank you for reviewing our manuscript and your comments and suggestions. We have answered your comments below in detail.
One overarching issue we would like to mention, that was also briefly discussed before in the interactive comments section of SOIL, is that this is a publication meant for the SOIL Letters format. Such a format is much more concise than the standard publication: "Letters have fewer than 2,500 words in the main text, 200 words in the abstract, and an appropriate number of figures, or tables, and references." Therefore, we are not able to go into detail in all mechanisms, patterns and literature that is potentially relevant for this topic. It also instucts the manuscript organisation, with the methods section at the very end and several figures in the appendix (supplemental) rather than the main text. Where possible, we have incorporated your comments, but in other places, this is not possible due to the journal format, which we think is in general a good fit for this work. But we have given detailed responses to the comments below, which we hope still answer many of the questions asked.

Some comments follow hereunder:

Manuscript organization

The topic is interesting, but is addressed poorly and apparently written down in a hurry.

A. I found the section order 1. Introduction; 2. Results and discussion very strange, why put all material and methods only in an appendix? In this appendix, essential information seems to be lacking (homogeneity of the parent material, precipitation).

Re: See comment above regarding the SOIL Letters manuscript style.

B. Where do I find a research hypothesis, research questions, a research approach and clear conclusions? Why is on l.74 already a conclusion mentioned, in the Introduction?

Re: See comment above regarding the SOIL Letters manuscript style. We have included the last line of the introduction, that contains conclusions of this work, to provide the engaging style that the SOIL Letters format specifically asks for.

This manuscript needs major reorganization to avoid the impression of an haphazard investigation.

Detailed comments

l.74: "We however show": this is a result, not part of an introduction. "show">"suspect"? This could then lead to a research question motivating what was done.

Re: See comment above regarding the SOIL Letters manuscript style. We have included the last line of the introduction, that contains conclusions of this work, to provide the engaging style that the SOIL Letters format specifically asks for.

Fig.1: What is a GLIM? Better not put unexplained abbreviations in a figure caption

Re: Thank you for noticing, we have now adapted this figure caption: "More outlines are available (Global Land Ice Measurements from Space (GLIMS) dataset12) but not included in the figure." We have also added the reference of the GLIMS dataset.

l.104: Why the Bachfallenferner area? Why is it particular suitable, to address what research question?

Re: We have now added this more clearly to the text: "Given the well documented glacial retreat in the proglacial area of the Bachfallenferner (Fig. 1)13, each location in this area can be given an age via linear interpolation between recorded glacial extents, making it particularly suitable for a space-for-time study approach like was used here."

l.116: This sentence is Material&Methods, not Results&Discussion

Re: As mentioned above, the material and methods section comes after the core text of this paper in this format, and like you mentioned, is therefore not the first thing people see. We therefore consider it helpful to provide this information here, to allow for a smooth reading experience for the audience. We want to make sure they are not confused by the meaning of the word soil throughout the text, as we know that different audiences have different definitions of the word soil.

l.123-125: This sentence is a conclusion.

Re: Given this is a mixed Results and Discussion section, and there is little to no space for a separate conclusion section in this concise and engaging format, we consider it useful to provide conclusions in the text whereever a subtopic is discussed. This makes sure we do not need to highlight the findings later again to then provide a conclusion at a separate location in the manuscript.

l.127-142: This appears speculative: the CO_2-flux cannot be described by the plant communities (mostly absent), thus it must be the microbial community. Explanation needed! What makes the community drawdown atmospheric CO_2? Could not also the composition of the drainage water from the glacier be a factor? Describe the chemoautotrophic pathways. Do these involve weathering? What would be a weathering pathway in the Bachfallenferner area, given the parent material(s)? A vague link to "microbial genes" should be elaborated.

Re: Unfortunately, there is simply no space in the letters format to expand on this, without removing other parts of the results and discussion. As a microbiologist myself, I would love to expand on microbial communities in proglacial soils and their expected role. However, as the SOIL journal attracts a wide audience, we tried to cover the different topics and explanations for our findings in the *discussion, without diving too much into detail of one of them. Talking into more detail about the microbial community and involved genes will shift the manuscript to a microbiology focus, as that will remove other text. And as we have not measured anything on the microbial community directly, we think this would not make sense. However, we also do not want to take the mentions and literature references on the microbial community out, as we think this is a very valuable insight and highly likely explanation for our findings, given the presented literature and the general knowledge that microbial communities that consume CO2 are abundant in soils, and are dependent on nutrient and water availability.*

l.148: Apparently, Guelland et al (2013) found an effect of burned allochthonous carbon, releasing CO2. You explain some emission sites by this mechanism. Any evidence? Did you observe incoming allochthonous carbon, e.g. by the color of the water?

Re: We have not done carbon species analysis on the soil samples we collected, which would be needed to determine the origin of the soil carbon. The water running through this valley seemed mostly clear, although low amounts of carbon are hard to detect by eye, and true humic-rich (thus colored) water is not be expected in these kind of environments. Allochthonous burned carbon could however also likely be delivered by wet or dry atmospheric inputs, which would be difficult to observe, unless one would do a large term deposition study.

l.156-158: Yes, CO2 drawdown can result directly from mineral weathering (e.g. proton consumption by weathering stimulates production of carbonic acid, from atmospheric CO2), and complexation with minerals and weathering products can slow down mineralization. As stated, this can occur in older soils (more weathering), so why do you also find the drawdown in the young wetlands?

Re: Although we cannot prove this based on our data, it is not unlikely that also young proglacial soils contain at least a certain amount of weathered materials. See for example Egli et al. 2011 (https://doi.org/10.1016/j.geoderma.2011.05.001). They also find that weathering indices are higher in wet soils than in dry soils, which would match our findings regarding the young wetlands.

Fig. B2: unclear caption. Upper graph=?= left graph, lower graph=?=right graph. Does the site coding make sense, what is 1-S, a soil of 1 year old? What is the soil depth? Rock-stone-fine earth correspond to what color/gray tone? Left graph shows color options no color-light-medium-dark green = 4 options, right graph=gray-green-blue.

Re: We have adapted this figure, because part of it is now included in Fig. 3. We also adapted the caption. We don't understand the question about soil depth, as this figure is about the surface cover, so there is no depth component.

l.160-182: more like a literature review than a discussion on your results.
Re: The other reviewer also noted this, and suggested to include a link to our results at the start. We have now included that, to make it more clear what the relevance of this literature is for our research. Thank you for pointing this out.

l.183-192: Some speculative hypothesis are formulated here, like effects of erosion/sedimentation. Could be true, but am not sure this is not just a "pick" of some possible mechanisms. E.g., could wetlands not be a carbon sink because of presence of aquic microbial species and/or carbonate water equilibria which are mostly absent in dryland soil?

Re: The text at lines 183-192 is almost exclusively results, so we assume that the reviewer refers to the section of text that is following line 192? We have presented the hypotheses that we think are most likely to explain our findings, as we do not have space to go into each hypothesis. We have tried to not only highlight a one-sides story, but give an overview of literature that is available on the topic. The microbial community is indeed likely different, in composition but also in activity, in dry vs wet soils. This is highlighted in line 151. Carbonate water equilibria could only explain a ongoing CO2 uptake if there is a CO2 consumption or transport mechanism active, as the water would otherwise get into equilibrium with atmospheric CO2 and would not result in a net uptake. Those potential uptake mechanisms are the things we hypothesize about in the discussion.

l.202: The wording "indeed" states that microbial activity may be associated with higher water content, but this seems to contradict with your findings on higher CO2 drawdown in wetlands. Explanation?

Re: As mentioned in the line above in the manuscript text, we expect that in young wetlands without a well-established plant community, the microbial plays a key role in CO2 uptake (via the mechanisms also explained in lines 137-152). They also respire stored carbon, so they are also responsible for CO2 losses. However, given our results for the young wetlands (CO2 uptake instead of outflux, and a low OC stock, so low presence of precursors for fermenters and mineralizing microbes), it is likely that microbes than fix CO2 are more abundant than the the ones that respire soil carbon. We expect that in general, the microbial community is more active in the wetland than dry soils, and that is what we refer to with 'indeed' here. We have now clarified the text better ("A study on litter decomposition in proglacial soils showed a positive correlation between microbial mineralisation of litter and the soil moisture content 18, suggesting a higher water content indeed promotes microbial activity, most likely not just the community responsible for remineralization, but also for CO2 uptake.")

l.222-231: Environmental covariates: indeed it would be nice to identify environmental covariates explaining the observed fluxes. Guelland et al assigned this to high heterogeneity, how about this argument in your study? Are the parent materials homogeneous? What is the contact time between transported water and the substrate, relating to nutrient concentrations?

Re: We have not performed geological or hydrological studies, and others have also not done this before for the area of the Bachfallenferner glacier or proglacial area. Therefore, we can only speculate. Heterogeneity is definitely present, this could result from the parent material but also from differences in steepness, exposure, snow cover in winter, erosion and deposition processes, preferential hydrological pathways etc. We believe this is one of the reasons why space-for-time studies in proglacial areas often show a large spread in the measured variables, like we also see in our data. There are so many factors influencing the processes of soil formation and the rate of chemical and biological processes, and the landscape is heterogeneous. This makes it very difficult to find direct relationships and correlations. And even if correlations are found, it is not always clear whether the relation between the variables is via direct or indirect effects.

l.349: sampling excluded lakes, but in l.352 they are included?
Re: We describe that we used Google earth imagery for locations that appeared to be wetlands or small lakes, and then confirmed in the field that they were wetlands (as small lakes often grow into wetlands over time, and are anyway hard to distinguish on sattelite imagery). But we have changed the wording in the text now to make it less confusing for the reader.

l.377 etc: fluxes were measured only at daytime. Is it then not possible that, when brought to 24h time span, many sinks become sources? It also makes me wonder how much the sink/source discussion is a function of the measurement season (now: 5 days in August). This should be in the discussion for sure.

Re: We cannot easily speculate about the fluxes during the night, because there are several factors affecting CO2 flux that change between day and night. Plant CO2 uptake will of course be strongly decreased during the night, but whether microbial CO2 uptake is decreased and repiration stayed the same, depends on the presence or absence of phototrophic microorganisms and the activity of the microbes responsible for respiration during the night. Other factors strongly affecting the nighttime gas flux are the temperature (much lower at night) and the humidity (higher at night), but also the water input which may be lower at night due to the lower icemelt during the night. As far as we know, there are no studies available of the CO2 fluxes in proglacial areas during the night. There is one study that tested the effect of darkness, but this study does not seem very applicable to extrapolate our results to the night because a) it only uses fluxes from collars that contain plants, and b) they only adapt light influx, not temperature, air humidity and soil humidity. Overall, we expect that the difference between dry soil and wetland locations would hold up during the night, and therefore that the sink/source statements of this paper, that focus on the difference between dry and wet locations and not on net ecosystem or yearly fluxes, will remain the same when the night is taken into account. We purposely have not upscaled any of our measurements, because we felt like we do not have sufficient measurements for that, based on the same concerns you raised in your comment.

However, to make it more clear to the reader that we only report on daytime fluxes, we have added this to the Abstract in two places where we mention the CO2 flux, and also in several other places in figure captions and the text. We have also added it explicitely in the results & discussion section now ("All our measurements, in both wet and dry soils, were done during daytime.").

Regarding the seasonality: the snow-free season in these proglacial valleys is very short. We have added information on the climate to the methods section now, to make this more clear to the reader: "No climate station is available at the Bachfallenferner glacier, but the nearby (23 km distance) Pitztaler glacier has a permanent climate station at 2864 m.a.s.l. The average daytime temperature at this station over the period 2020 – 2025 was -0.6°C, with an average daytime temperature of 8.6°C in the summer months (July, August). A consistent snow cover is generally observed from September/October to June (Geosphere Austria23)."
Although we agree it would be great to have measurements of more timepoints than just August, we would argue the data collected of just 5 days in August still has a good predictive value for the uptake of CO2 and storage of soil carbon in the whole active season of these type of proglacial valleys. The reason why we believe this has to do with two factors: 1. The growing season in these high altitude systems is very short (July and August are considered summer, June spring, and September autumn. Outside of the summer months, there is a partial or full snow cover). Therefore, the sampling campaign of 5 days in August cover a substantial part of the growing season. 2. In addition to CO2 flux, we also measured soil carbon content (Fig. 3). This is a parameter that represents long term, not seasonal, changes, and is therefore not dependent on sampling time.
However, it would be great if more measurements, also during the shoulder seasons and the winter, would be done. We have added this now to the last section of the results and discussion section, to acknowledge the lack of nighttime and non-summer measurements: "The factors controlling soil carbon storage and CO_2 fluxes in proglacial environments remain poorly understood, and no rate measurements during the night, or outside of the growing season, are available."