RC2: 'Comment on bg-2021-287', Anonymous Referee #2, 13 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.*
> *NB: Line numbers in the authors responses refer to the revised manuscript with 'track changes' as provided as *.pdf file.*

General comments:

This study reports on the results of a global change experiment on the carbon budget of an Alpine grassland. Intact soil/vegetation monoliths were transplanted to lower elevations in order to simulate warming and in additional a 2-level irrigation and a 3-level nitrogen addition treatment was superimposed. The main finding is that warming < 1.5°C did not clearly affect carbon stocks and NEP, while stronger warming (up to 3°C) increased ER with little effect on GPP and as a consequence a strong net loss of carbon from the system.

This is an interesting and novel paper, the strongest point in my view being that the authors approach the ecosystem carbon balance both from the point of few of stock changes over time, as well as a gaseous flux perspective by simulating GPP and ER.

In addition to the detailed comments below, I have 5 major comments:

(1) In the methodology of the gas exchange measurements (section 2.7) from which GPP and ER are estimated and then simulated over the course of the experiment, there is not enough detail to judge the validity of these data. Especially, I struggle with the following sentence (l. 191-193): "The light response curve of GPP was derived at CS2reference, and the temperature response of ER was established for CS2reference, CS4 and CS6 separately …". To me this sounds like as if GPP is simulated just based on data from the reference site and then applied to the warmed sites. If that is true, then this might suggest that one of the major findings of the study, no significant changes in GPP despite warming, is an artefact of how GPP was estimated.

> **>** *Thanks for pointing at this; we recognize that more detail is necessary here. GPP was indeed* not *parameterized just on a light response curve from $CS2_{reference}$. Instead, it is based on five years of bi-weekly to monthly measurements of NEE at clear-sky, midday conditions and at night, individually for every single monolith used in the analysis. This procedure ensures that canopy development and soil moisture availability in each monolith are appropriately represented. From these high frequency*

‘stepping stone’ data, maximum potential GPPpot and normalized ER were established and parameterized for every hour, using global radiation and temperature.
We produced a comprehensive description of the process (cf. below), that is now located in the Appendix.

Suggestion for Appendix:

Supplementary information on the gas exchange measurement and parameterization

A 2.1 CO_2 flux sign convention

Throughout this study we adopt an ecosystem perspective when stating gas fluxes. This implies that gross primary productivity (GPP) has a positive value, while ecosystem respiration (ER) has a negative value. Net ecosystem exchange (NEE) is positive when GPP > ER. Analogously, net ecosystem productivity (NEP) for a given time is positive, if the ecosystem is accumulating C.

A 2.2 NEE

On 7 – 12 days per year, on snow free days between April and December, NEE was measured for five years. NEE_{day} measurements were done in full sunlight, between ca. 2 h before and after solar noon (clear sky, midday conditions). NEE_{night} measurements were started no earlier than 1 h after sunset.
To measure NEE, we used a dynamic CO_2 concentration, non flow-through cuvette made of transparent polyacrylics (30 × 40 × 35 cm). An infrared CO_2 probe (GMP343 diffusion model, Vaisala, Vantaa, Finland) connected to a handheld control and logger unit (MI70 Indicator, Vaisala) was mounted inside the cuvette to directly measure the chamber [CO_2]. A small fan created moderate turbulence inside the cuvette (0.5–0.8 m s^{-1}) to facilitate air mixing. During the measurement, the cuvette was tightly sealed to the rim of the box containing the monolith using a cell foam band.
CO_2 concentration changes were measured at 5 s intervals during a 2 min measurement period per monolith. The short measurement period was chosen to minimize changes in environmental conditions inside the chamber and avoid fogging of the cuvette at high evapotranspiration rates. CO_2 concentration did not drop below 340 ppm or rise above 500 ppm. The first 10 s of data after placement of the chamber were omitted in subsequent measurements to allow for initial adjustment of chamber [CO_2]. The quality of the measurement was considered acceptable if a linear regression of [CO_2] vs. time during the following 110 s yielded R^2 of 0.95 or better, indicating strictly linear changes

in chamber [CO_2]. During each flux measurement, soil temperature of the respective monolith was recorded at 8 cm depth, using a handheld electric thermometer.

A 2.3 Ecosystem respiration (ER)

Measured NEE_{night} was considered to represent ER for the entire day:

$$\mathrm{ER} = \mathrm{NEE}_{\mathrm{night}}$$

For the days between measurements (bi-weekly to monthly during the snow-free period), ER was parameterized. First, ER for each monolith was normalized for temperature (10 °C at 8 cm soil depth) using the exponential function for NEE_{night}/soil temperature established earlier. Second, on the basis of the ER gained during measurement nights, a normalized daily ER between measurement nights was linearly interpolated. These normalized values integrate the effects of seasonal changes of substrate availability, heterotrophic and autotrophic biomass, and soil moisture availability. Ultimately, ER on hourly basis was calculated using normalized ER values for the respective day and hourly soil temperature values.

A 2.4 GPP

NEE_{day} data were used to estimate GPP according to:

$$\mathrm{GPP} = \mathrm{NEE}_{\mathrm{day}} - \mathrm{ER}$$

GPP estimates from mid-day, clear sky NEE_{day} measurements reflect a situation without radiation limitation for assimilation. Therefore, GPP estimates reflect potential GPP at maximum radiation (GPP_{pot}) at seasonal solar altitude. At the beginning of the season GPP_{pot} was interpolated to rise exponentially between snow-melt and the first measurement of the season. Between the measurement days, GPP_{pot} was linearly interpolated for every day. This way, effects of canopy development and soil moisture availability are reflected in the model.

A 2.5 GPP light response

From clear sky NEE_{day} measurements at fully developed canopy stages, we parameterized light response curves of GPP. Between photosynthetic compensation points in the morning and evening, NEE data were collected at a frequency of 50 min or higher. Maximum GPP was observed at GR of ≥ 900 W m^{-2}. No significant differences between treatments were found, and light use efficiency α was subsequently derived from data pooled across treatments. Light response was described by a nonlinear, least squares fit of flux data to a rectangular hyperbolic light response model (Michaelis–Menten model):

$$\mathrm{GPP} = \frac{\alpha \times \beta \times \mathrm{GR}}{\alpha \times \mathrm{GR} + \beta}$$

where GPP is in µmol CO_2 m^{-2} s^{-1}, α is the initial slope of the light response curve (the light use efficiency factor in µmol CO_2 J^{-1}), β is the asymptote of GPP_{pot}, and GR is in W m^{-2}. We calculated actual GPP for each hour based on interpolated GPP_{pot} for every day of the growing season, together with hourly means of GR and previously established light use efficiency α:

$$\mathrm{GPP} = \frac{\alpha \times \mathrm{GR}}{1 - \frac{\mathrm{GR}}{900\ \mathrm{W\ m^{-2}}} + \frac{\alpha \times \mathrm{GR}}{\mathrm{GPP_{pot}}}}$$

A 2.6 NEP

NEP was used in the sense of describing the balance between GPP and ER, equivalent of an hourly, daily, or annual CO_2 balance for the ecosystem, neglecting other potential C imports or exports. Hourly ER flux rates were used to calculate hourly sums of C loss. Hourly GPP flux rates were used to calculate hourly sums of C gain for each monolith. NEP was then derived by subtracting hourly losses of respired carbon (ER) from hourly C gains (GPP) for a given period for each individual monolith:

$$\mathrm{NEP} = \mathrm{GPP} - \mathrm{ER}$$

I do hope that this is a misunderstanding – in order to dispel my doubts the authors will need to be fully transparent in terms of their methodology and (i) provide the full details of the chamber NEE measurements and show these data, (ii) detail and show how the GPP and ER models were fit to the data, and (iii) detail and show how they simulated GPP and ER over the course of their experiment. Much if not all of this can go to the supplement, but it must be accessible in a transparent and reproducible way. I do understand that some of this has been presented in previous papers of the authors, but the current paper must be able to stand alone without the need to resort to other papers in order to understand the results.

> *We think that the above details are sufficient to answer your questions. In case not, we will be happy to supply more technical details.*

(2) The irrigation treatment is not very well motivated in the introduction and the results remain inconclusive, mostly because the authors repeatedly claim a drought, which is however never explicitly shown.

> **>** *To better explain the motivation behind our irrigation treatment in the Introduction, we suggest to introduce l. 56-58*
>
> 'First, warming favors productivity, resulting in increased availability of organic matter. This effect is strongest at intermediate warming levels and becomes smaller at warming levels that cause seasonal water shortage (Volk et al., 2021).'
>
> *And l. 88 ff:* 'In addition, to uncouple potential temperature effects from temperature-driven soil moisture effects and to consider effects of atmospheric N deposition, a two-level irrigation treatment and a three-level N treatment were set up in a factorial design.'

If soil water shortage affects plant performance at this site, then I suggest the authors present and discuss the corresponding evidence (e.g. time course of plant available soil water) and better motivate the irrigation treatment in the introduction.

> **>** *Yes, water shortage does significantly affect plant performance. This topic is well covered in the companion paper on plant productivity (Volk et al., 2021), but not in this manuscript on the C budget.*
>
> *Here, the results of the irrigation treatment on the C budget turned out to be not significant. This is likely because the chosen amount of water was too small, and the issue is briefly discussed at l. 427 ff.*
>
> *Generally, we assume that water supply affects plant performance in all conditions except the optimal condition. The increased evapotranspiration, resulting from a 3 °C warming, reduces at the same time the resource of and increases the demand for water. This makes it very likely that plant performance is affected by water shortage. Yet, given our results we have to conclude that:* "… results from the current experiment must leave it open whether mitigation of water shortage due to warming would change SOC stocks" (l. 431-432).
>
> Volk, M., Suter, M., Wahl, A. L., and Bassin, S.: Subalpine grassland productivity increased with warmer and drier conditions, but not with higher N deposition, in an altitudinal transplantation experiment, Biogeosciences, 18(6), 2075-2090, 2021.

Or alternatively, the authors might consider removing the irrigation treatment from the analyses and manuscript.

> **>** *We would very much regret to omit the results of irrigation treatment because the interaction of warming and altered water availability is essential for understanding the effects of global climate change. The non-significance of the irrigation treatment on the C dynamic presented here is a result per se that has to be shown. Finally, presenting the irrigation treatment in an earlier and related study (Volk et al. 2021; see reference above) but not here will lead to confusion.*

(3) In a companion paper I see that the treatments had substantial effects on the species composition, which may provide important clues for the interpretation of the results presented in this manuscript, yet this link is hardly made. Also differences in snow melt date and phenology are hardly discussed.

> **>** *Species composition is an important, but extremely difficult subject in the context of the results presented here. At least at the functional group level, we can contribute relevant information and suggest including a statement on the* potential *effects of biodiversity, e.g. based on our previous publications on the subject (l. 433-440):*
>
> Bassin, S., Werner, R. A., Sörgel, K., Volk, M., Buchmann, N. and Fuhrer, J.: Effects of combined ozone and nitrogen deposition on the in situ properties of eleven key plant species of a subalpine pasture. Oecologia, 158(4), 747-756, 2009.
>
> 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.
>
> *Yet, we suggest not digging into the subject any deeper because only measuring changes in community composition is standard procedure. However, establishing a* quantitative *cause-effect relationship between composition changes and ecosystem C stock changes in a vascular plant system with more than 100 species seems close to impossible. Given this, we decided against an in depth discussion of the biodiversity issue because it would be merely speculative.*
>
> **>** *Indeed, the snowmelt date is an integral element of the experiment and one of the reasons why we speak of 'climate scenario' sites, rather than just a warming treatment. Nevertheless, the experiment is not designed to investigate effects of snowmelt date. To differentiate between effects of time and thermal energy would require NECB data sets that contain different vegetation period lengths with equal degree day sums.*

The number of days between snowmelt and harvest, and Degree Days above 0 °C per climate scenario can be found in the companion paper (Volk et al., 2021; Tab. 2).

(4) The authors report a loss of 1 kg C/m2 over 5 years of the most extreme (+3°C) warming manipulation – this is certainly a lot (massive), especially given the ecosystem C pool size. Expressed as an average per year, this is equivalent to a 200 gC/m2 net loss and I suggest the authors, in order to put the results of this manipulation experiment into perspective, compare with the results of studies investigating mountain grassland NEP over multiple years, including extreme climatological conditions. There are quite a few studies out there that would be suitable, even from closely related grassland ecosystems in Switzerland and Austria, but also from other alpine regions of the world (e.g. North America, Tibetan plateau, …).

> *Indeed, our data should be put into perspective better. We suggest to newly introduce a wrap-up of how we discuss them here (Discussion 4.3 l. 482 ff)).*
The individual mountain grassland studies that present annual C balances often report quite substantial C sinks in the grassland. These studies mostly use EC measurements and they unfortunately have neither multi-level treatments nor replications, which would allow to test a mechanistic hypothesis against the ecosystem response or put the reported fluxes into perspective.

For example, from a 15-year data set on the 1000 m asl site Früebüel in Switzerland, Rogger et al. (2022) reported a mean 'net biome production' (including NEE, fertilization and harvest related C fluxes) gain of 154 (± 80) g C m^{-2} $year^{-1}$. For the local topsoil, this amount would represent a C sequestration rate of ca. 4% per year, making it unlikely that the study describes typical situations, despite the long duration of the experiment.

Similarly the Hörtnagel et al. (2018) analysis of 14 managed grassland sites reported net GHG balances (including N_2O and CH_4) and revealed C sinks between 70 and 4671 g CO_2–eq. m^{-2} $year^{-1}$. This opens very interesting perspectives for the short-term development of the local SOC stock, but we find them not discussed.

Also from the European GREENGRASS network (9 sites, including very intensively managed grassland, Soussana et al. 2007), the authors report that on average the annual C storage (net biome productivity, NBP) in the grassland plots was a sink of 104 (±73) g C m-2 year-1. Specifically for the site that is most similar to our reference site

(Malga Arpaco, 1699 m a.s.l., 2 years data, + 3 °C warmer than $CS2_{reference}$, 90 kg N ha^{-1} $year^{-1}$) an NBP budget with a gain of 358 g C m^{-2} $year^{-1}$ was reported. Assuming a topsoil C stock of e.g. 6 kg C m^{-2}, the 358 g C represent a C sequestration rate of 6% per year. This demands a plausible suggestion how an annual C input that would double the topsoil C stock in 17 years could be sustained. In other words, such C sink events should be rated as extreme situations that would be balanced by C source events in the mid-term.

We suspect that such substantial C sequestration situations cannot be considered typical in permanent(!) grassland. Instead, we consider it more likely that deviation from a zero balance indicates either
A) weather driven year-to-year variability,
B) agricultural management effects or
C) problems with C flux accounting. This implies, that the published annual C budgets more often than not represent a spotlight on the highly dynamic transition between the present OC stock and a future, hypothetical OC stock. Only rarely, there are arguments available that suggest in which direction the underlying C sink/source dynamics may drive.

For example, Rogier et al. (2008) report from the CARBOMONT project that in 2003, the studied site Seebodenalp was a net source of 355 g C m^{-2}. But the authors attribute the annual C-losses on the decomposition of the drained peat soil of the study site. Moreover, the strongest C sink within CARBOMONT (Amplero) was used as an agricultural field a long time ago and might be recovering its carbon stocks.

Only in an overview of the all-European CARBOMONT project Berninger et al. (2015) found that 'Especially, the natural mountain grasslands in our study were quite close to carbon neutrality.' By comparison, the equivalent value at our $CS2_{reference}$ is -69 ±79.4 g C m^{-2} in five years, indicating a C source of 14 g C m^{-2} $year^{-1}$, essentially undistinguishable from zero (Table 3b).

Berninger, F., Susiluoto, S., Gianelle, D., & Balzarolo, M. (2015). Management and site effects on carbon balances of European mountain meadows and rangelands. Boreal environment research.-Helsinki, 20(6), 748-760.

Hörtnagl, L., Barthel, M., Buchmann, N., Eugster, W., Butterbach-Bahl, K., Díaz-Pinés, E., ... & Merbold, L. (2018). Greenhouse gas fluxes over managed grasslands in Central Europe. Global Change Biology, 24(5), 1843-1872.

Rogger, J., Hörtnagl, L., Buchmann, N., & Eugster, W. (2022). Carbon dioxide fluxes of a mountain grassland: Drivers, anomalies and annual budgets. Agricultural and Forest Meteorology, 314, 108801.

Rogiers, N., Conen, F., Furger, M., Stoeckli, R., & Eugster, W. (2008). Impact of past and present land-management on the C-balance of a grassland in the Swiss Alps. Global Change Biology, 14(11), 2613-2625.

Soussana, J. F., Allard, V., Pilegaard, K., Ambus, P., Amman, C., Campbell, C., ... & Valentini, R. (2007). Full accounting of the greenhouse gas (CO2, N2O, CH4) budget of nine European grassland sites. Agriculture, Ecosystems & Environment, 121(1-2), 121-134.

(5) The text is generally well written (but certainly will profit from English proof reading), but at times imprecise and thus ambiguous – see my detailed comments below.

Detailed comments:

Title: too bulky in my view, also avoid abbreviations – I suggest something like this: „Massive warming-induced carbon loss from subalpine grassland soils in an altitudinal transplantation experiment"

> *Good suggestion, we will adopt it. Any missing content is in the Abstract.*

l. 29: references could be more up to date

> *A few newer Refs are introduced, but please also compare our response to your general comment #4. Apart from that, we feel that we did not miss anything that is both a good match AND introducing a new perspective. If you have a specific suggestion, we would be happy to either adopt it or justify why we do not want to use it.*

l. 33: a large carbon pool size does not necessarily mean that an ecosystem is currently a sink for CO_2 – I think here the terms pools and fluxes are mixed or at least the statement should include a temporal perspective? Since for CH_4 and N_2O soils are, compared to the corresponding chemical destruction (sink) in the atmosphere, really minor sinks, I think the term "greenhouse gases (GHG)" should be replaced by "CO_2"

> *Agreed. We changed to:*

‘Indeed, today grassland soils are one of the largest terrestrial CO_2 sinks, because they contain a pool of 661 Pg C (ca. 28% of total global soil C; Jobbágy and Jackson, 2000) or >80% of C contained in the atmosphere.’

But because sink does not necessarily imply that the stock is growing at all times, and a temporal perspective would also require to distinguish between night and day, we prefer to keep the term ‘sink’. See also next response.

l. 46: “… the largest soil C sink” – similar to the above comment - isn’t this statement confusing a pool/stock with a flux of carbon as the next sentence refers to pools?

> *We don’t share the opinion that the noun ‘sink’ necessarily implies a flux. Please compare*
https://www.merriam-webster.com/dictionary/sink
on the issue.

l. 47-48: in order to make sense of this statement one would need to know how land is fractionally distributed with elevation in Switzerland; I presume that because of the mountainous terrain land area decreases with elevation and thus the given numbers indicate that a larger proportion of SOC is found at higher elevations, but this needs to be explained, e.g. by saying that 24 % of SOC is found at elevations > 2000 m despite these areas representing just x % of the total land area

> *Agreed, needs improvement to make sense. Now reads (l. 46 ff):*
‘This leads to the apparently paradox situation that less productive ecosystems support larger soil C sinks. In Swiss grasslands for example, more than 58% of SOC is stored at 1000-2000 m a.s.l. (37% of the total area), and despite the very shallow and cold soils 24% of SOC are found above 2000 m altitude (21% of the total area; Leifeld et al., 2005; Leifeld et al. 2009). As a result the 1000-2000 m a.s.l. region stores 3.6 times more SOC per unit land area, compared to the < 1000 m a.s.l. region, and the > 2000 m a.s.l. region stores 2.7 times more SOC, respectively.’

l. 50: shouldn’t GHG here be replaced by CO2?

> *Agreed, and CO_2 is now used.*

l. 61: undesired in what sense and from which perspective?

> *In order to avoid adding a discussion on undesired eutrophication and biodiversity loss vs. desired agronomic yield increase and potentially larger OM input to support the SOC sequestration potential, ‘undesired’ is omitted.*

l. 66-67: "… the input of organic carbon to the terrestrial carbon sink" – suggest to reformulate

> **>** *We suggest (l. 69):* ' …, warming and N deposition therefore … lead to a larger input of organic carbon to the terrestrial carbon sink.'

l. 85: the irrigation treatment is poorly motivated in the introduction – the warming and nitrogen addition is motivated as to increase productivity – does that imply that these systems are limited by water availability and thus the authors expect an increase in productivity by alleviating this limitation? If so this needs to be introduced in the introduction

> **>** *Please compare our response to your major comment #2 that refers to the same group of issues.*

l. 95: "drought due to warming" – does that mean the authors expect the warming treatment to increase evapotranspiration and thus cause decreases in soil water availability which are strong enough to limit plant productivity? If so this needs to be introduced in the introduction

> **>** *Please compare our response to your major comment #2 that refers to the same group of issues.*

l. 95-96: what about a positive effect of additional nitrogen on plant growth?

> **>** *Yes, there is certainly such an effect. The positive effect of additional N on plant growth and its relevance for this study is well covered in the Introduction. But it is not a hypothesis we test here.*

l. 122: soil surface?

> **>** *Yes, text now reads (l. 130)*
> '… drained plastic boxes, at level with the surrounding soil surface, resulting …'

l. 136: isn't this a bold assumption given that l. 119 states that the site is covered by snow for 6 months every year, that is to say that even a small CO_2 emission rate during the period of snow cover may accumulate to a significant fraction of the annual carbon budget?

> **>** *Sorry for being unclear. Of course ecosystem respiration was parameterized using the CS soil temperatures for the full winter period. The sentence in question only describes why we expressed the site characteristics with the growing season temperature, instead of the annual mean temperature. We suggest the new text (l. 142-144):*
> 'The climate scenario treatment was induced by the different altitudes of the CSs at the AlpGrass site, where the monoliths were installed. As a result, the transplanted

monoliths experienced distinctly different climatic conditions (Table 1). To describe the climate scenarios, we focused on the mean growing period temperature from April to October, instead of the annual mean temperature. The temperature under the snow cover was ca. 0 °C at all CSs. The CS temperature treatment was defined as the deviation from CS2reference temperature.'

l. 148: replace "several" with the actual number of irrigation applications for the two treatments and give the corresponding total amounts

> *The treatment levels were irrigated and non-irrigated. We suggest to replace the paragraph (l. 157-160) and write:*

'A two-level irrigation treatment was set up to distinguish the warming effect from the soil moisture effect, driven by warming. Precipitation equivalents of 20 mm were applied to the monoliths under the irrigation treatment in 4-6 applications throughout the growing period. Depending on the year, this treatment amounted to 80-120 mm or 12-21 % of the recorded precipitation sum during the growing periods.'

l. 154-155: 12 x 0.2 l / 0.1 m2 = 24 l/m2 – is that correct? that represents around 5 % of the natural precipitation of CS6? Are these amounts included in the calculated irrigation amounts?

> *Mean annual precipitation at CS6 is 687 mm (Table 1). N deposition of 24l/m^2 = 24 mm is about 3.5% of annual precipitation. This is not included in the irrigation amounts since this amount of water was applied to all monoliths, irrespective of the irrigation treatment.*

l. 157: replace "Meteorology" with something like "Environmental conditions"?

> *We adopt your suggestion*

l. 159: was soil temperature and SWC measured inside the plastic containers and if so with how many replicates?

> *Borrowing from the companion paper we suggest to elaborate at l. 176-179:*

'2.5 Environmental conditions

At all CSs, air temperature, relative humidity (Hygroclip 2, Rotronic, Switzerland), and precipitation were measured (ARG100, Campbell Scientific, UK). Soil temperature and SWC were measured at 8 cm depth (CS655 reflectometer, Campbell Scientific, UK). At CS2reference and the lowest CS6 these parameters were obtained in 18 monoliths and at two points in the surrounding grassland, using time domain reflectometers (TDR) with 12 cm rods (CS655, Campbell Scientific, UK). In all other CSs six monoliths each were equipped with such TDRs. All parameters were integrated for 10 minutes originally and later averaged for longer periods if necessary.'

l. 162-165: are these data reported somewhere in the manuscript? If not remove

> *The background N deposition is important to put our N treatment into perspective. We therefore suggest to add these data by rewriting l. 162-165 as follows (l. 181-185):* 'Ambient wet N deposition was 3.3 kg N ha^{-1} a^{-1} at CS2reference and 3.3 kg N ha^{-1} a^{-1} at the lowest CS6. Wet deposition was collected using bulk samplers (VDI 4320 Part 3, 2017; c.f. Thimonier et al., 2019) from April 2013 to April 2015. Nitrate ($NO3^-$) was analyzed by ion chromatography (ICS-1600, Dionex, USA) and NH4+ was analyzed using a flow injection analyzer (FIAstar 5000, Foss, Denmark) followed by UV/VIS photometry detection (SN EN ISO 11732).'

l. 168: "Aboveground plant material …"

> *Adopted*

l. 169: when was maturity reached approximately?

> *We will convert material from Table 2 of the companion paper to one sentence and add in l. 189-190:*

'Accordingly, mean harvest dates for CS1 to CS6 were 12. Aug., 26. July, 22. July, 14. July, 9. July and 5. July, respectively.'

l. 177: productivity is a rate and thus needs to include some time units

> *We agree, compare e.g. Table 3.*

In l. 198 we will replace

'In the context of this study productivity is expressed as g C m^{-2}.' *with*

'In the context of this study productivity is expressed as g C m^{-2} per time unit.'

l. 179: how was CO2 measured and at what frequency, i.e. how many data points were available for the regression? What about the initial data after chamber closure (deadband) – were some data excluded? Need to state sign convention for NEE, NEP, GPP and ER

> *Please compare the A 2.1 CO_2 flux sign convention section and the A 2.2 NEE section in the add-on to the Appendix that we suggested in our response to your major comment #1. We hope you find the issues covered satisfyingly.*

l. 189: the measurement of global radiation was so far not mentioned (section 2.5)

> *The information is now added to the new Environmental conditions section (l. 174-175):*

'Global radiation as W m-2 was measured at $CS2_{reference}$ and CS6 using Hukseflux LP02-05 thermopile pyranometers.'

l. 191: I think the corresponding equations should be given in order to save the reader to switch back and forth to older papers from the authors

> *Please compare the add-on to the Appendix that we suggested in our response to your major comment #1, which refers to the same group of issues. We hope you find the issues covered satisfyingly.*

l. 192: does "the light response curve of GPP was derived at CS2_reference" mean that the parameters that were determined at CS2_reference were applied also at CS4 and CS6? What is the underlying idea/justification for this approach given that apparently NEE was measured at all sites? The parameters of the GPP response represent the combined effects of canopy structure and leaf-level plant physiology. By assuming these to be the same at CS4 and CS6 you are implicitly assuming that phenology (e.g. due to different snow melt or harvesting dates), canopy structure and leaf-level photosynthetic characteristics of the plant species are the same – how do you justify this assumption? If I understand this correctly, then actually GPP should be the same at CS2_reference, CS4 and CS6 unless there are differences between the sites in global radiation – is this correct? Why does then Figure A2 show differences in GPP? Every summer, except for 2017, there is a depression in GPP –is this reflecting the harvest of the above-ground plant material (> 2 cm) applied to mimic grazing or something else (drought)?

> *Indeed, not integrating e.g. canopy development, water availability and temperature in a climate change experiment would have been not sensible. But our parameterization of GPP (and ER) includes exactly these factors. This is why you find different GPPs per CS and per year in Fig. A2, which you quoted. Please compare the* GPP *and* GPP light response *section in the add-on to the Appendix that we suggested in our response to your major comment #1. The summer depression of GPP, visible in Figure A2, is indeed a reflection of the aboveground harvest.*

l. 194-195: given that snow cover apparently lasts for 6 months this is a non-trivial assessment; Scholz et al. (2017) found that a grassland at similar elevation in Switzerland emitted on average around 0.3 g C/m/d during the period of snow cover, which would amount to around 60 g C/m2 during a 6-month period of snow cover – this value could be used to cross-check your assumptions; also other studies from mountain grassland in

Switzerland and Austria may be used to that end, e.g. Rogiers et al. (2005), Rogger et al. (2022), Hörtnagl et al. (2018)

> ***>** Thanks for pointing to the Scholz et al. paper. The 0.3 g C/day value is probably not the mean snow covered period ER, but the non-CO2-Uptake Period (non-CUP) ER mean. The non-CUP lasted almost nine months (from 27. Sept. 2013 until 22. June 2014) with a maximum ER of 1.49 g C m-2 day-1 and a mean of 0.32 g C. This period includes potentially very warm days in the fall and in the spring, with no green vegetation, but warm soils. In the Discussion, Scholz et al. refer to an 'average winter flux of –0.33 g C m^{-2} d^{-1}'. We do not find the expression 'winter flux' or the number elsewhere in the paper. It may be a differently rounded non-CUP ER, though. Also, the authors report that 'During the snow season, CO_2 release continued with declining CO_2 emission rates over time.' We find that statement nowhere quantified and therefore tested the line for cumulative NEP (Scholz et al. Fig. 3) during the snow covered period graphically instead. We doubt that it differs significantly from a straight line. With respect to the other papers, please compare our response to your major comment #4. Indeed, winter respiration is important. In our experiment both soil temperature and growing season length are used to parameterize ER in winter. Only we have no parameterization for a potential substrate limitation of respiration. Our daily ER in winter is between 0.1 g and 0.2 g C m^{-2} day^{-1}.*
>
> *If therefore a typical C loss of 0.15 g/day and a worst case error of 50% (due to unaccounted substrate limitation) is assumed, then our balance may be off by 0.075 g C × 183 days × 5 winters = 69 g C m^{-2}. Compared to a 5 year cumulated ER between 3 and 4 kg C m^{-2} (Table 3b) this amounts to an error of 2.3 – 1.7%. Compared to other sources of error, this value may safely be considered irrelevant.*

l. 197: should add that cumulative NEP was derived as simulated GPP-ER?

> ***>** Done (l. 215). Please also compare the corresponding sections in the add-on to the Appendix.*

l. 211: how was leaching estimated for the other years?

> ***>** We computed a mean annual leachate C loss from the three years of available data and scaled this value to five years.*
>
> *Considering that we had covered 60% of the time and that the resulting five years sum of C exported as leachate amounted to only 0.6% ($CS2_{reference}$) and 0.5% (CS4 and CS6) of the total ecosystem C loss, the potential error seems negligible.*

l. 218: more accurate compared to what?

> *The sentences (l. 240 ff) have been amended to:*

'Data were modeled for C stocks and C fluxes. SOC stock data were available for 2012 and 2017, to calculate the SOC stock change over the five experimental years. We used SOC stock change as the primary variable for the analyses of the CS treatment effect. Shoot and root C stock data were available from the destructive harvest at the end of the experiment in 2017. … '

l. 308: this is the first time the authors mention that apparently soil moisture and canopy development play a role in simulated GPP and ER … this needs to be introduced in the methods section

> *This point has now been clarified in the add-on to the Appendix that we suggested in our response to your major comment #1.*

l. 311: if simulated GPP is based just on CS2_reference then this is what I would expect …

> *Please compare our response to your l. 192 comment that covers exactly this subject.*

l. 315: cumulative NEP

> *Done. For consistency it should be 'five year NEP' (l. 340) because the sentence refers to Fig. 4*

l. 359: better say that the reference for the ecosystem C balance response to the climate scenarios is air, not soil temperature

> *Will improve to (l. 382-384):*

'It is important to note, that our description of the C balance temperature response is not based on soil temperature, but based on air temperature change, because it is the reference to describe climate change effects on ecosystems.'

l. 362-363: nevertheless this is what you do in order to simulate cumulative ER …

> *This is the best proxy available. We postulate that a single known soil temperature in our system is on a mid-term average associated with ± the same set of ∞ unknown soil temperatures, along the time and space gradients. New text reads (l. 384-386):*

'Also, under field conditions there is no single soil temperature, but an extremely dynamic, diurnal soil depth temperature gradient, that drives the CO_2 evolution from various organic matter fractions with different temperature sensitivities (Conant et al., 2011; Subke and Bahn, 2010).'

l. 377-385: what is the role of changes in species composition, e.g. in terms of the major plant functional types, reported in other papers by the authors for the observed changes in the R/S ratio? These changes in species composition may provide important insights to that end

> **>** *The change in species composition is an interesting, but extremely difficult subject. At least on the functional group (FG) level, we can contribute relevant information. Despite the lack of R/S data on the FG level, we will include a statement on the potential effects of biodiversity at the end of paragraph 4.1 (l. 433-440), e.g. based on the previous publication on the subject that contains the climate treatment.*
>
> 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.
>
> *Beyond that, we suggest not to dig into the subject any deeper, because establishing a quantitative cause-effect relationship between composition changes and ecosystem C stock changes driven by R/S ratios in a vascular plant system with more than 100 species seems impossible to us. We thus decided against an in depth discussion of the biodiversity issue because we feel that it would be highly speculative.*

l. 405: I would understand the argumentation that adding water in a situation where there is enough water does not have an effect but if water is a limiting factor wouldn't alleviating this limitation have some effect?

> **>** *Agreed. Therefore, we wrote*
>
> 'We assume that the applied amount was insufficient to make a difference, in particular at the warmer CSs, because we deem it likely that water was a limiting factor there (Volk et al., 2021).' *l. 405-407.*
>
> *Now expanded at l. 427-432.*

Maybe the additional water might not be enough to trigger a plant response, but possibly microbial respiration would be boosted, as is for example observed after rainfall events in dry ecosystems? This discussion would also profit from soil water content data giving us an idea of how irrigation affected plant available water and in general to what degree the studied systems experienced drought conditions.

> **>** *The applied amount of water was sufficient for a significant effect on plant growth in dry years (Volk et al., 2021) but not on the 5-year C budget. Given this, discussing potential microbial respiration seems inappropriate here. We will introduce a reference to Table 2. in Volk et al., 2021, that contains data both on the relative difference of*

water availability between the different CSs and on the effect of the irrigation treatment on water availability (l. 430-431).

l. 414: as mentioned above, this might be an artefact of the way GPP is simulated; if true, this would mean that changes in species composition and structure of the above-ground vegetation, reported by the authors in other papers, had no effect – this might be worth discussing

> **>** *We assume that the questions around our simulation of CO_2 fluxes are answered with our replies to your major comment #1 (ff)*
> *With respect to your reference to our finding (now l. 445-448)*
> 'Despite a positive effect of warming on aboveground plant productivity (Volk et al., 2021), the five years GPP flux – quantifying the total amount of assimilated C – was not significantly different between climate scenario treatments CS2reference and each of CS4 and CS6 (Fig. 4).'
> *we are asking for a literature hint: We would be happy to include a short discussion and reference of a study that shows how species composition or canopy structure (not biomass) significantly affected the amount of C assimilated by the canopy.*

l. 417: since ER includes also respiration of above-ground plant components that was not quantified here, does the comparison to soil respiration make sense? Why not cite other mountain grassland studies which actually have quantified annual ER instead?

> **>** *We didn't find any that matched better in terms of climate/altitude/productivity than the Bahn et al. 2008 paper. Also, the plant respiration term is likely not very large in an ecosystem that yields a harvest of 80 g C m^{-2} a^{-1}. The overview on CarboMont does not provide separate ER budgets and the reports from the individual sites (that we are aware of) suffer the problems we described above in detail in our response to your major comment #1.*

l. 436: is that latter statement supported by your data?

> **>** *Yes, it is.*
> *This is most obvious in Fig. 1a, showing the best available temperature resolution of SOC stock changes, that ultimately reflect NEP:*
> *With increasing temperature (from left to right) the C stock change became positive (growing NEP) up to the +0.7 °C CS3 site, indicating predominantly beneficial warming effects for a positive NEP without concomitant detrimental drought effects = trade-off positive.*

With further increasing temperature C stock change became negative (decreasing NEP), indicating (potentially) beneficial warming effects with predominantly detrimental drought effects for a negative NEP = trade-off negative.

We suggest to rephrase (468-469):

'We thus conclude that the wide range of possible NEP responses to warming depends on the warming benefit vs. water limitation trade-off, when the temperature is rising.'

l. 445: what about the role of the exchange of other gaseous C components, such as CH4 and the large group of biogenic volatile organic compounds?

> *The fact that the sum of NEE + harvest + leachate fluxes matches the observed change in the ecosystem C-stock (Table 3 a and b), shows that methane and volatile organics do not play a role in quantitative terms.*

l. 446: 1 kg C/m2 over 5 years equates to around 200 gC/m2 – how does that compare to mountain grassland studies which report annually resolved NEP? For example, in a climatologically extreme year, have net carbon losses on the order of 200 gC/m2 been reported?

> *Annually resolved data for ER in low productivity grassland is not easy to find, even though for example CarboMont covered 2003, the year of the so called 'European heat wave'. It resulted in 4-5.5 °C warmer mean summer temperatures (most pronounced in June and the first half of August) in Switzerland, compared to the then norm values. But this is an example for a so called extreme event and not directly comparable to the moderately and continuously higher temperatures in our experiment.*

But for example, the already quoted Rogier et al. study from a drained peatland, which is always a CO_2 source, shows an ER increase of 268 g C m^{-2} a^{-1} in 2003 with the 4-5.5 °C increased mean summer temperatures. This compares well with our ca. 200 g C m^{-2} a^{-1} increase with a + 3.0 °C April to October temperature.

Also from the ten year (including 2003) Oensingen grassland study the authors report a 'weather attributed NECB variation' of 100-200 g C m^{-2} a^{-1} (Ammann et al., 2020). On the other hand, from a number of FLUXNET sites, Reichstein et al. (2007) reported reduced ER in 2003 and attributed that to drought stress coinciding with the high temperatures. The only analyzed grassland site has a -19 g C m^{-2} month-1 reduction in ER.

A wrap-up on these studies is now included in the Discussion section 4.3. (l. 482 ff).

Ammann, C., Neftel, A., Jocher, M., Fuhrer, J., & Leifeld, J. (2020). Effect of management and weather variations on the greenhouse gas budget of two grasslands during a 10-year experiment. Agriculture, Ecosystems & Environment, 292, 106814.

Reichstein, M., Ciais, P., Papale, D., Valentini, R., Running, S., Viovy, N., ... & Zhao, M. (2007). Reduction of ecosystem productivity and respiration during the European summer 2003 climate anomaly: a joint flux tower, remote sensing and modelling analysis. Global Change Biology, 13(3), 634-651.

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.

> *These considerations are touching an important, but extremely difficult subject. At least on the functional group level, we can contribute relevant information and 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., Werner, R. A., Sörgel, K., Volk, M., Buchmann, N. and Fuhrer, J.: Effects of combined ozone and nitrogen deposition on the in situ properties of eleven key plant species of a subalpine pasture. Oecologia, 158(4), 747-756, 2009.

To take up the most important issues, we have now added at the end of paragraph 4.1 (l. 433-440):

'Warming, nitrogen and water must also be expected to affect plant species composition, which in turn may affect ecosystem C fluxes. In a very similar environment Bassin et al. (2009) studied eleven key plant species of a subalpine pasture and found only very small responses of growth to N deposition, except for the cyperaceous *Carex sempervirens*. Within the experiment described here, Wüst-Galley et al. (2020) predicted an increased grass cover at the expense of forbs and legumes with rising temperatures and N deposition, while they found increased sedge cover with cooler temperatures and N deposition. In consequence, changes in plant species composition in response to the applied climate change scenarios can be assumed, but attempting to predict effects on the ecosystem C stock would be highly speculative.'

We suggest not digging any deeper into the subject, because establishing a quantitative cause-effect relationship between composition changes and ecosystem C stock changes in a vascular plant system with more than 100 species 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. Also, given the complexity of the grassland system as noted in the previous comment, we cannot see how either a crude proxy such as species number or a more complex index such as species evenness would help to explain any outcome regarding C dynamics. For all of these reasons we would rather not introduce additional variables into the statistical models.*

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 in the bottom corners 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. Most importantly, we assume that such a hypothetical shift in species composition had no effect on the ecosystem C budget.*

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?

> *Agreed. To make clear that there are two complete, separate sentences an extra comma before the 'and' was introduced (l. 100-102):*
3) Irrigation mitigates effects of water shortage 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.

> *There are more recent studies, despite the expensive free air CO_2 fumigation research has peaked already a while ago. However, low productivity grassland received much less attention than agronomic systems or forests*.
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 CO2'.

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

By contrast, the Jasper Ridge experiment, which 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 meta-analysis from 2010 finds that beyond the generally small response to CO_2 there is no significant relationship between CO_2 concentration and response size. This suggests that CO_2 saturation of photosynthesis and plant growth are 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 because the CO_2 issue will not be further addressed in the manuscript, we suggest to simply omit l. 67-69, rather than extending the text significantly and describe our conclusions from the above literature.

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 if they can. When we probed the original sites of the monoliths, we generally found 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.*

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?

> *The treatment combinations are actually full factorial. The sentence has been changed to (l. 135-136):*
>
> 'Within both irrigation treatment levels monoliths were subjected to three levels of N deposition.'

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.

> **>** *We agree that 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 actually 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).

> **>** *Clearly, this statement needs to be put into perspective more carefully. To achieve this, we added a new paragraph in Discussion 4.3 (l. 486-507).*
> *Also, we feel that ‘massive’ and 14% loss in five year do not contain any kind of contradiction. If, hypothetically, the 14% loss in five years would continue, after 35 years there would be only bare sand left. Please also compare this loss to the claim of the renowned ‘4p1000’ initiative (https://www.4p1000.org/) that 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.*