Biogeosciences Discussions

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Comment on bg-2021-287

Anonymous Referee #2

Referee comment on "Massive warming-induced carbon loss from subalpine grassland soils in an altitudinal transplantation experiment" by Matthias Volk et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-287-RC2, 2022

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

(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.
(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, …).
(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"
l. 29: references could be more up to date
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"
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?
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
l. 50: shouldn't GHG here be replaced by CO_2?
l. 61: undesired in what sense and from which perspective?
l. 66-67: "… the input of organic carbon to the terrestrial carbon sink" – suggest to reformulate
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
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
l. 95-96: what about a positive effect of additional nitrogen on plant growth?
l. 122: soil surface?
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?
l. 148: replace "several" with the actual number of irrigation applications for the two

treatments and give the corresponding total amounts
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?
l. 157: replace "Meteorology" with something like "Environmental conditions"?
l. 159: was soil temperature and SWC measured inside the plastic containers and if so with how many replicates?
l. 162-165: are these data reported somewhere in the manuscript? If not remove
l. 168: "Aboveground plant material …"
l. 169: when was maturity reached approximately?
l. 177: productivity is a rate and thus needs to include some time units
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
l. 189: the measurement of global radiation was so far not mentioned (section 2.5)
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
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)?
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 gC/m/d during the period of snow cover, which would amount to around 60 gC/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)
l. 197: should add that cumulative NEP was derived as simulated GPP-ER?
l. 211: how was leaching estimated for the other years?
l. 218: more accurate compared to what?
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
l. 311: if simulated GPP is based just on CS2_reference then this is what I would expect …
l. 315: cumulative NEP
l. 359: better say that the reference for the ecosystem C balance response to the climate scenarios is air, not soil temperature
l. 362-363: nevertheless this is what you do in order to simulate cumulative ER …
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
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? 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.
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
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?
l. 436: is that latter statement supported by your data?
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?
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?