

We would like to thank all five reviewers for their detailed, constructive and positive feedback on our original manuscript “Winter GHG emissions in a sub-alpine grassland”. We believe the comments improved the manuscript considerably. Here, we respond to all general and specific comments of each reviewer separately (regular font is the reviewer’s comments, italic font represents our answer).

Reviewer 3, Benjamin Runkle:

The authors present a good dataset on greenhouse gas fluxes from a snow-covered grassland during the under-measured winter season. This work was carefully performed and is of strong interest to the scientific community. I think more studies should analyze these three major greenhouse gases, use a variety of methods to cross-validate methods and investigate spatial heterogeneity, and take a year-round approach that includes the winter season presented here. The data and its presentation are generally of the quality expected by Biogeosciences, and I think the work should be published there. However, before its publication there are considerable revisions necessary to work out how to be present the data collect, how to contextualize it with respect to other studies at this and other sites, and in improving the quality of the written English.

I do not wish to re-examine the points raised in the other reviews, so present my key suggestions in the comments below. I strongly recommend going over the writing with a fine-toothed comb to work through the structural, textual, and conceptual issues raised in this and the other reviews, and suggest letting a native English speaker assist in the final editing.

We would like to thank B. Runkle for his constructive comments and tried to implement all of the issues raised in the revised manuscript.

Specific comments

p. 402, line 13 – I suggest putting the study time period (November – April) before you present anything about the results (ideally in the first or second sentence of the abstract); additionally I wonder why in the abstract the time period is Nov-Apr but in the text the measurements began in December and went only to mid-April, the snow cover started 19 Nov, and the upscaling starts 16 Nov.

We corrected this in the new version of the manuscript, while permanent snow cover occurred already in mid-November we were only able to install our instruments at the beginning of Decemeber due to technical difficulties. This further affected our up-scaling/data interpolation activities which were adjusted to 1st of Dec. – 31st of March in the revised manuscript.

p. 403, lines 8-11 – I rarely find one-sentence paragraphs warranted and suggest either expanding the thoughts presented in this paragraph or folding it into one of the paragraphs above or below it.

This has been changed as large parts of the introduction have been restructured.

p. 404, lines 5-1 – This paragraph requires a topic sentence giving us some context and thesis for what the paragraph will present. Also please be more specific when saying “the most important N₂O sources” – is there some quantitative proportion or magnitude available?

Similar to the answer given before, this part of the previous introduction has been rewritten and “the most important N₂O sources” statement was changed to “considerable” N₂O sources. Nowadays the largest N₂O emitters are artificial fertilizers, fossil fuel combustion and husbandry.

p. 404, line 28-page 405, line 2 – this long line of citations deserves a bit more explanation. Are these papers which include other GHGs, or do not? Are they relevant comparisons (e.g., of the ecosystems and time periods studied here)?

These papers either include other GHGs in similar ecosystems or were undertaken during winter and therefore are relevant for comparison. We believe that the papers cover the most important facts of the current knowledge on winter GHG emissions and a reliable review on winter GHG emissions is currently unavailable.

p. 407, More information should be provided about the EC set-up, particularly as the valley sounds quite small and possibly steep. Is there flat enough terrain to suit the EC method? What is the average footprint size? Do any wind directions require screening out? At the maximum snow depth the measurement system is less than 1 m above the surface – are there any additional considerations during this period? Does any of the spatial heterogeneity uncovered during the transect measurements make an appearance in a footprint model (or even by wind direction)?

We hope to have clarified the issues raised by B. Runkle in the revised version of the manuscript. We include a calculated footprint after Kljun et al. 2004 in Figure 1b clearly showing that the footprint was within the observed grassland. Further such valley locations are suitable for EC measurement for two reasons: (1) the valley is not as narrow as one would think (see also <http://de.wikipedia.org/wiki/Dischma>) and (2) the wind direction in such valley is most commonly along the valley either in or out of it. Further the mast height was adjusted (since we used a telescope pole) to the snow height during the course of the season. None of the spatial heterogeneity makes an appearance in the footprint model.

p. 410, line 2? I would have suggested starting a new paragraph somewhere in this region, but seeing the (non)results from the ²²²Rn work, I suggest shortening this section considerably – it is not so important to get into the details of how this would measure. I do appreciate that you left this work in the text even though it failed as I think it can provide useful lessons for others in the community – both about an interesting tracer for use in measurement and about the potential challenges this work entails. Probably lines 4-12 can be put into one sentence with a reference. [Reduce also the discussion on the bottom of page 418; perhaps you could measure Rn in the samples from the ski-pole method?]

We thank B. Runkle for his statement on this specific topic and shortened the discussion on this methodology in the revised manuscript considerably. This also included a shortening of the paragraph in the introduction. Concerning the comment of measuring Rn in the samples from the ski-pole – such a measurement is with the currently available techniques not possible.

p. 411, line 20 “In March...” – to what temperature did the air reach? Was the

temperature increase maintained?

Temperature rose above +10°C and maintained mostly above zero till April (see Figure 2a).

p. 412, line 10: Was there truly a significant linear relationship in 100% of the measured gradients?

We observed in the majority of the measured gradients for CO₂ a linear relationship (>80%). In contrast we found less linear relationships for CH₄ and N₂O (≈60%). There was no clear pattern of possible non-linear relationships therefore we applied linear functions to calculate each respective GHG flux. Similar approaches have been used previously. Certainly we are aware of publications that stated non-linearity of CH₄ increase in arctic ecosystems due to e.g. methane ebullition. Ebullition is most likely not occurring in the grassland under observation since the site is mainly characterized methane uptake rather than methane release.

p. 413 Driving factors – have you tried a day-of-year variable? This may help sort out whether SWE is a true driver of fluxes or is just a convenient indicator for time and/or snow height. I also wonder if you can derive a roughness length (i.e., z₀) from the eddy data to see if the landscape “smoothness” could correlate to the fluxes.

We would like to thank B. Runkle for his thoughtful ideas. We did not directly try the day-of-year variable, however we argue that such a variable is already included in the calculation of SWE. SWE is most commonly smallest at the beginning and end of the winter season, despite large snow density due to heavy wet snow. We derived the roughness length – which was needed for the flux footprint calculation – and could not find a correlation between CO₂ fluxes measured by EC to landscape “smoothness” as hypothesized by reviewer 3.

p. 414, line 16; it should be obvious which methods you are referring to (i.e., the 670 g/m²) is from the gradient approach, right?

Indeed, this must be stated more clearly and was corrected in the revised manuscript including the revised values.

“Seasonal budgets derived by the gradient measurements of CO₂, CH₄ and N₂O during the period of 1st of December until 31st of March (121 days) were 541 g CO₂ m⁻², -0.4 g CH₄ m⁻² and 0.11 g N₂O m⁻², respectively. Cumulative emissions for CO₂ measured by EC were slightly lower (516 g CO₂ m⁻²) than the values calculated from the concentration gradients.”

p. 415, section 3.5: I suggest putting this section above the budget part as the reader wonders why it isn't being used or mentioned yet.

Done.

p. 416, Section 3.7: Are there differences in the edges of the valley in terms of solar radiation received or aspect? It appears there could be some justification for exploring these questions in the transect's margins as presented in Fig 8.

As stated before the Dischma valley is not as narrow as one would think of. However we acknowledge the comments of reviewer 3 and possible differences in solar radiation are assumed to be minor due to the NE-SW location of the valley. We further included a Figure with global radiation in the revised manuscript (Fig. 2d). Largest differences in solar radiation at the snow surface across the valley are most likely occurring due to the ecosystem type (snow covered grassland vs forest).

p. 416, line 8: What is “the filled ground”? This should be defined in the study site description and also not used independently in Figs 8-9 since its meaning is unclear.

Our revised objectives “...(iii) placing the grassland CO₂ fluxes in context with the surrounding ecosystems.....” state a comparison with the surrounding ecosystems which we briefly name in the study site description in the new version of the manuscript.

p. 422, line 4: Are these studies all for wintertime fluxes? If so add “wintertime” before “values”.

Done.

p. 422, line 24-5: Aren't the units g m⁻²?

We thank B. Runkle for this tiny but important remark. We corrected the revised manuscript accordingly.

p. 424, line 2: here there is a reference to the annual budget, but do we know a range of the annual budget (or even the non-wintertime budget) of this site? Can this work's results be contextualized in this way?

Unfortunately we were unable to keep the site running year-round in order to derive a non-winter time budget. However we still believe these results can be contextualized since we have non-winter seasonal budgets from similar grasslands in Switzerland (e.g. Merbold et al. 2011, BGC). However possible changes in winter GHG flux with ongoing climate change remain largely unknown.

p. 424, line 5: “is crucially needed” – but why? If it is only 5% of the flux terms is such a detailed analysis really justified? If it is “crucial” please expand on why (perhaps in view of land use changes).

We state the word “crucial” since the current understanding on the drivers of GHG fluxes in winter remains unclear since currently available research studies found a diversity of drivers for several ecosystems. Furthermore researchers are still challenged in disentangling physical and biological drivers of winter GHG exchange.

Figures, tables generally: use capital letters to start labels (including the word “mean” in Table 1; “contribution” in table 3, “Julian” in Fig 4, and elsewhere).

Done.

Table 2: I would suggest the following caption: “Monthly averaged (measured weekly and modeled with meteorological drivers) and winter season mean of CO₂ flux data ...”

We adjusted the Table caption of Table 2. “Table 2: Monthly averaged (measured and gap-filled) and overall mean of CO₂, CH₄ and N₂O flux data derived by the gradient approach and the eddy covariance method.”

Table 3: Can this include error bars on the terms?

Since these are monthly averages we calculated the SD for the respective values.

Fig 1: Maybe laying these maps horizontally would be more space efficient? In my printed version (a) and (b) are not very clear, but perhaps it will look better in the final.

We revised Figure 1 which now further includes the footprint of the EC tower.

Fig 3: The “average seasonal fluxes” could be more clearly distinguished from the other measurements (maybe with coloring and/or different marker shapes). Could “continuous” be a better word than “permanent”?

Done.

Fig 4: Please be more specific than “standard procedures” and than “deviation” – is this the standard deviation or range?

We further cite Reichstein et al. 2005 and refer to the online gap-filling tool to explain the presented uncertainty. In brief “the gapfilling procedure is repeated with increased window sizes until the value can be filled. Both, the method, the window size, and the number and the standard deviation of values averaged is recorded then, so that for individual purposes appropriate data can be selected and e.g. uncertainties can be estimated. Uncertainties are also calculated for actual measurements by simulating a gap and applying the gap-filling procedure and are found in the column fs_unc of the output file.” The uncertainty given here seem to be a better estimate than the standard deviation. We further revised Figure 4 e, including real measured high quality data, gap-filled data and the daily averages derived from the gap-filled data.

Fig 5: I think the second (d) should be (e); I would also end the caption with “...the 95% confidence intervals on the fit lines”. I would change the x-axis label on figures a and c to “Temperature at the snow-soil interface” or “Snow-soil interface temperature”.

Figure 5 was changed in the revised manuscript since the temperature response seemed to be an artifact in the data, leading to unrealistic values of Q₁₀. Therefore we only included snow water equivalent as a driver of CO₂ and CH₄ flux at our grassland.

Fig 6. I tend to agree with Reviewer 1 that this is unnecessary; if you do include it add

“are” before “incorrect” and make “photo credit” two words.

Done.

Fig 7 – do not connect lines at the points where the transverse and longitudinal “transects” (not “cuts”) meet.

Done. We further separated both transects in the revised version of Figure 7.

Fig 8: replace “done” with “performed” and don’t let “ArcGIS” be split at the end of the line.

Done.

Fig 9: can you add an “n” value of samples analyzed for each? Also add to the caption something about how the fluxes were estimated so that it can be read independently (i.e., via the gradient method; what time period).

Figure 9: Boxplot of the CO₂ flux calculations based on CO₂ concentration gradients across the Dischma valley. The grey highlighted box of the grassland data indicates significantly different fluxes from the other three ecosystem types.

Technical comments

p. 402, line 20: emission à _emissions

Done.

p. 402, line 22: according snow à _according to snow

The original sentence was replaced by “While this study revealed the major drivers of CO₂ and CH₄ fluxes in this grassland ecosystem during winter, the drivers of N₂O could not be determined and need further investigation for two major reasons: (1) to deepen currently existing knowledge and project possible changes in flux magnitude during shorter and warmer winter periods and (2) to thoroughly constrain annual balances.”

p. 404, line 13: system à _systems

Done.

p. 404, line 14: “two of them being conducted” à _“two of them conducted”

Done.

p. 405, line 13: contradicting à _contradictory

Done.

p. 405, line 25: the Dischma valley à _Dischma Valley [** and change throughout **]

Done.

p. 406, line 20: eliminate “majorly”

Done.

p. 406, line 26: following the ... à _following the orientation of the ...

Done.

p. 406, line 21: eliminate the comma before “which”

Done.

p. 406, line 24: provide units for u^* (I presume 0.01 m/s?)

Done.

p. 408, line 12: the “dash” separating the units looks like “divided by”, perhaps use a semicolon instead? (also between CH₄ and N₂O?)

Done.

p. 409, line 1: please indent the start of a new paragraph

This is a new paragraph.

p. 409, line 9: “with 60 mL” sounds funny – rewrite two sentences; e.g., 60 mL gas samples were collected from the ski pole and immediately transferred into pre-evacuated 12 mL vials.

Done.

p. 409, line 11: few à _a few

Done.

p. 409, line 17: 1b à _1c

Done.

p. 411, line 19, 20: add “its” before “lowest”; remove “the” before giving dates (also throughout text)

Done.

p. 413, line 22: eliminate “above given” and add “tested above” after “variables”

After detailed re-analysis and daily averaging we were able to detect similar

*responses to snow water equivalent as found for the gradient measurements.
The revised manuscript was changed accordingly.*

p. 414, line 13: eliminate “actual”

This paragraph was changed in the revised manuscript.

p. 414, line 20: ecosystems à _ecosystem’s

Done.

p. 414, line 22: “respectively” is not clear – I suggest changing this to say “using the range of values derived between the EC and gradient approaches”

Done.

p. 417, line 2: than à _as

This subsection of the discussion was rewritten in the revised manuscript.

p. 417, line 8: adjective à _adjective

Done.

p. 417, line 19: seems unclear (is it a double negative?)

This was changed in the revised manuscript, also given our previous statement on nonlinearities before. “In our study, we observed only few nonlinearities, which could not be related to a specific ice layer. Therefore the entire concentration gradient was chosen for flux calculation”

p. 417, line 26: eliminate “being”

Done.

p. 417, line 29: can not à _cannot

Done.

p. 418, line 6: such approach à _such an approach

This subsection was rewritten in the revised manuscript.

p. 418, line 16: “slower” is not a verb

This subsection was rewritten in the revised manuscript.

p. 418, line 20: subj-verb disagreement in “methods underestimates”

This subsection was rewritten in the revised manuscript.

p. 419, line 21: The second à _A second

This subsection was rewritten in the revised manuscript.

p. 423, lines 4-5: in both cases I suggest using “GHG budget” and not “Budget” or “-budget”

Done.

p. 423, line 12: “this being” is unclear

Done.

p. 423, line 14: “can not” à _cannot

Done.

p. 424, line 10: there is no verb in this sentence.

Done.