

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 5:

This manuscript provides valuable information of greenhouse gas fluxes, in this case CO₂, CH₄ and N₂O, from a region and season where such estimates are few. The authors have used several different techniques to estimate these fluxes and a comparison between the methods has been conducted. The manuscript, its results as well as method comparison, is of importance for the scientific community and would after some careful editing be acceptable for publication.

General and minor comments will here be outlined according to page and line numbers:

p401- title. In the title the word “emission” is used. However, the result of this study also includes a consumption of CH₄. Consider the use of “emissions”, maybe “fluxes” would be a better word to use.

We would like to thank reviewer 5 for this constructive comment.

p402- line 13-14: Please include error estimates of the winter flux. Also the range estimate for the CO₂ (631-670 g CO₂ m⁻²) is not a range estimate. This is the result of the two methods used. Please, state the two methods separate with error estimates, or if no significant difference between the two methods can be found then give one “best guess” estimate with error estimates.

Now, we present the error separately for each method.

p405- line 18. I would suggest that you exclude “the permanent automatic monitored gradients” from your bullet point (i) and instead make a bullet point (iv) with something like “also a novel permanent automatic monitored gradient method was tested and will be described and discussed”. However, after reading the manuscript I’m not sure if it is advisable to actually include this ²²²Rn method at all. This since it does not contribute to anything in the results itself. Do not get me wrong, the idea of loosely hanging tubes instead of fixed metal once is great and so is the ²²²Rn approach. But this paper does not really gain anything by including it. Please take a careful consideration if it will be included in the future!

We adjusted our objectives in the revised version of the manuscript and shortened both, the M&M part but also the discussion section of the “novel permanent automatic monitored gradients. We still believe stating the failure of this approach is important to help other researchers in avoiding similar mistakes.

p406- line 20. Here it is stated “Two majorly different techniques”. But earlier studies have shown that also gradient and trace gas (²²²Rn in this case) are “majorly different”. Rephrase or deduct to two methods only.

The sentence was changed to: “Two different techniques – eddy covariance and gradient measurements – were used to derive independent GHG flux estimates.” estimates.”

p407- line 11. Change “(half-hourly averages)” to (30 min averages). Also the c in (c , μmol , Eq. 1) should be in *italic* as (c , μmol , Eq. 1).

Done.

p407- line 13. In Eq. 1 the term w is used. But it is not described in the text. Please do so.

Done.

p407- line 23. Here the term “SD” is used for the first time. Please explain.

Done.

p407- line 24. “ u^* ” is probably the friction velocity, please state so or include “(u^*)” behind the friction velocity in line 17 p 403 in the introduction. However, if it is included at p 403, then also the soil temperature and the snow density should be given their proper notifications in the same line.

Done.

p407- line 24-25. Please explain why such overestimation occurs with an extra line.

This was a typing error and was supposed to be “underestimation” due to non turbulent conditions and associated mixing of the air.

p408- line 2. Was the ^{222}Rn also sampled manually? Or was the ^{222}Rn only sampled at the permanent automatic monitored gradients? Please change “ CO_2 , CH_4 , N_2O and ^{222}Rn ” to “ CO_2 , CH_4 , and N_2O ” if appropriate.

Rn^{222} was sampled manually nearby to determine background signal.

p408- line 13-14. Here the slope of the linear regression is mentioned, please fill in which linear regression that is referred to and how the regression was established.

Linear regression between concentrations. And this regression was established by Microsoft Excel.

p408- line 15. Here the “diffusion coefficient of CO_2 ” is mentioned. What about CH_4 and N_2O ? Change “the diffusion coefficient of CO_2 in air (Eq. 3)” to “the diffusion coefficient in air (D_{air} , Eq. 3).

Done.

p409- line 9. I guess it should be “ with a 60ml syringe” ?

Done.

p409- line 9-10. Using pre-evacuated vials works very well in temperatures above zero. However, when temperatures drop the rubber usually get stiff and, especially, when using pre evacuated tubes a leakage of surrounding air might occur when the needle is taken out of the vial. This would dilute the sample and the subsequent flux estimates might be misleading. Was any tests done to rule out the leakage effect? I.e. syringes with standard gas could be used to test this in the field. Also, include that needle was used to transfer the sample from the syringe to the vial. Well, only if this was the case off course. Otherwise fill in how the transfer was conducted.

We added the needle information and performed a test during previous measurement campaign. Pre-evacuated vials worked reliable under different temperatures (including negative values) without a getting stiff of the rubbers. The vials were rarely exposed to very cold temperatures since these were taken to the field in a backpack and only taken out for sampling. We performed a test concerning the leak-tightness of the vials over time using standard gases. Such comparison resulted in no significant losses of the concentration in the vial over the time of almost one year (not shown). Moreover, the almost identical CO₂ concentration measured with the IRGA and in the exetainers using a GC indicate that there was no leak.

p409- line 15. If this part about the permanent automatic monitoring is kept in the text, then the paper would greatly be improved by adding an own headline for this section. Also give the method an own shorter name, this to separate the three methods properly in the method, discussion and conclusion.

The location in the text on the automatic gradients will remain similar as in the previous manuscript version. However the automatic gradients subsection has been reduced considerably.

p410- line 1. Is this the same LI-COR as mentioned in line 16 (p 409)? Maybe change the text so the instrument is only mentioned once.

This is a different Li-Cor, We used three different IRGA's during this study.

p410- line 11. The assumption about ²²²Rn as a natural tracer is a really interesting idea. This since the application of any other trace gas will include a lot of assumptions, as the lateral diffusion mentioned later on in the text. Was any summer measurements conducted as well to have some background values for comparison?

Here, we added: “To quantify the actual ²²²Rn flux, we measured the increase in ²²²Rn with time in chambers placed on the soil surface in snow pits every week.”

p410- line 18. Here the depth of 10, 30 and 50 cm is used for sampling. This would mean that the regression lines used for the flux calculation where based on 3 points. Is three points really a sufficient number to establish a regression line? If one of the three points would be slightly out of the true line, the estimated flux would be severely changed. Was any criteria used (as R² valued) for rejecting regressions

where data were scattered? Further, was only one sample taken at each depth? This is an massive and impressive approach that has been conducted, but please verify the method a bit deeper.

3 points of measurements were used for the spatial gradients only. More than 80% of the concentrations profiles surrounding the tower showed a linear relationship between concentrations (CO₂) which included 4-6 measurements each. We reduced the number of concentration measurement per profile during the intensive sampling campaign in order to cover a larger area. This resulted in 2 transects on the grassland (50 and 90m in length, Figure 7) where all GHGs were measured and an even longer transect across the valley (Figure 8) where we measured CO₂ only. At each profile one sample was taken per depth.

p412- line 9-10. Back to the same question as above: Is the snowpack at Dischma valley really that homogenous and without layers that a proper linear regression could be made at all occasion (even during the 10, 30 and 50 cm sampling)? Please include a figure to show this. Well, such a figure might be tricky but it might work if you normalize all your data. Further, consider the use of “significant” in this context. Significant here would probably mean that the regression lines are significantly separated from a zero increase. This is no doubt the case, but is the regressions representative for the true fluxes?

We thank reviewer 5 for this very important questions. And as stated in the original manuscript ice layers are a common characteristic of the snowpack in the Dischma Valley due to regular melt freeze cycles. Moreover the layers of ice within the snow are not always at the same location therefore including these in one of the figures given would be misleading. Even though with the presence of ice-layers at various depths about 80% of our concentration gradients showed a strong linear increase in concentrations towards deeper snow layers from which we conclude that our regression represents the true flux. This linear relationship between concentration and depth was found for 80% of our CO₂ profiles and for about 60% of our CH₄ and N₂O data. We agree with reviewer 5 n the difficult issue of ice layers and how these may alters GHG fluxes, however up to date we have not found a suitable solution that enables us to properly detect ice layers, define the respective thickness as well as the spatial distribution now how this is affecting the actual flux calculation. We hypothesize that such ice layers may still be a possible explanation for the large deviation between gradient derived CO₂ flux estimates and CO₂ fluxes measured by eddy covariance. The homogeneity of the snow pack is supported by the small variability of snow depth on the grassland (60-70cm; Figure 7).

p413- line 7. How did you test this influence? Did you start with testing one by one of the parameters, or did you to include all of them in your glm?

In the original manuscript we tested one by one and later included the variables in the GLM. In the revised manuscript we only correlated the fluxes with single variables providing a more meaningful information. .

p413- line 10 and 13. The SWE used here can, as mentioned, be seen as a measure of the mass of the snow pack. However, the true outcome is a height. As included in figure 5 it is given in cm in this case. Please change the denotation from SWE to

$hSWE$ throughout the text. This since you already use h as a symbol for height in you snow height (h_s).

Done.

p413- line 17. Usually fluxes can be denoted as F , so please change “CO₂ fluxes (Eq. 6)” to “CO₂ fluxes (F_{CO_2} , Eq. 6)”.

Done.

p413- line 18. Eq. 6 should then be changed to: This looks much better and is easier to follow.

Eq. 6 was adjusted to involve a single variable only. Done.

p413- line 22-24. Please rephrase the sentence starting with “Average flux...” since it is hard to understand.

This paragraph has been rephrased in the revised manuscript.

p413- line 25. Which of the gradient techniques is it referred to here? Name all three methods in a way that is easy to deviate from the other methods and use the same name throughout the text.

Gradient always refer to the manually repeated gradient measurements, since the automatic profiles were incorrect. We hope we could clarify this in the revised version of the paper.

p414- line 3-4. Change “temperature at the soil snow interface” to “ T_s-s ” and “snow water equivalent” to “ $hSWE$ ” and include which figure or table this can be referred to.

Temperature at the soil snow surface was changed to soil temperature since the sensor was located in 3cm depth and snow water equivalent was changed to $hSWE$.

p414- line 16-20. Here a error estimate of each individual flux is needed! I expect that with such error estimate you might not really be able to separate your “gradient” and “EC” methods. This might need some effort to get these error estimates but they are indeed needed for this manuscript. Also include these error estimated throughout the text.

Error estimates as SD were included in the revised manuscript.

p414- line 21. You have already defined the winter season and can simply denote it “this winter season”.

Done.

p414- line 22. Why the use if “respectively” here?

The word “respectively” was removed from the revised manuscript.

p415- line 1-7. This part might fit better in the method description.

We moved this paragraph to the results section and removed most of the redundant information through the manuscript.

p415- line 1-10. Again, this part might be removed from the MS or restructured depending on if you still feel like including the ²²²Rn gradient method.

Done.

p416- line 18-19. This sentence need to be rephrased so there is no doubt which if the methods that under estimated the flux.

Done.

p417- line 1-5. Here it would be nice with a short discussion about the natural convection in snow (work by Matthew Sturm), and the possible channeling of air flow in layered snow (e.g. Colbeck 1997), and how such things might affect the EC measurements. For example, the EC might pick up emissions from “hot spots” of CO₂ emissions, while the transect and traverse sampling might have missed such “hot spots”.

We would like to thank reviewer 5 for pointing these important studies out which we read and added to our discussion.

p417- line 12. Which method of CO₂flux is it referred to here?

This was changed in the revised manuscript. “...CO₂ flux measured by EC...”

p417- line 20. Hence the notation on regression lines based on 3 points.

The regression line was based on 3 points each for the spatial measurement only, and consisted of 4-6 measurements each (10cm increments) for the regular weekly measurements.

p418- line 5-6. Please include references for the different trace gases used. And if the purpose was to measure GHG's then CO₂ would be a rather strange trace gas to use...

Large parts of the manuscript were rephrased after including the comments of four previous reviewers who mostly stated to remove the tracer subsection from the manuscript.

p418- line 15-17. Would not the water also percolate down into the soil and by this alter the biogeochemistry in the soil?

We agree with the comment given by reviewer 5 and further state that additional water in the soil is assumed to have only little effect on the biogeochemistry of the soil since under snow with temperatures around 0°C in soils which are probably close to water saturation. We can only hypothesize such statement and refer to our volumetric

water content measurements in 10cm depth showing only very little variation.

p420- line 8-9. There and increase in CO₂ is mentioned and relater to a constant soil water content. Is this what is meant, or did the soil water content in Liptzin et al 2009 change over time?

In Liptzin et al. 2009, soil water content remained most of the winter constant and only increased towards the end of the season similar to the pattern shown in Figure 2c for our site.

p420- line 21. Insert “negative” in front of “correlation” to make the direction clear.

We disagree with reviewer 5, since the correlation stated in the original manuscript was positive. However, the temperature response was removed in the revised version of the manuscript due to reasons as stated before.

p421- line 10-13. Sentence starting with “Still, the reported...”. Here it is hard to follow which uptake rates that is lower in respect to what. Please rephrase this sentence to make it easier to follow.

Done.

p422- line 4. Include error estimates for the flux.

Done. (see comments to reviewer 4)

p422- line 20-22. The paper by Mohn et al., 2013 is referred to here. But what did this paper say, it is mentioned that isotopes where used, but what was the emissions rates found? Please include these numbers to meth the sentence and reference complete.

The paper of Mohn et al., identifies the potential mechanisms of N₂O fluxes but not the magnitude. In the revision, we report their findings: “In mid Februrary, high-precision N₂O isotopomer analysis using laser spectroscopy indicated that the main N₂O source processes were heterotrophic denitrification and nitrifier denitrification (Mohn et al., under revision).” p422- line 23. Name the two different methods that is referred to here.

Done.

p422- line 24-25. Include error estimates!!!

Done.

p422- line 28-29. Which period is it referred to here? In the sentence above you have three “periods” mentioned: peak winter, the beginning and the end.

In the original manuscript we refereed to the beginning and end of the winter period, respectively. However we changed large parts of the discussion and showing modeled as well as measured data for both applied methods (gradient and EC) in Figures 3 and 4.

p435- Table 2. Is the error estimate here standard error or standard deviation?

The errors indicate the standard deviation. We added this information in the table caption.

p436- Table 3. Please include error estimates for all the numbers given!

Done.

p437- figure 1. Change “meteo tower” to “meteorological tower” and change “profile sampling unit” to “profile sampling units”.

Done.

p438- figure 2. Change “Meteorological conditions” to “Environmental conditions” since soil temperatures and soil moisture is not really meteorological data... Furthermore, for Fig. 2, 3 and 4 change so the same labeling occurs on the x-axis. As it is now you have month in some figures and Julian days in others. This is confusing and will be better if you chose only one labeling method. Also, the error bars here is standard deviations? Please include in text.

Done.

p439- figure 3. Include if it is standard error/deviation or other error estimates used in the figures.

Done.

p441- figure 5. Change “meteorological variables” to “environmental variables”.

Done.

Thanks for a nice manuscript and good luck with the corrections!