

Discussion of “Forest-floor greenhouse gas fluxes in a subalpine spruce forest: Continuous multi-year measurements, drivers, and budgets”

Author Response to Referee 2 comments

Krebs et al.

November 30, 2023

In the following, *reviewer comments are given in italics*, author comments are given in normal font.

1. General comments

This ms reports high-resolution GHG fluxes from a forest floor in a subalpine coniferous forest using four automated chambers. Such automatic measuring systems are of great scientific interest, because events that occur for a short time can be recorded with them. The GHG measurements are integrated in a network for long-term observations of ecosystem fluxes.

Thanks for the comment.

Three objectives were defined, but no hypotheses or research questions.

This is correct. In the revised manuscript, we will add the following hypotheses:

“We hypothesize that the forest-floor is a source of CO₂ throughout the years, with large seasonal variability due to the temperature sensitivity of respiratory processes, but with very low N₂O emissions due to the overall low N supply at the site. In contrast, we expect that the forest-floor is a net sink of CH₄, with soil temperature and snow dynamics being important drivers due to their impact on microbial activity and diffusion rates between soil and atmosphere. Thus, we expect the highest respiratory CO₂ emissions and highest CH₄ uptake in exceptionally warm years, such as in 2022 at our site. Overall, we anticipate the GHG budget being mainly determined by CO₂ fluxes, with CH₄ uptake only slightly offsetting the respiratory CO₂ losses, and very low N₂O emissions.”

The measurement technique of CO₂ and CH₄ fluxes seems very robust, while the measurement technique for N₂O fluxes is obviously critical. Many N₂O measurements indicate negative values, a net N₂O uptake by the forest floor. Few net N₂O uptakes have been reported, but mostly in dry soils during the summer months. I can only speculate that the measurement duration of 180 seconds is too short for the large chamber volume (281 L) or for the height of the chambers (50 cm) at low N₂O fluxes. Own measurements in a spruce forest with a different laser technique and a different chamber system showed that the measurement time often required more than 20 min before a significant increase of the N₂O concentration could be determined. In this respect, I propose to remove the N₂O measurements completely from the manuscript and focus on CO₂ and CH₄ fluxes.

The suggestion to remove the N₂O fluxes from the manuscript seems to be based on two reasons:

- 1) measurement duration of 180 sec is too short for the large chamber volume, and
- 2) unlikely that forest floor shows uptake of N₂O.

In the following, we want to address those two points.

1) In order to check for the validity of our chamber measurements, we have performed measurements using static chambers with the dimensions of d = 30 cm and h = 30 cm (Hutchinson and Mosier, 1981). We used eight static chambers, i.e., four chambers next to the automatic chambers, and four chambers placed randomly within the research area. Soil collars were installed two weeks prior to the first measurement campaign. Four rounds of sampling were done on two measurement days in October 2023 (n=32), when soil temperatures were between 5.5-10 °C, well above the long-term mean, and soil moisture values above 8%, favoring microbial activities. Three collars were irrigated between the first and second sampling round on the two days to simulate a heavy rainfall event, favoring denitrification. We left the chambers closed for 1 h and sampled the air in the headspace every 20 min. The fluxes that we measured with the eight static chambers were low (mean±SD = 2.9±31.1 nmol m⁻² h⁻¹). Furthermore, they agreed very well with the fluxes which we have measured using the automatic chambers over two years (mean±SD = 0.63±58.6 nmol m⁻² h⁻¹), and in October (mean±SD = 10.2±14.7 nmol m⁻² h⁻¹). We are thus confident that the low fluxes measured using the automatic chambers are real and that an insufficient closure time is not the reason for the low fluxes.

2) The N₂O fluxes measured with the static chambers mentioned above showed occasional N₂O uptake as did the automatic chamber measurements. We would like to point out that the uptake rates we have measured are very low and probably not significantly different from zero. However, microbial processes in forest soils can contribute to both uptake and release of N₂O, depending on the prevailing environmental conditions such as oxygen availability, soil moisture and microbial communities. Under aerobic conditions, denitrification contributes to N₂O release, while under anaerobic conditions, N₂O reduction to N₂ can dominate over N₂O production, which results in observations of net N₂O uptake by soils (Wen et al., 2017). Moreover, N₂O uptake has been observed in a German spruce forest (Goldberg and Gebauer, 2009). Therefore, we think occasional N₂O uptake as measured with our chambers are real.

Due to the scarcity of long-term and high-resolution N₂O fluxes from forest ecosystems, we think that our dataset is very valuable and would therefore like to keep the N₂O fluxes in the manuscript. Instead of showing the N₂O fluxes only in the appendix, we would like to move them to the main text (including the data from the static chamber measurements) and discuss them in the discussion part. Please see our response to your comment on the discussion part (section 1.12 Discussion) and our response to the comments on N₂O fluxes of Referee 1.

Another problem with respect to the calculation of the GHG budget is the contribution of ground vegetation to CO₂ fluxes. Due to the opaque chambers, only the respiration of the vegetation is measured, as it naturally occurs only at night. Thus, CO₂ fluxes were overestimated during daylight hours. For a correct GHG budget, however, the CO₂ fixation of plants would also have to be recorded. An estimation of the contribution of aboveground plant organs to the CO₂ flux would be interesting. Calculating the GHG budget for the forest does not seem justified to me.

We agree that our budgets do not include CO₂ uptake from the understory plants during daytime and thus talking about a full forest-floor CO₂ budget is misleading. Thus, we adjusted our terminology and now talk about a forest-floor “respiration” budget when talking about CO₂ throughout the manuscript.

Overall, a thorough revision of the manuscript is needed. As is usual in scientific papers, clearly formulated research questions or hypotheses, e.g. on the effect of the snowpack, would improve the quality of the ms.

Thanks for your suggestions to improve the manuscript. On the hypotheses, see above. We hope we have addressed your overall concerns.

2. Specific comments

1.1 Title

Please change the title if N₂O fluxes are omitted. 'multi-year' is a bit exaggerated when the fluxes were only measured for 3-4 years.

We rephrased the title to: "Forest-floor respiration, N₂O and CH₄ fluxes in a subalpine spruce forest: Drivers and annual budgets".

1.2 Line 16

Please present only means of the annual fluxes.

We will change this, thank you.

1.3 Line 19

Provide here the mean CH₄ flux, not the CO₂ equivalent

We will change this, thank you.

1.4 Line 19-20

'driven mainly by snow depth' – do you mean that increasing snow depth reduced CH₄ uptake? Is the relation between CH₄ flux and snow depth significant?

Our random forest (RF) driver analysis showed that snow depth had the highest importance in the RF model to predict CH₄ fluxes. We will add a plot showing the curvilinear relationship between CH₄ fluxes and snow depth in the revised manuscript. Please see the suggested figure (Fig. 3) and our response to comment about line 271 (page 7 of this response; in short: 2x "yes").

1.5 Line 27-28

'with negative effects on its carbon sink behavior' the data don't show this, please omit the statement.

We would like to keep this statement in the manuscript due to the following reasons: i) We have shown that the forest-floor respiration budget was highest in the warm year of 2022. In the future, the forest site is projected to experience more years similar to 2022 (IPCC, 2021; CH2018, 2018). Thus, high respiratory losses from the forest floor will decrease the forest C sink. ii) Furthermore, studies show that the length of snow-covered periods will decrease in the Swiss Alps. This will also increase respiration fluxes and also contribute to decreasing C sinks of the forest (Klein et al., 2016; CH2018, 2018).

1.6 Line 54-58

Experimental soil warming was not investigated in this study, but annual variation of gas fluxes. A more general view at temperature influence would better fit this study.

We agree that soil warming experiments are not relevant to the current study. We will adjust this part of the introduction.

1.7 Line 92 (Table 1)

Provide some data of the forest floor and mineral soil: horizons, thickness, texture, stocks. Does bulk density (5 cm) refer to the mineral soil or forest floor? (see comment below)

The bulk density at 5 cm refers to the upper 5 cm of the mineral soil, which is high in organic matter. The stocks have already been reported in Table 1 of the original manuscript. The horizons, thicknesses and textures were measured for two soil profiles within the study area, a chromic cambisol and a rustic podzol. We will report information on horizons, thicknesses, and textures from these two profiles in the revised manuscript. Furthermore, in the meantime additional soil data from the ICOS ETC became available, which will also be shown.

1.8 Line 113

180 s measuring time - why where chambers closed for 10 min? When where concentrations measured during the 10 min? Please provide the length of the tubing between chamber and detector and the flow rate or pump rate.

Measurement cycle: This is a misunderstanding. The complete chamber measurement cycle is 10 min, and this includes the time for closing and opening the chamber (the chamber moves very slowly, so it takes around 3.5 min to close and around 3.5 min to open the chamber). The time in which the chambers were actually closed was 3 min. During the entire chamber cycle, the concentrations were measured continuously once per second. We will rephrase the text so that it becomes clearer.

The flow rate ranged between 0.9-1.0 slpm. The tube lengths between chamber and instruments ranged between 49-85 m. We determined the time lags in the arrival of the gas in the instrument based on the change in chamber status (fully open, fully closed) and max. CO₂ concentrations measured.

1.9 Line119

Were the chambers closed 16 times per day = 160 min or 11% of daytime? Does this mean that 11% of annual precipitation was also excluded and the forest floor was drier than outside the chambers?

We are aware that by using any chamber method, we are potentially altering environmental conditions. This is unavoidable for all chamber studies. However, with our chamber design and closure duration, such potential effects could be avoided as much as possible, since the chambers were white (high albedo), very large (reducing edge effects), and in the open position, they moved far away from the soil collar (avoiding shading; Fig. 1). See also our response on soil temperatures to referee 1.

We will include a picture of one of the chambers in the appendix (Fig. 1) to show how the chamber moves and that about 7 minutes of the 10 minute cycle were used to move the chamber down onto the frame. Thus, the chambers were actually only fully closed for 3 minutes per chamber cycle = 48 minutes or 3.3% of the day, and not 160 minutes per day. If we add the time spent opening and closing the chamber as it hovers over the frame (4 minutes per cycle), we estimate – very conservatively – that the chamber is closed for a maximum of 7 minutes per chamber cycle = 7.8% of the day. However, rain does not usually fall perpendicular to the floor, but at an angle, i.e., during these 4 minutes, rain will still fall inside the frame. We think that our conservative estimate of 7 minutes is thus more realistic than the 10 minutes assumed by referee 2. We will add this info into the Materials and Methods section in the revised manuscript.

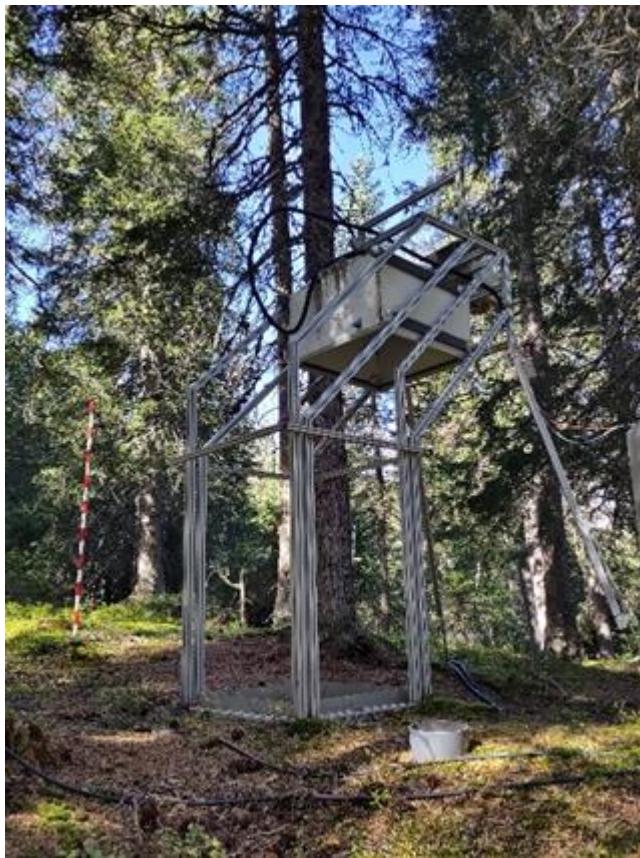


Fig. 1: Picture of chamber 3.

Moreover, we think it is too simplistic to say that we exclude 11% or (see above) 7.8% of the precipitation, because the chambers were closed for 11 or 7.8% of the day. Rainfall is not evenly distributed throughout the day. Moreover, in a spruce forest, throughfall is typically less than bulk

precipitation above the canopy due to interception and is very heterogeneous within a forest (Schulze et al., 2019). These factors challenge the statement that we exclude a certain percentage of bulk precipitation because we close the chambers for this percentage of the day.

Furthermore, we have the chance to test for soil moisture bias due to the chambers because for our chambers 1 and 2, we do have soil water content (SWC) measurements from inside and outside the chambers available for four years (Fig. 2). SWC was highly variable over time as well as in space. SWC differences between inside and outside varied between plus 10% and minus 10% during the four years. No clear trend was detectable over time. The average difference between inside and outside SWC over the four years was $-2.9 \pm 5.8\%$. During most of the year, no significant difference in SWC inside vs. outside the chamber was detected, although we found on average 5% lower SWC values inside the chamber during winter (Fig. 2b). Based on the rather large uncertainties in absolute measurements of SWC (see answer to comment on Line 155), we believe that a difference of 5% is minor. Moreover, we found a high agreement in the dynamics of SWC inside and outside the chambers 1 and 2 when applying a Pearson correlation of the SWC inside and outside the chambers (R^2 values of 0.69 and 0.82). We will add this information to the revised manuscript.

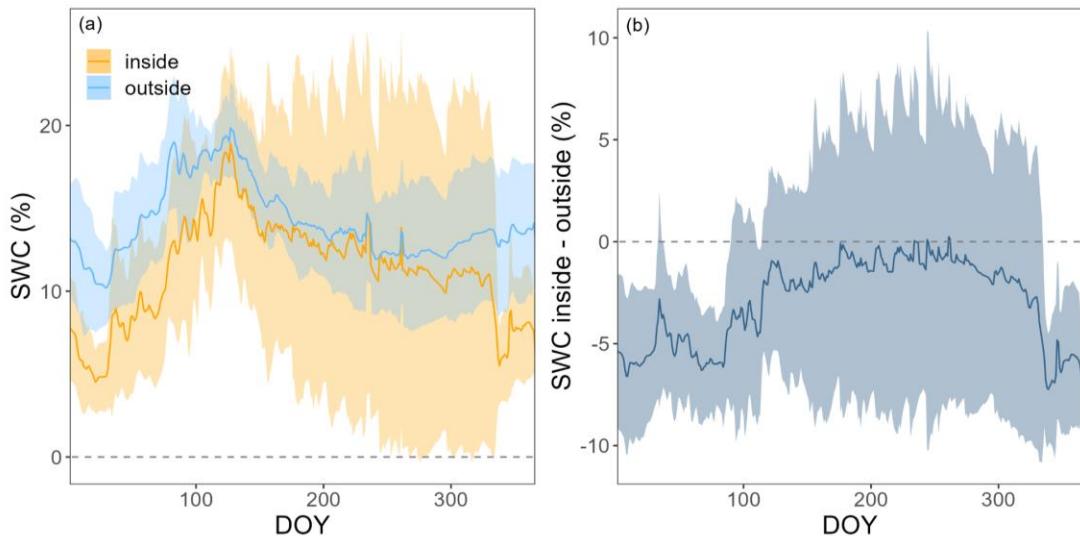


Fig. 2: a) Soil water content (SWC) at 5 cm inside (orange) and outside (lightblue) the chambers over the course of a year. b) Difference in SWC at 5 cm between inside and outside the chambers over the course of a year. Lines show means, bands show standard deviations over all years and chambers 1 and 2.

1.10 Line 155

The installation depth was 5 cm for the SWC sensors. The low bulk density indicates that the sensors were installed in the organic horizon or in the transition from the organic horizon to the mineral A horizon. This is critical because the EC-5 sensors have only a standard calibration, which is often not suitable for many forest soil horizons with high root density or stone fraction. Where the sensors calibrated with the soil from 5 cm depth? While the sensors show nicely the dynamics of the water content, the absolute value is often incorrect. When bulk density changes due to shrinkage and swelling of the forest floor, further uncertainty is added to WFPS. Overall, the WFPS is very low (Fig. 1b), especially after snowmelt where much higher values should be reached.

We fully agree that reliable absolute measurements of SWC are difficult to obtain. Especially at the Davos site where the soil is very heterogeneous, and the upper horizons are full of roots and rocks which makes reliable calibration impossible. Since we were aware of these aspects, we used centered and scaled WFPS values for our data analyses as described in the original manuscript. With this approach, we take the correct temporal dynamics into account but avoid relying on potentially incorrect absolute values.

1.11 Line 271

This result could be better presented, perhaps by linear/non-linear relationship (decrease in CH4 uptake/cm snow depth)

Thanks for this suggestion. We will add a plot showing the relationship between CH₄ uptake and snow depth in the revised manuscript (Fig. 3). Furthermore, we will add that the snow depth and the GHG fluxes are highly correlated in the months Oct-May, as spearman correlations coefficients are 0.59 and -0.79 for CH₄ fluxes and forest-floor respiration, respectively.

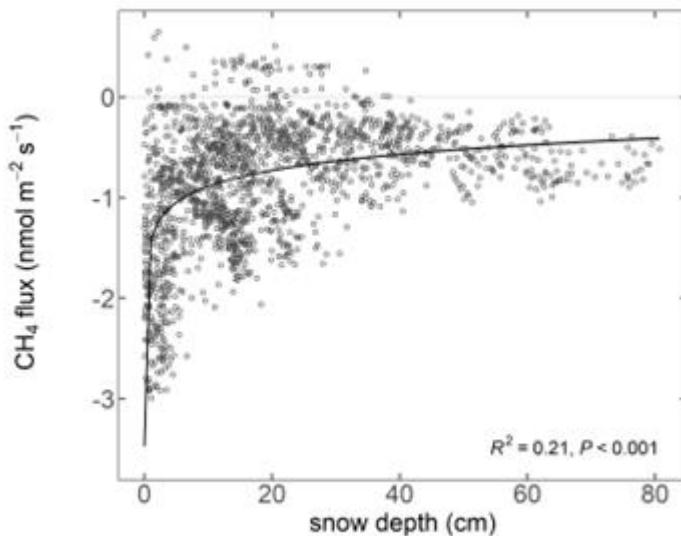


Fig. 3: Relationship between forest-floor CH₄ fluxes (nmol m⁻² s⁻¹) and snow depth (cm). Black line shows fitted logarithmic curve.

1.12 Discussion

N2O fluxes are not discussed at all.

It is true that we did not discuss the N₂O fluxes in the manuscript. We decided to not use them for driver analysis and budget calculations, two main objectives of the manuscript. This decision was made mainly because of the low magnitude of the fluxes and their irrelevance for the forest-floor GHG budget (using the mean N₂O flux measured with the automatic chambers over the two years, 0.63 nmol m⁻² h⁻¹, we arrive at an annual budget of 0.066 g CO₂-eq m⁻² yr⁻¹ which represents 0.003% of the annual forest-floor GHG budget). However, we still think that it is important to show the N₂O fluxes in the manuscript

because such measurements in forests are very scarce. So, instead of removing N₂O from the manuscript completely, we would like to move the N₂O figure (Fig. A.1 in the submitted manuscript) to the main text and adding a panel (d) showing the fluxes from the static chamber measurements (Fig. 4). This allows us to discuss the N₂O fluxes in the paper and show that the magnitude of the fluxes is indeed very low.

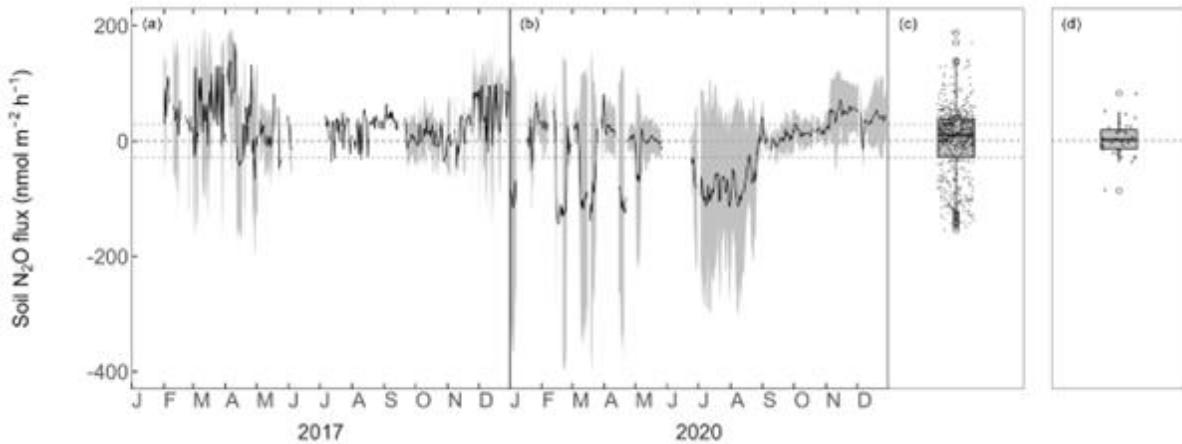


Fig. 4: Forest-floor N₂O fluxes (nmol m⁻² h⁻¹) for the years 2017 (a) and 2020 (b). Black lines show means over four chambers, grey bands show standard deviations among four chambers. Boxplot showing distribution of means over four automatic chambers (c) and N₂O fluxes from static chamber measurements (d). The dotted lines depict the minimum flux which could be detected by the Dual Quantum Cascade Laser spectrometer.

1.13 Line 370

How many ‘hot moments’ were identified in this study. One message of this study could be that the effort with automatic measurement systems for these forest types is very large and weekly or bi-weekly measurements with many chambers yield more robust flux rates on a larger spatial scale.

The question about “hot moments” is difficult to answer since we focused in our manuscript mainly on daily and annual fluxes, not necessarily on hot moments, even though we described and discussed them in the original manuscript.

Nevertheless, we do not think that automatic measurements always need more effort than manual bi/weekly measurements, which need more person-power than automatic chambers, particularly when visiting remote sites bi/weekly. The approach clearly depends on the research questions asked. However, hot moments can only be identified when high-temporal resolution measurements are available, which are very difficult to obtain in high enough temporal resolution with manual measurements. Those are typically taken during daytime and good weather conditions, rarely 24/7/365 as automatic measurements. We agree that manual measurements can represent spatial variability better than automatic measurements, which need mains power if run at high temporal resolution. But then, “hot spots”, not hot moments would be the research question asked.

1.14 Table 1

Temperature, WFPS and snow cover are presented in Fig. 1. If needed, annual means can be described in the text.

Thank you for the comment. However, Fig. 1 shows aggregated data for the entire research area while Tab. 1 gives data separated for the different chambers which we treated as replicates and used for the driver analysis. Thus, we suggest moving Table 1 to the Appendix instead of deleting it.

1.15 Table 4

Were the fluxes in these studies measured exclusively from forest floors where vegetation had not been removed? If present, the above-ground soil vegetation is very often removed by clipping to measure soil respiration. There are many more long-term studies where GHG fluxes were published in different papers from the same forest site. A table without CO₂ and CH₄ flow rates is redundant anyway.

Thank you for the comment. We agree that there are many more studies measuring one of the three GHG, but we only selected those in which all three greenhouse gases were measured at the same time. We reported studies irrespective of whether vegetation was removed or not. We will include the magnitude of CO₂, CH₄ and N₂O fluxes (including the fluxes from our study) as well as information about vegetation removal in the table in the revised manuscript. We will also highlight in the text that soil respiration and forest-floor respiration are not equal and cite relevant references, such as Barba et al. (2018). However, we would like to stick to our approach and focus on studies which show all three GHG fluxes measured at the same time (and thus been published in the same paper), to be able to compare our study and approach with their measurement method and frequency.

3. References

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