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

Author Response to Referee 1 comments

Krebs et al.

November 30, 2023

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

1. General comments

Luana Krebs and co-authors present an impressive soil GHG flux dataset from a subalpine forest. Multi-year datasets from such ecosystems are rather scarce and therefore definitely deserve publication. CO2, CH4 (and N2O) were measured in high temporal resolution year-round during 4 years and annual CO2equ budgets were calculated showing higher net emission during the warmest year 2022. I have some suggestions to improve the manuscript.

Thanks for the comment and your suggestions.

It should be considered removing N2O from the manuscript. Obviously N2O data is available only for 2 years and the extremely low fluxes are only shown in the supplements and not included in the budgets or any other calculations. It appears that the 180 sec chamber closure likely were not long enough for serious N2O flux calculations (extremely low 10th percentile R2 in Table.2) – as mentioned in the manuscript, using such a poor fit to calculate fluxes should be avoided. I don't know the actual cause. It is unlikely that the subalpine soil does not show any, or not measurable N2O emissions throughout the whole year. It seems more likely that there appeared problems with the laser. I don't operate one by myself but heard from colleagues that the real measurement accuracy in the field is not really the 0.06 ppb for N2O that is suggested. There might be drift or whatever else. How and how often was the laser calibrated? If you do not have 100% trust in the N2O data, I would take them completely out of the manuscript. Currently you state that there was no soil N2O efflux at this site – are you 100% sure?

The suggestion to remove the N_2O fluxes from the manuscript seems to be based on five reasons:
1) data available only for 2 years,
2) very low N_2O fluxes,
3) 180 sec closure for flux measurements and low R^2 values,
4) unlikely that alpine soils show such small fluxes, and
5) problems with the laser (accuracy, drift).
In the following we want to address those five points.
1) Long-term N_2O fluxes in forests are very rare, due to the difficulties measuring year-round. Thus, two years of N_2O flux data are actually extraordinary, and there is no reason to delete such rare fluxes.

2) We agree that the N_2O fluxes were low, and we did not use them for further driver analysis, one of the 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 N_2O 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 CO_2-eq m^{-2} yr^{-1} which represents 0.003% of the annual forest-floor GHG budget). However, we think that the result of the irrelevance of N_2O fluxes for the annual forest-floor GHG budget of the forest does not diminish the importance of these fluxes.

3) 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.

The low R^2 values can be due to different reasons, high variability during closure time or small fluxes (see point 4). We would like to point out that the static chamber fluxes show rather low R^2 values too. However, we found a clear positive relationship between the R^2 value and the magnitude of the flux for both measurement techniques. Therefore, we suggest looking at a different criterion to assess the N_2O flux quality, such as the mean square error (RMSE) which is not dependent on the flux magnitude itself.

4) At the site, N supply to plants and microorganisms is limited, so it is to be expected that N_2O fluxes are low as well. Foliage N concentrations indicate N limitation for spruce (foliar N concentration are about 1% in 0- and 1-yr-old needles as opposed to the optimum range of N content in needles between 1.5 and 2.3 %; Thimonier et al., 2010; Ingestad, 1959), N concentrations in the soil are low (1.4% in the organic layer and 0.4% in 10-20 cm depth, Jörg, 2008), N deposition is low (about 10 kg N ha^{-1} yr^{-1}; Thimonier et al., 2019; Gharun et al., 2021). Thus, our site is rather low in N which could be used for microbial transformations, competing with plant uptake (Schulze et al. 2019).

Moreover, we measured N_2O fluxes using eddy covariance above the forest in 2016 and 2017 where we found that the forest emitted 0.047 g N_2O m^{-2} yr^{-1} which corresponds to 0.122 µmol N_2O m^{-2} h^{-1}. These low fluxes further back up the notion that the soil N_2O fluxes are very low at the site. Furthermore, a study conducted a boreal spruce forest with low nitrogen deposition rates (about 5 kg N ha^{-1} yr^{-1}), reported very low mean N_2O fluxes of around 0.02 µmol N_2O m^{-2} h^{-1} agreeing well with our results (Rütting et al., 2021).

5) The flux detection limit has been assessed based on the precision of the laser and the closure time. Our flux detection limit is a lower bound, i.e., under ideal conditions, we could detect fluxes as low as 29 nmol N_2O m^{-2} h^{-1}. The fluxes within the range of ±29 nmol N_2O m^{-2} h^{-1} are therefore not significantly different from zero. It is correct though, that the real measurement precision is likely to be higher than 0.03 ppb. This basically just means that our flux detection limit would be higher. A study by Nemitz et al. (2018) reports the precision of the instrument as 0.09 ppb. This would give a flux detection limit of 87.2 nmol N_2O m^{-2} h^{-1}, which is still a very low flux. The flux detection limit could be reduced with longer chamber closure time. According to this logic, we can say that the higher the measured flux, the

more confidence we have in the measurements. Overall, we can say that we are confident that our N_2O measurements are very low and not relevant for the GHG budget of the forest floor.

Based on the explanations given above, we would like to keep and discuss the N_2O fluxes in the manuscript. Long-term and high-resolution N_2O chamber measurements in forest ecosystems are very rare which is why we think showing this dataset is very important for the scientific discourse. We would like to redo the quality assessment of the N_2O fluxes using the RMSE instead of the R^2 which is dependent on the slope of the linear regression. Generally, we suggest moving the N_2O figure (Fig. A.1) from the appendix to the main text, add a panel (d) showing the boxplot from the static chamber measurements (Fig. 1), and discuss these measurements more in detail later on.

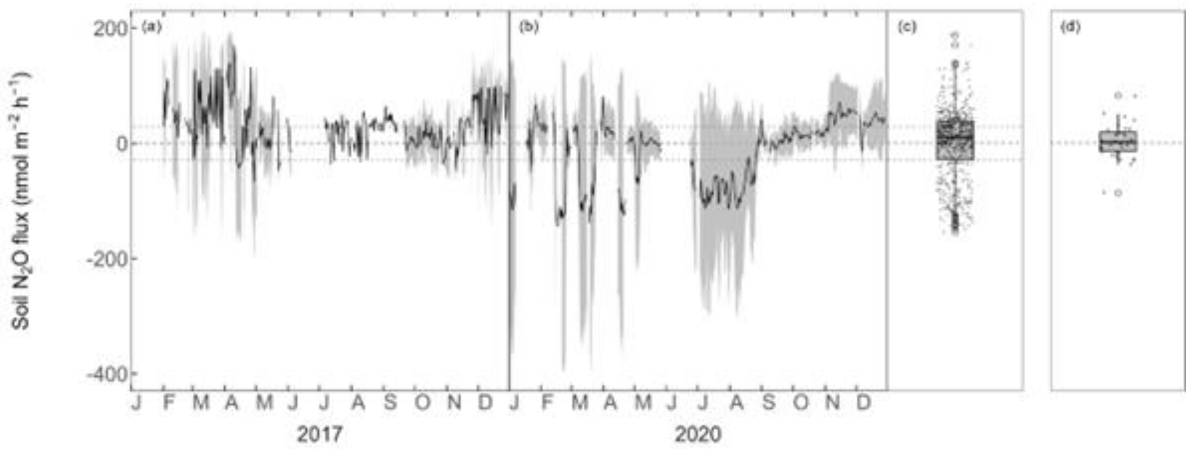

Fig. 1: Forest-floor N_2O fluxes (nmol m^{-2} h^{-1}) for the years 2017 (a) and 2020 (b). Black lines show means over four chambers, grey bands show standard deviations among four chambers. Boxplot showing distribution of means over four automatic chambers (c) and N_2O fluxes from static chamber measurements (d). The dotted lines depict the minimum flux which could be detected by the Dual Quantum Cascade Laser spectrometer.

Some information should be added to the method section about how the snow in the chamber was treated in flux calculations. The volume of the water(ice) of the snow cover must be subtracted from the camber volume during snow cover. How was that done? Was snow porosity measured or assumed somehow? If the snow volume was not subtracted, it is no wonder that CO2 emissions became the lower the more snow was in the chamber. Or was only the volume above the snow surface used for calculation? (In this case it would be flux from the snow surface, not the forest floor). Just of interest, what had happened in spring? Typically opaque chambers warm up faster and snow melts much earlier in and around them.

Thanks for the comment. Indeed, we did not provide enough details on how we treated the presence of snow in the chamber during flux calculations. As the reviewer rightly guessed, we subtracted the snow volume from the chamber volume during snow covered periods. We also accounted for the varying chamber volume due to the chamber frames we had installed during winter. Thus, the original flux calculations were correct. We will add the following formula to the manuscript, which describes the volume calculation for the flux calculations:

V = 0.75 * 0.75 * (0.5 + frame number * 0.5 – snow depth)

Where the chamber area was 0.75 m * 0.75 m. The height of the chamber was 0.5 m. However, the height of the volume depended on the number of additional frames added to allow flux measurements during snow covered periods as well as the snow depth inside the chamber. The additional frames were made of the same white PVC as the chambers themselves with a height of 0.5 m. Up to two additional frames were added.

With our chamber design we are confident to have avoided potential side effects on the environmental conditions as much as possible. 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). We do not have data on the timing of snow melt inside of the chambers. However, we observed that snow melted rather later inside the chamber compared to outside. To assess potential chamber effects on environmental conditions, we measured soil temperature inside and outside of each of the four chambers. We did not find any significant differences inside vs. outside the chambers (Fig. 2a). In winter (Dec-Mar), the temperatures inside the chambers (orange) were only 0.1-0.5 ºC lower than outside the chambers (blue, depending on the chamber). However, the differences were only significantly different from zero in December and February-March (Fig. 2b). In these months, soil temperatures are around 0 °C. When looking at the temperature response curve of forest-floor respiration (Fig. 2c), we can say that at such low soil temperatures, a difference of 0.5 °C in soil temperature has a minimal effect on the magnitude of forest-floor respiration. Therefore, we think that the effect of our chambers on the forest-floor respiration is negligible. We will add this info in the appendix of the revised manuscript.

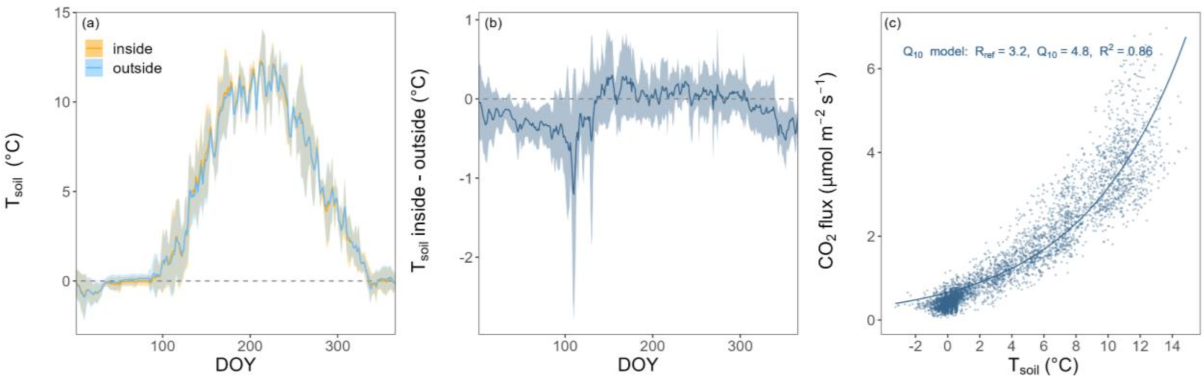

Fig. 2: a) Soil temperatures (T_{soil}) at 5 cm inside (orange) and outside (lightblue) the chambers over the course of a year. b) Difference in T_{soil} at 5 cm between inside and outside the chambers over the course of a year. Lines show means, bands show standard deviations over all chambers and three years (2017, 2020 and 2021). c) Q_{10} model showing the relationship of daily means of T_{soil} at 5 cm and forest-floor respiration of all chambers and years.

It is often stated that high temporal resolution measurements have the advantage to capture hot-moments in GHG fluxes. Well – have such hot moments been observed? Currently it does not really seem so. Did freeze-thaw periods occur? What happened during these periods with CH4 fluxes? It would be important to zoom out some such hot moments from the long-term datasets and show them separately. Even if there was freeze-thaw and no peak in CH4 flux occurred – this could be shown. Were there any CH4 emissions at all, at least short-term? We for instance once observed a small CH4 and huge N2O peak during freeze-thaw in a deciduous forest (Schindlbacher Biogeochemistry 2022) *but very similar CH4 pattern with just lower uptake during winter as in your study (Heinzle AgrForMet 2023) in a spruce mountain forest. Another possibility for hot moments is after rain post drought periods. Did you observe any flux peaks of any GHG during such periods? Seems there were some very short term peaks and some longer-term positive CH4 fluxes in the "observed fluxes" in Fig. A.3.*

Thanks for the suggestion to look deeper into hot moments. We actually have already described bursts of CH_4 emissions coinciding with snow fall and snow melt in section 3.1 of the original manuscript. Furthermore, we showed daily flux data in Fig. 1 of the originally submitted manuscript where such emission periods can be observed. Since the manuscript is already quite dense, we had planned to write a separate paper concerning short-term variations in CH_4 as well as CO_2 fluxes, using the full resolution of the data (3-hourly) and showing the data per chamber separately. In the next manuscript, we would like to not only investigate the short-term effects of snowfall, snow melt and freeze-thaw events, but also effects of drought and precipitation (e.g., rewetting events) on the fluxes. Here, we focused on daily flux data as well as long-term means over all chambers to represent the entire forest. We will rephrase the sections in the manuscript where we talk about hot moments.

There appeared very high CO2 fluxes during a period in summer 2022 – any idea why? If possible the advantages of the high resolution GHG dataset should be worked out – but probably the advantage is not as great as can be seen from the fact that the simple Q_{10} driven model produced more or less the same CO2 budgets as the random forest model with a lot more input parameters than soil temperature.

Thank you for the comment about the very high CO_2 fluxes in 2022. As shown in the manuscript, summer 2022 temperatures were very high at the site; summer 2022 was the warmest summer since we started our measurements at the site in 1997. It is well known that temperature is a major driver for any respiratory process (Davidson et al., 2006; Amthor, 2000). Also our Random Forest (RF) driver analysis revealed that soil temperature was the main driver for forest floor CO_2 fluxes. Furthermore, a study by Anjileli et al. (2021) has shown that heat extremes can increase the soil respiration by 25%. Therefore, it is not surprising that we measured very high forest floor CO_2 fluxes during summer 2022.

Since we found that the CO_2 fluxes at our site are mainly driven by soil temperature, it is not surprising that the Q_{10} model worked reasonably well ($R^2 = 0.86$) and generally captured the temporal dynamics of respiration at our site. However, we would like to point out that this is not self-evident. Many studies have shown that Q_{10} models do not reproduce measured fluxes well when additional drivers impact the fluxes, for instance when soil moisture, frost, or carbohydrate limitations come into play (e.g., Rühr and Eugster, 2009; Reichstein et al., 2013; Mitra et al., 2019).

In our study, the very high fluxes in summer 2022 were not accurately reproduced by the Q_{10} model, while the RF model estimated them well. Therefore, we argue that Q_{10} models are not able to capture extreme fluxes which might be caused by more drivers than temperature alone. In contrast, our high-resolution dataset coupled with machine learning offered a more comprehensive model, which included multiple environmental variables and at the same time was able to consider chamber specific characteristics, and thus was able to capture the extreme fluxes. Thus, we think that the reliability of the RF budget is higher compared to the Q_{10} budget. In order to make this point clearer, we will also discuss this in more detail in the revised manuscript.

Moreover, unlike CO_2 fluxes, CH_4 fluxes cannot be effectively modelled using a simple and reliable approach like the Q_{10} model. Therefore, we favor high-resolution data and machine learning approaches to gain insights into the underlying drivers and obtain reliable GHG budget estimates.

2. Line Comments

1.1 Title

“Continous” is a bit confusing since there were no measurements done in 2018 and 2019

We rephrased the title to: “Forest-floor respiration, N_2O and CH_4 fluxes in a subalpine spruce forest: Drivers and annual budgets”.

1.2 Intro: P2 50-60:

I would rather not discuss soil warming experiments here. The current study is no soil warming experiment and has no connection. You might better refer to generally increasing soil temperatures (Lembrechts, J. J., (2022) Global Change Biology, 28(9), 3110-3144.) and to the global trend of soil respiration under warming (Jian, Jinshi, et al. "A restructured and updated global soil respiration database (SRDB-V5)." Earth System Science Data 13.2 (2021): 255-267.; Bond-Lamberty, Ben, and Allison Thomson. "Temperature-associated increases in the global soil respiration record." Nature 464.7288 (2010): 579-582.).

We agree that soil warming experiments are not relevant to the current study. We will change this part of the introduction and incorporate the mentioned references in the introduction.

1.3 Methods:

Calibration of laser?

The instrument has not been calibrated during the measurement campaign. However, to ensure fit quality, the laser temperatures and tuning rates were adjusted on a regular basis. After the measurement campaign in 2017, the instrument was sent to Aerodyne laser instrument for repair and maintenance; the laser measuring N_2O was replaced in 2019. We did not calibrate the instrument since we were not interested in absolute concentrations. Furthermore, comparable instruments used for N_2O measurements with eddy covariance are commonly not calibrated on a regular basis either. In a study by Rannik et al. (2015) comparing the available equipment for N_2O flux measurements employing the EC technique and evaluating their performance, ability to detect small fluxes, and assessing long-term stability in determining the N_2O exchange with a comparable instrument (CW-TILDAS-CS, Aerodyne), no calibration was performed during the campaign.

1.4 Methods: Tab1:

For snow depth usually rather the maximal depth is provided than the mean depth

Thank you for the comment, we will add the maximum depth to the table.

1.5 Methods: Line 140

It is mentioned that 2% of the data were discarded after step 1 but not how many data were discarded after step 2 and 3. Please add.

Step 2 removed 0.2% and 0.7% of CO_2 and CH_4 fluxes, respectively. Step 3 excluded 6% and 9% of CO_2 and CH_4 fluxes, respectively. We will add this information to the manuscript.

1.6 Fig.1

Fig 1 and similar cases: It took me a while until I figured out that the record is not consecutive and that 1.1.2020 follows the 31.12.2017. Please indicate somehow that there is a 2 year gap in the dataset (eg. by a gap between the panels)

Thank you for the comment. We will add a gap between the panels as suggested and update the figure caption as well. Following the updated figure:

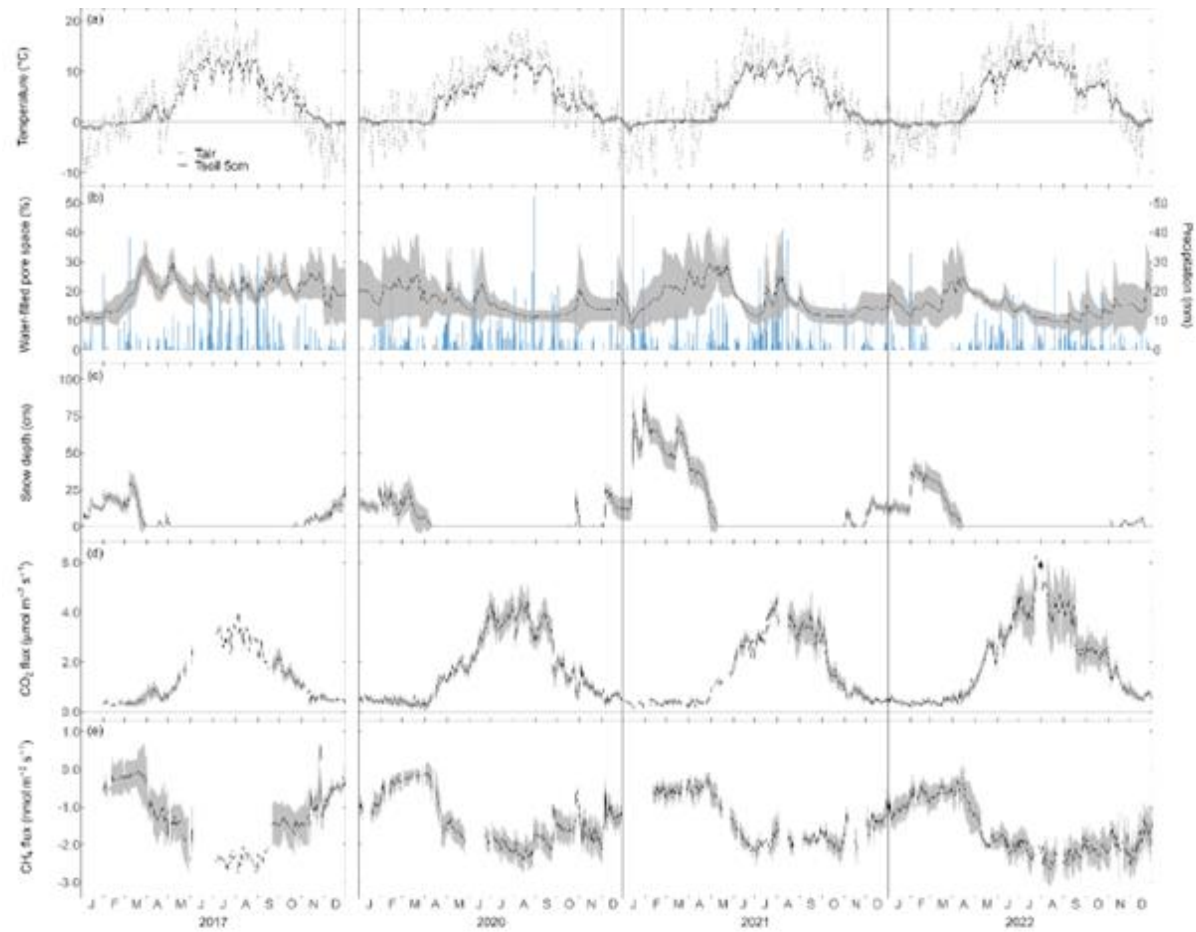

Fig. 3: Daily mean a) air temperature and soil temperature at 5 cm depth, b) water-filled pore space at 5 cm depth (left axis) and daily sum of precipitation (right axis), c) snow depth, and daily mean forest-floor d) respiration fluxes (not gap-filled), and e) CH_4 fluxes (not gap-filled), for the years 2017, 2020, 2021, and 2022. Please note the gap in measurements between 2017 and 2020. Black lines show means over four chambers, grey bands show standard deviations among four chambers. All data shown were quality-checked as described in the main text.

1.7 Line 230

I'd rather write "after snowmelt" instead of late winter

We are not sure what the reviewer refers to since we did not use the expression "late winter" in line 230. However, did you mean line 232 where we wrote "at the end of winter"? This we can change to "after snowmelt".

1.8 Lines 300-305

When discussing the budgets terms such as "higher-than-usual" "considerably higher" etc. should be avoided. If you want to make a solid statement that the 2022 fluxes were higher than average, then it would be necessary to apply statistics to the budget data. Otherwise no scientifically valid conclusion can be drawn.

Thank you for the comment. We will back up our statements with statistics in a revised manuscript. We will add to the text that the 2022 CO_2 budget and the annual mean T_{soil} of 2022 fall outside of the 95% confidence intervals (±1.96SD, i.e., for the CO_2 budget: ± 392 g CO_2 m^{-2} yr^{-1}). In case of CH_4, the 2022 budget does not fall outside of the 95% confidence interval, therefore, we will delete the word "considerably" in line 302.

1.9 Line 305

delete "exceptionally"

We will change this, thank you.

1.10 Discussion: Line 315

There is a bulk of literature that shows that CO2 emissions are depressed under really dry conditions – please rephrase here (the WFPS in the manuscript are generally rather low, but this is likely a matter of the very low bulk densities, which might have been underestimated a bit?). Anyway, was the soil in summer 2022 really so dry?

Thanks for the comment. We would like to point out that it is difficult to obtain reliable absolute values of soil moisture at our site due to a very heterogeneous soil, and many roots and rocks in the upper horizons (please see our response on SWC to reviewer 2). Therefore, in our discussion we would rather not focus on absolute SWC or WFPS values, but rather on relative changes in soil moisture. Annual means of WPFS were indeed lowest for 2022 compared to the other years, but also soil temperatures were very high (see above). We do not see any indication of soil moisture limitation of respiration in our data (see driver analysis). Bulk densities are indeed low at the site which is likely due to high organic matter content in the upper 5 cm. Bulk density calculations were done using soil data collected according to ICOS RI standards which gives us high trust that these values are correct (Arrouays et al., 2018, please see also Saby et al., 2023). We will rewrite the mentioned text section.

1.11 Table 4

Table 4 can be moved into the supplements, or also the results = measured fluxes from all the studies are shown in the table for comparison with the current ones.

Since long-term measurements for multiple greenhouse gas emissions are scarce, we would like to add the flux rates to Table 4 and keep it in the main text.

3. References

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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 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 CO_2 throughout the years, with large seasonal variability due to the temperature sensitivity of respiratory processes, but with very low N_2O emissions due to the overall low N supply at the site. In contrast, we expect that the forest-floor is a net sink of CH_4, 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 CO_2 emissions and highest CH_4 uptake in exceptionally warm years, such as in 2022 at our site. Overall, we anticipate the GHG budget being mainly determined by CO_2 fluxes, with CH_4 uptake only slightly offsetting the respiratory CO_2 losses, and very low N_2O 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 N_2O 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 N_2O.
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 N_2O fluxes measured with the static chambers mentioned above showed occasional N_2O 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 N_2O, depending on the prevailing environmental conditions such as oxygen availability, soil moisture and microbial communities. Under aerobic conditions, denitrification contributes to N_2O release, while under aerobic conditions, N_2O reduction to N_2 can dominate over N_2O production, which results in observations of net N_2O uptake by soils (Wen et al., 2017). Moreover, N_2O uptake has been observed in a German spruce forest (Goldberg and Gebauer, 2009). Therefore, we think occasional N_2O uptake as measured with our chambers are real.

Due to the scarcity of long-term and high-resolution N_2O fluxes from forest ecosystems, we think that our dataset is very valuable and would therefore like to keep the N_2O fluxes in the manuscript. Instead of showing the N_2O 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 N_2O 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 CO_2 uptake from the understory plants during daytime and thus talking about a full forest-floor CO_2 budget is misleading. Thus, we adjusted our terminology and now talk about a forest-floor “respiration” budget when talking about CO_2 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, N_2O and CH_4 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 CH_4 fluxes. We will add a plot showing the curvilinear relationship between CH_4 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±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.

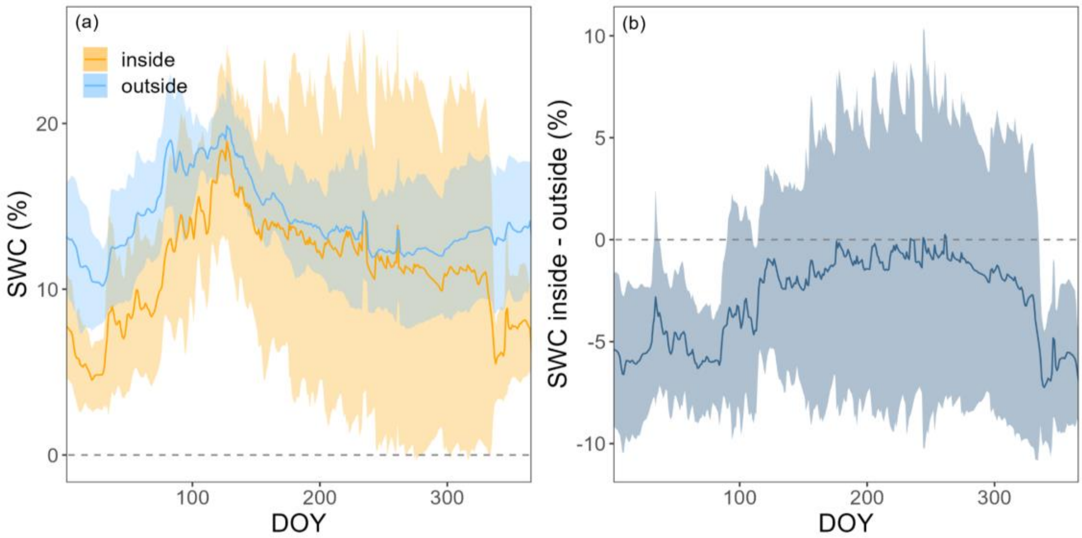

Fig. 2: a) Soil water content (SWC) at 5 cm inside (orange) and outside (lightblue) the chambers over the course of a year. b) Difference in SWC at 5 cm between inside and outside the chambers over the course of a year. Lines show means, bands show standard deviations over all years and chambers 1 and 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 CH_4 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 CH_4 fluxes and forest-floor respiration, respectively.

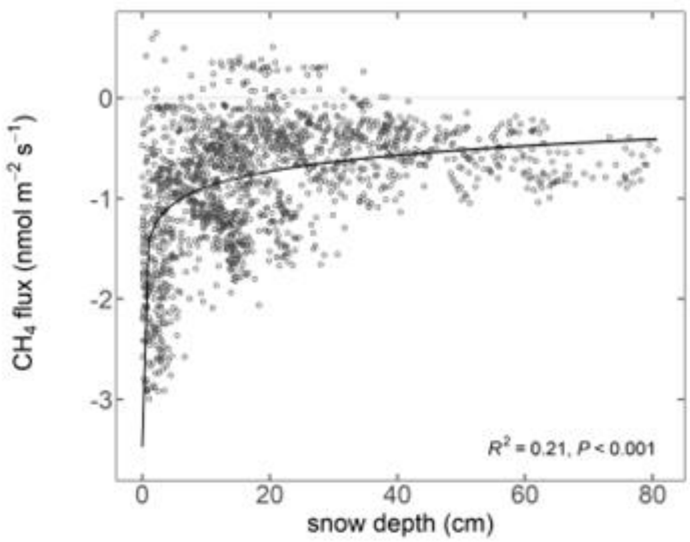

Fig. 3: Relationship between forest-floor CH_4 fluxes (nmol m^{-2} s^{-1}) and snow depth (cm). Black line shows fitted logarithmic curve.

1.12 Discussion

N2O fluxes are not discussed at all.

It is true that we did not discuss the N_2O 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 N_2O 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 CO_2-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 N_2O fluxes in the manuscript because such measurements in forests are very scarce. So, instead of removing N_2O from the manuscript completely, we would like to move the N_2O 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 N_2O fluxes in the paper and show that the magnitude of the fluxes is indeed very low.

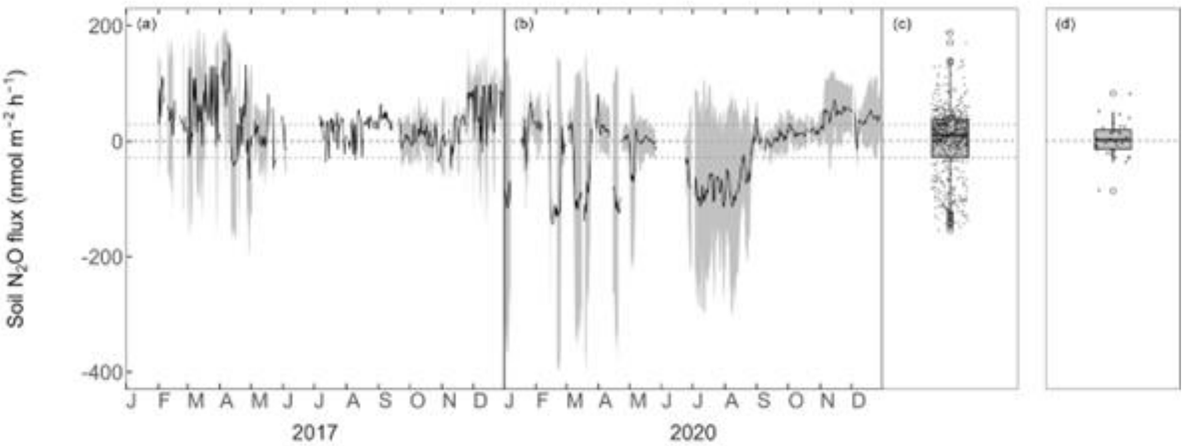

Fig. 4: Forest-floor N_2O fluxes (nmol m^{-2} h^{-1}) for the years 2017 (a) and 2020 (b). Black lines show means over four chambers, grey bands show standard deviations among four chambers. Boxplot showing distribution of means over four automatic chambers (c) and N_2O fluxes from static chamber measurements (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 CO_2, CH_4 and N_2O 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.

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