Discussion of “Forest-floor respiration, N_2O, and CH_4 fluxes in a subalpine spruce forest: Drivers and annual budgets”

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

Author Response to Referee 1 comments

Thank you for thoroughly revising the paper. It now reads superwell and is very interesting and informative. Especially the fact that soil CO2 efflux was very high in the warm year, albeit drying out soils is interesting. I also very much appreciate the environmental driver analysis via the random forest model. If authors are in the mood, I'd suggest a quick last round of fine-tuning.

Thanks a lot for the positive feedback.

I suggest being careful with the terms carbon-balance or carbon-budget and GHG-balance or GHG-budget throughout the text. For a carbon–balance only the carbon (C) in the CO2 and CH4 is relevant. Hence, only the annual carbon and not the annual CO2-, or the CO2-equivalent sum is relevant and should be reported (hence only CO2-C not all the mass of CO2 including two oxygen molecules is the topic of the carbon balance). This problem for instance becomes evident in the Abstract (L20-25). What is reported here is to my understanding the GHG-budget. In the GHG-budget, the fluxes of CH4 (and N2O) are aligned with the CO2 fluxes by calculating their CO2-equivalents or global warming potentials. This, however, has nothing to do with a “carbon balance”. Please check through the text, especially Page 17 4.3 Forest-floor C and GHG budgets.

Thank you for the comment. We were not using the term “balance” in the earlier manuscript, so this might have been a misunderstanding/interpretation.

When it comes to the terms “carbon budget” and “GHG budget” and their respective units (C or CO_2-eq) when referring to CO_2 and CH_4 fluxes, it is true that we have not used them consistently enough throughout the whole text. We changed the text accordingly to make it more consistent. We now only write about a “GHG budget” when considering all three GHGs (in g CO_2-eq m^{-2} s^{-1}). When not writing about all three, we mention the gases separately.

The abstract reads superwell except for the last lines. The sentence mentioned above might be re-formulated. E.g “The mean forest floor GHG-budget indicated emissions of ....CO2-eq...., with respiratory fluxes dominating and CH4 uptake offsetting a small portion (0.8%) of the CO2 emissions.” In the last sentence you may change to “....effects on the carbon sink of the forest ecosystem” (as it is written in the conclusions).

Thank you for the comment. We changed the two sentences in the abstract accordingly. They now read:

“The mean forest-floor GHG budget indicated emissions of 2319±200 g CO_2-eq m^{-2} yr^{-1} (mean ± standard deviation over all years), with respiration fluxes dominating and CH_4 offsetting a very small proportion (0.8%) of the CO_2 emissions. …... In a future with increasing temperatures and less snow cover due to climate change, we expect increased forest-floor respiration at this subalpine site modulating the carbon sink of the forest ecosystem."

Similarly, we adjusted the text in 3.3 and Tab. 1 in the Results, and in the Discussion sections.

It is true that there exist only hand full studies about year round GHG flux measurements in alpine mountain forests with winter snow-cover. Therefore the study of Heinzle et al. 2023 (Soil CH4 and N2O response diminishes during decadal soil warming in a temperate mountain forest in AgrForMet) might be considered for the discussion. Heinzle et al also observed low CH4 emissions during snow cover (fig4), which is in line with your results and the N2O fluxes might be used as a good example for higher fluxes at such higher N containing sites/soils. I don't want to push this particular study into your paper, but as mentioned above, such studies are rare and the few ones conducted might not be neglected. However, if you don't like the study out of any reason, I am totally fine if you do not cite it!

Thank you for bringing this study to our attention. We included the paper in the sections 4.2 and 4.3.

Author Response to Referee 2 comments

The authors have satisfactorily revised most parts of the manuscript. Nevertheless, there are few statements with which I do not agree or of which I am not convinced.

The hypothesis is a bit trivial. One reason for using the automatic chamber system was apparently the temporal variability of GHG fluxes including 'hot moments' which can have a large impact on the annual GHG budget. Even if no hot GHG moments were observed, the method is suitable for this purpose.

We added the hypotheses on demand during the review process. We think that coming up with hypotheses in hindsight is not ideal, we would have preferred to stay with objectives only. "Hot moments" are not mentioned in the current version of the manuscript. We agree with the reviewer that this is a "hot topic", but this will be studied/written in a separate manuscript, although the reviewer seemingly would have loved to read about it in this manuscript already.

I cannot agree with the statement that the N input by deposition and the N availability are low in the forest. N deposition rates have decreased in European forests over the past two decades but are still at a high level. The natural background of N deposition would be about 1-2 kg ha-1 y-1. With a deposition of 10 kg N ha-1 y-1, I would not expect any substantial N limitation in the coniferous forest. N2O emissions from coniferous forest soils are usually very low, even at significantly higher N inputs. This 'forest type' effect on N2O could be discussed in more detail.

We politely disagree. The vegetation itself is the indicator for high or low N supply and N limitation. As mentioned in the manuscript, the foliar N concentration at the site is about 1 % which is well below the optimum range in needles of between 1.5 and 2.3 %. Moreover, N deposition per se is insufficient to detect high or low N supply, as nicely summarized by Butterbach-Bahl et al. (2011) in the European Nitrogen Assessment (Tab. 6.2, Chapter 6; Sutton et al., 2011):

Table 6.2 Characteristics of coniferous forest ecosystems with low, intermediate and high N status (Gundersen *et al.*, 2006). Nitrogen input is not a good indicator of N status, but the ranges given are typical for low, intermediate and high N status ecosystems

Nitrogen status	Low N status (N-limited)	Intermediate	High N status (N-saturated)
Input (kg N ha^{-1} yr^{-1})	0–15	15–40	40–100
Needle N% (in spruce)	< 1.4	1.4–1.7	1.7–2.5
C:N ratio (g C g N^{-1})	> 30	25–30	< 25
Soil N flux density proxy (litterfall + throughfall) (kg N ha^{-1} yr^{-1})	< 60	60–80	>80
Proportion of input leached (%)	<10	0–60	30–100

Furthermore, N deposition rates of 1-2 kg N ha^{-1} yr^{-1} are rather preindustrial. And yes, also at our site, N deposition has decreased over the last decades (Gharun et al., 2021; from 17.5 kg N ha^{-1} yr^{-1} in the late 1980ties). Critical loads (as the reviewer seems to have in mind) of 1-2 kg N ha^{-1} yr^{-1} are now-a-days used for soft-water alpine lakes, tundra etc., while for fir and spruce forests, the critical load is about 10 to 15 kg N ha^{-1} yr^{-1}. This is supported by a recent report by Hettelingh et al. (2017) on European Critical Loads (chapter about Switzerland, p. 177-190, written by Swiss Federal Office for the Environment) where N deposition levels below 10 kg N ha^{-1} yr^{-1} were set as the lower limit above which critical loads for nutrient N (CLnutN) were calculated. This evaluation is supported by Braun et al. (2017) for Switzerland, and Wang et al. (2022) for Europe. Only N deposition rates above 20–22 kg N ha^{-1} yr^{-1} were negatively related with basal area increments for Norway spruce, thus harmful, while below this N deposition growth increased, clearly showing N limitation to tree growth (Braun et al., 2017). Similarly, N deposition rates around 22 kg N ha^{-1} yr^{-1} had the highest positive effect on NEP (C sink) of forests across Europe (Wang et al., 2022), again, indicating N limitation below this level. Together with the best indicator, i.e., the vegetation and its foliar N concentration, we think that we can safely say that our forest is indeed low in N, also compared to other Swiss and European forests, and thus low N_2O emissions are to be expected. We have added information and the additional references in the discussion, which now reads:

"At our site, N supply to plants and microorganisms is limited. 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). Furthermore, N concentrations in the soil are low (1.4% in the organic layer, 0.4% in 10–20 cm depth; Jörg, 2008). N deposition at the site (about 10 kg N ha^{-1} yr^{-1}; Thimonier et al., 2019; Gharun et al., 2021) corresponds to the lower level of critical N loads for forests in Switzerland (Hettelingh et al., 2017), well below the N deposition negatively related to basal area increments for spruce (20–22 kg N ha^{-1} $year^{-1}$; Braun et al., 2017) or that with the highest positive effect on net ecosystem productivity, i.e., the C sink, of forests across Europe (22 kg N ha^{-1} yr^{-1}; Wang et al., 2022). Thus, our site can clearly be considered rather low in N, which could be used for microbial transformations like nitrification, competing with plant uptake (Schulze, 2000), therefore, low soil N_2O fluxes were to be expected."

I agree that overall N2O fluxes are very low but suspect that many negative fluxes could be methodological. However, the finding that the forest soil can be a net N2O sink even at higher soil water contents is critical in my view. How can the strongly fluctuating and negative N2O fluxes in winter 2020 be explained? As far as I know, net N2O uptake in soils was only observed under dry conditions in summer.

We never claimed that we found a net N_2O sink even at higher soil water contents. Fig. 2 clearly shows large variations in N_2O fluxes but generally a source of N_2O during winter. Overall, the N_2O source of the forest floor is very low, close to zero. In any case, we are aware of at least one study from a temperate forest which has found net N_2O uptake during winter (Heinzle et al., 2023). Furthermore, we have applied strict quality assessment based on the RMSE of the linear fit in change in N_2O concentration, which excluded around 25 % of all N_2O fluxes. We are confident that the reported fluxes are correct.

The argument with the chamber comparison did not completely convince me. An open question is whether low negative N2O fluxes would occur with a longer detection time of e.g. 20-60 min as compared to 180 s. The accuracy of the measurement should increase significantly with the duration of the measuring time. Such a test could be easily performed and would increase the credibility of the method. For future N2O measurements with laser technology, it is a fundamental question how long detection time should be to generate robust results. Given the very large headspace volumes and the very long tubes between the chambers and the laser, it would be useful to systematically investigate the influence of the detection time on the N2O flux rate.
We agree that such a test would be helpful when no additional, clear evidence is available that the N status of the forest is low and therefore also the N_2O fluxes. Moreover, for a methodological paper, one would need to compare fluxes using different/multiple laser spectrometers, but such a study should be done at a site where larger N_2O fluxes occur, such as grasslands or croplands, not at a low N supplied spruce forest. As we have written in the manuscript (and the earlier answers to the reviewers), the choice of closure time is a compromise between avoiding confounding effects on environmental conditions during chamber closure and the flux detection limit of our method. As we have described in our responses in the last round, a longer closure time would enable us to reduce the flux detection limit. However, for our purpose, we did not need this level of precision, as highlighted by the fact that the second chamber method (i.e., static chambers and gas chromatography, closure time of 1 h) confirmed the low magnitude of N_2O fluxes at our site. The small chambers had a much longer integration time and nevertheless showed minimal fluxes even when environmental conditions were favourable for N_2O production. Therefore, based on this additional evidence, we are confident that the low N_2O fluxes that we measured with our automatic chambers are reliable.

Response 1.5 'Thus, high respiratory losses from the forest floor will decrease the forest C sink.' Higher soil CO2 fluxes can also be caused by an increase in root respiration or in litter production. Based on soil respiration, no conclusions can be drawn about the C sink strength of forests. To answer this important question, long-term and comprehensive analyses of all forest C fluxes or C stocks in the biomass and in the soil are required. Please, omit statements 'forest C sinks'.

We are not sure we understand the reviewer correctly. The forest C sink is the difference between GPP and R_{eco}. Beyond our study, the flux community has clear evidence that respiration, one part of the equation (R_{eco}), might indeed increase in the future due to higher temperatures. Forest-floor respiration (including root respiration) is a major component of R_{eco}. Litter production is not increasing respiration, unless litter is decomposed, which would then be included in forest floor respiration and in R_{eco}. Therefore, we think it is indeed feasible to say that the forest C sink might be modulated by an increasing forest-floor respiration (see also our answer to reviewer 1, abstract).

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