

Final author response to the reviewers

We would like to thank both reviewers for their constructive and thoughtful comments on our manuscript. We highly appreciate the time and effort they have invested in carefully evaluating our work. Their suggestions and remarks have been very helpful and will improve the clarity and quality of the manuscript. In the following, we provide detailed responses to all comments point by point. The comments are repeated below in black font, and the responses in blue font. Please note that all line references correspond to the manuscript version with tracked changes. In particular, we have added the previously missing detailed information on the applied methods (GC, LAS), which required structural and content-related revisions of the material and methods section. We would also like to note that we have implemented necessary adjustments in the Conclusions section as well. Furthermore, we have implemented minor revisions to enhance clarity and readability, corrected typographical errors, and incorporated six new references.

Review comment #1

The manuscript presents comparative trace gas measurements (N₂O, CO₂, and CH₄) using a GC and a MIR laser system. Both systems are commercially available. The measurements were conducted on two dates in two different treatments on a field under organic farming.

These two dates were very well chosen because they represented a wide concentration range, at least for N₂O and CO₂. The flux rates calculated using different algorithms showed a very high degree of agreement between GC and laser data for both CO₂ and N₂O. In contrast, the correlation between GC and laser measurements was significantly poorer for the CH₄ fluxes, which was probably a result of low CH₄ fluxes and an unfavorable ratio of volume to emitting surface area of the chamber system. To my best knowledge, the laser used is, apart from an ICL laser described by Stiefvater et al. (2023), the only device with a low weight that can be carried on the back, for example, which may be useful for practical application in the field. The manuscript is well written, the calculations and statistical methods are correct in my opinion, and the measurements were conducted with technical skill. The comparison of the two systems is particularly interesting for the greenhouse gas community because the laser used for CO₂ and N₂O measurements is currently experiencing strong sales growth in Europe among institutions that have previously worked with conventional GC methods. For this reason, the data is worth publishing! I was uncertain when evaluating the manuscript. On the one hand, all of my comments are easy to implement, but on the other hand, there were several comments, so I ultimately decided on a major revision.

Thank you very much for your thorough and positive review of our manuscript. We appreciate your recognition of the strengths of our study. Your comments on the CH₄ fluxes and the chamber volume ratio are very helpful, and we agree that this is an

important aspect to clarify. We have provided a response to the corresponding comment regarding the CH₄ fluxes and the chamber volume-to-surface ratio (effective chamber height) further below. We also value your perspective on the relevance of our results for the greenhouse gas flux community. We will carefully address all your comments and suggestions in the revised version.

The title is somewhat misleading. I initially expected that, for example, different detectors or different device settings would be used for GC or that several laser systems would be employed. Ultimately, two commercially available systems were compared without testing different device settings. Perhaps the authors could include this in the title, stating that two systems were compared and not just two detection methods.

You are absolutely right; the original title was potentially misleading. We have revised it in the updated version to: “Technical Note: Comparison of simultaneously applied chamber-based gas flux measurements from arable soils using gas chromatography (static chamber) and mid-infrared laser absorption spectroscopy (dynamic chamber)”

Introduction

Line 43: Delete „with syringes“, because there are also other collection systems.

We appreciate your suggestion and you are definitely right. We have removed “with syringes” and additionally added “discrete” as indicated in line 45–46.

I. 53-55: Most columns used for trace gas detection in GC separate CO₂ from N₂O (e.g. porapak Q, haysep, etc.). How explained Zheng et al. (2008) this effect of CO₂ on the detector response for N₂O?

Thank you for asking such a good question. Zheng et al. (2008) used a Shimadzu GC-14B with a Porapak Q column and an electron capture detector (ECD). Even though the column separates CO₂ from N₂O, they found that residual CO₂ can still interfere with the N₂O signal because the ECD responds to other electron-affine gases and the CO₂ peak appears between, and partially overlaps with, the CO₂ and N₂O peaks. As a result, the standard procedure using dinitrogen as the carrier gas, can give biased N₂O measurements, particularly when CO₂ concentrations are high (e.g. through soil respiration) and N₂O fluxes are very low (< 200 μg N m⁻² h⁻¹). We have briefly added this information in line 57 for further clarification, showing that chromatographic separation alone is not always sufficient.

I. 68: Chamber size is somehow too imprecise, better use the ratio of the chamber volume to the emitting surface area (effective chamber height)

We totally agree that the term chamber size is too imprecise. For clarification, we have replaced it with “effective chamber height (ratio of volume to emitting surface)” in line 68–69 to improve clarity.

I. 66-67: Please insert some citations!

Thank you for the hint. We have referred to Christiansen et al. (2015, doi: 10.1016/j.agrformet.2015.06.004) and Nickerson et al. (2016) (doi: 10.13140/RG.2.1.4149.2089) in line 70.

I. 71: I would suggest to use something like: world-wide very common GC method with a ^{63}Ni ECD.....

Thank you for the suggestion. We have taken it into account and have written “world-wide very common GC method” in line 74. However, since the mention of GC here refers to all three investigated GHGs, we have decided to omit the suggested information specific to the ECD (^{63}Ni ECD) for consistency.

I. 80: The following text has somehow found its way into the manuscript, remove it: Klicken oder tippen Sie hier, um Text einzugeben

Thank you for recognizing; the oversight was due to the citation software. We have now removed it from the manuscript (line 83).

I. 91: When were the fertilizers applied relative to the measurements?

Point well taken. Fertilizer applications and also soil tillage interventions are an important factor which requires specific and dedicated mentioning in the manuscript, in both the material and methods, and discussion. Accordingly, we have included a few sentences addressing this in lines 92–101 and lines 354–360.

Chapter 2.2.: Flux estimation: Given the amount and quality of data that a laser provides, the use of a generalized additive model may be more appropriate than the use of a parametric model (see Themistokleous et al. 2024, <https://doi.org/10.1111/ejss.13560>). Insert this information.

Thank you. As suggested, we have added the following statement to the discussion section (line 451–453) of the revised manuscript: “Given the amount and quality of data that LAS analyzers can provide, a suitable alternative approach might be the use of a generalized additive model for calculating fluxes from individual chamber measurements (Themistokleous et al., 2024; doi: 10.1111/ejss.13560), which is recommended for future assessments of LAS data.”

I. 93: State the pump rate of the laser.

We thank the reviewer for this comment. In our setup, the pump of the CH_4 analyzer was set to 5 V and the pump of the N_2O analyzer to 4 V. Due to the simultaneous operation of both analyzers, the resulting combined flow rate was approximately 0.4

to 0.5 L min^{-1} . As also mentioned in further comments, we have rearranged the paragraph and also added more detailed information about the LAS system to the material and methods section (line 130–137).

I. 93: Replace „with syringes“ by „or transferred to evacuated exetainers using syringes“!

We appreciate your suggestion. We have rearranged the whole paragraph and your suggestion has been taken into account in line 138–139.

I. 96: What was the point of the closable opening when the chambers already were equipped with a vent?

When placing a chamber on a collar, the downward movement can lead to capturing extra air inside the chamber. This results in an artificial overpressure inside the chamber which can either flush stored gases out of the soil pore system or act as a diffusive barrier at the soil surface. The closable opening prevents this chamber placement artefact. Additional captured air will escape through this hole as it offers the path of least resistance for excess air. We have now added this information in the revised manuscript; please refer to lines 109–113.

I. 97: Why have the authors used such high chambers? Though there is no optimum effective chamber height (V / A), commonly used chambers have an effective chamber height around 0.2 m, the chambers used for this investigation are in the range of 0.6 m! Please discuss this aspect more intense in the discussion section because it directly affects your results on MDF etc.!

Thank you for this helpful suggestion. We have added the effective chamber height (0.68 m) to the material and methods section in line 114–115 of the revised manuscript. Furthermore, we have added an explanation to the discussion (line 394–399) of the revised manuscript.

I. 108/109: The lasers are not really well described. Don` t assume every reader is familiar with the laser technique, so describe the principle briefly! Add further information on your devices such as the cell volume and the volume of the tubings, were the cells heated or not?

Thank you for this helpful suggestion. To give more detailed information about the analyzers, we have rearranged the paragraph and added further information about the LAS systems to the material and methods section (line 130–137) of the revised manuscript.

I. 110: Were the samples taken at equidistant intervals? If so, please specify the intervals!

Thank you for asking. Regarding the GC samples, we collected measurements at six time points. The interval between the first and second samples was 215 seconds and from the second sample onward, samples were taken at regular intervals of 390 seconds up to the sixth sample for all chamber measurements. We have now included that information in the material and methods section in the revised manuscript; please refer to lines 139–141.

I. 112: The vials were filled with slight overpressure (~20 ml gas sample squeezed into the 12ml container). Why? To load sample loops etc.? Explain briefly, don't assume every reader (including me) knows the functioning of a Bruker autosampler!

Thank you for your valuable comment. Since neither the gas chromatograph nor the autosampler has its own pump to draw the sample into the system, overpressure is used in the sample vials (~20 ml gas sample was transferred into a 12 ml vial). This ensures that the sample is pushed into the sample loop and also pushes any residues from the previous sample out of the sample loop to prevent the samples from mixing. Another important reason for overpressure is the protection of the gas sample during storage: if the septum becomes leaky, the gas will escape outward first, preventing contamination from ambient air. Minor leaks therefore pose no risk to the sample gas. We have now added this information in line 150–154 of the revised manuscript.

I. 113-116: Please insert more information on GC settings that are of relevance for the sensitivity of the ECD towards N₂O, like e.g. the detector temperature!

Thank you for the important note. The GC used was a *Bruker Model 450 (Bruker Corp., USA)* connected to an autosampler and equipped with three separate detectors: a thermal conductivity detector (TCD, 200 °C) for CO₂, a flame ionization detector (FID, 300 °C) for CH₄, and an electron capture detector (ECD, 300 °C) for N₂O. The column oven was maintained at 50 °C. In the first channel, which carried both the TCD and FID, the gas first passed through a 1.0 m × 1/8" Hayesep Q (80/100) guard column, followed by a 1.5 m × 1/8" Molsieve 13X (80/100) column for separation of CO₂ and CH₄. The second channel dedicated to the ECD, was equipped with a 0.5 m × 1/8" Hayesep N 80/100 guard column and a 2.0 m × 1/8" Hayesep D 80/100 column for N₂O separation. No water flushing or additional pre-columns were used, and Ar/CH₄ (10 mL min⁻¹) was applied as make-up gas for the ECD. We have now added this and further details on the GC measurements, as can be seen in 142–154.

I. 130: Explain abbreviation LGA

Thank you for pointing that out. The abbreviation “LGA” (Laser Gas Analyzer) was used mistakenly, and it is no longer in use. We have updated the text and replaced it with our current abbreviation, “LAS” (Laser Absorption Spectroscopy) in line 170 of the revised manuscript.

I. 192: Explain abbreviation GA

Thank you for pointing that out. The abbreviation “GA” (Gas Analyzer) was used mistakenly, and it is no longer in use. We have updated the text and replaced it with our current abbreviation, “LAS” (Laser Absorption Spectroscopy) in line 233 of the revised manuscript.

Table 1: Data for CO₂ and N₂O are the same for the laser measurements, is that a copying error?

We understand that this question arises, as the values appear almost identical except for the decimal places (CO₂ is reported in g m⁻² h⁻¹ and N₂O in mg m⁻² h⁻¹). This similarity is due to the analytical precision of the instruments (200 ppb for CO₂ and 0.2 ppb for N₂O) and the fact that the two gases have very similar molar masses.

Table 1: Values in g (for CO₂) and mg (for N₂O) are difficult to compare with literature, present them in more common units as mg CO₂ or as µg N₂O and CH₄

We appreciate this helpful comment and understand the concern regarding comparability with literature values. However, we prefer to keep the current units (g m⁻² h⁻¹ for CO₂ and mg m⁻² h⁻¹ for N₂O and CH₄) to ensure consistency with all other figures, tables, and results presented in the manuscript, where the same units are used. Maintaining identical units across all sections facilitates comparability within the paper and improves readability. Changing the units only in Table 1 could cause confusion, while the current representation adequately reflects flux magnitudes observed in our study.

I. 163: The analytical precision for N₂O is given with 0,0002 ppb. In the Allan plot of the datasheet provided for the MIRA LAS the precision is <100 ppt. This value was also provided by the AERIS company, please correct the value and also check for CO₂ and CH₄!

We thank the reviewer for pointing out this mistake. The MDF calculations were carried out using the correct precision values; however, there was an error in the running text. The correct values are 0.2 ppb for N₂O, 200 ppb for CO₂, and 1 ppb for CH₄ and are taken from the instrument’s data sheets. We have corrected this in line 203–205 of the revised manuscript.

Figure 3c: Check Y axis labelling, GC should be wrong, right?

Thank you for pointing that out. We have corrected the axis label of Figure 3c by removing "GC", which now correctly reads “N₂O flux (mg m⁻² h⁻¹)”.

I. 288/289: across all three investigated gases..... There aren’t any high CH₄ fluxes presented, please correct the sentence accordingly.

We thank the reviewer for this helpful comment. We agree that no high CH₄ fluxes were observed in our dataset; however, the typical range for arable soils (Le Mer and Roger, 2001, doi: 10.1016/S1164-5563(01)01067-6) is well represented. Accordingly, we have revised the corresponding sentences as indicated in line 332–335 to clarify this point.

L.289/290: could have offered further insights Unclear, insights in what?

Thank you for pointing this out. Our intention was to express that, if significant treatment effects had been observed, we could have examined whether these effects were consistent across the different measurement methods and flux calculation approaches, which would have provided additional insight. Since, however, no statistically significant treatment effects were found, we have revised the sentence in line 335–337 of the revised manuscript for clarity.

L. 326: Even in the low concentration range.... Should be replaced by something like: ... under conditions with only low changes in CH₄ concentrations

We appreciate your suggestion. We agree that your proposed wording sounds clearer and the sentence was updated accordingly in the revised manuscript (refer to line 379–381).

L. 331: However, within this study, CH₄ oxidation rates were in part comparatively high: CH₄ oxidation rates in aerated soils rarely exceed values of 0.1 mg m⁻² h⁻¹ (Le Mer and Roger, 2001), but in our case, we observed rates up to 0.8 mg m⁻² h⁻¹. Figure 4 shows something different: CH₄ fluxes ranged between +0.03 and -0.08 mg m⁻² h⁻¹! This range is common for well aerated soils and definitely not exceptional high!

Thank you for pointing out this mistake. We accidentally wrote 0.8 mg m⁻² h⁻¹ instead of 0.08 mg m⁻² h⁻¹ and therefore overstated CH₄ oxidation rates. Figure 4 shows the correct values of CH₄ fluxes which are within the commonly reported range for arable soils and not exceptional high (Le Mer and Roger, 2001, doi: 10.1016/S1164-5563(01)01067-6). According to Le Mer and Roger (2001), CH₄ oxidation rates in aerobic upland soils rarely exceed 0.1 mg m⁻² h⁻¹; in our study, we observed rates up to 0.08 mg m⁻² h⁻¹. This suggests that a more extensive dataset of simultaneous chamber measurements would probably not have provided further clarity, as our values were already close to this typical upper limit. We have now corrected the corresponding lines (386–391).

L. 334: For further investigations, reducing chamber volume may represent the most effective measure to minimize MDF. However, in field campaigns, this approach is often not feasible, as it is often of interest to include plants within the chamber and also to level out spatial variability over capturing larger soil surfaces (e.g. heterogeneous distribution of solid manure and composts (Krauss et al., 2017)). I agree with the first part of this sentence and would like to encourage the authors to discuss their chamber design concerning the high effective chamber height a little bit more in detail, also with respect to low CH₄

fluxes! The argument of growing plants in the chambers justifies the use of chambers with high effective chamber heights. However, at both points in time during the investigation, lower chambers would have been desirable in terms of minor changes in the N₂O and CH₄ concentrations of the chambers' atmosphere. Further, I don't understand the argument with the spatial variability. In order to minimize large spatial variability, the area covered by the chambers must be larger, but the ratio of chamber volume to emitting area can still be maintained!

We appreciate this helpful suggestion. You are right, that in our case, it would definitely have been possible and desirable to use chambers with lower height in terms of plant growth. However, as the crops grow taller, this is no longer feasible. The comparison measurements in this study were conducted due to a method change during a long-term GHG measurement campaign (not yet published). In this long-term study with weekly measurements, we aimed to include the plants (cover crops, potatoes, winter wheat) within the chambers. The comparison measurements reported here were performed using the smallest chamber setup available to us.

We agree, using chambers with lower height can still cover the same soil surface area and therefore also address spatial variability. Ultimately, chamber design depends both on the focus of the study and on available financial resources. At the same time, our results demonstrate that with LAS systems it is possible to even use large volume chambers while still maintaining a low MDF. We have addressed this point more explicitly in the revised version of the manuscript (line 391–399).

I. 344: The application of the goFlux script, including correction for water vapor inside the chamber during the measurement (dilution effect) (LI-COR, 2023), As far as I know, the AERIS Ultra already measures water vapor and corrects trace gas concentrations accordingly. Why is there a further correction needed?

For chamber measurements, three water corrections have to be considered. First for absorption band broadening, second for instrument cross-sensitivity, and third for the dilution of the air sample by water. The dry trace gas values reported by the Aeris Ultra are corrected for the first two effects which are specific for an instrument and are implemented in every trace gas laser analyser that reports dry values. The third correction is only necessary for chamber measurements. This correction is not implemented in the Aeris Ultra software and has to be done by the users themselves. We added this explanation to the discussion section (line 407–413) of the revised manuscript.

I. 347: ... This is in contrast to Kong et al. (2025), who found a significant effect of water vapor corrections on N₂O fluxes, especially below 50 µg N₂O-N m⁻² h⁻¹..... Please specify the detection mode in Kong et al.! Did they also use a MIR laser spectroscopy? And if so, what do the authors expect as a reason for this discrepancy?

Thank you for your comment. Kong et al. (2025, doi: 10.1016/j.agrformet.2025.110591) compared GC with a LI-COR LI-7820 N₂O/H₂O trace gas analyzer (LI-COR Biosciences, USA) based on optical feedback cavity-enhanced absorption

spectroscopy (OF-CEAS) employing a near-infrared (NIR) tunable diode laser source: The authors applied water vapor corrections for dynamic chamber approach (OF-CEAS) resulting in a significant effect on N₂O fluxes, especially below 50 μg N₂O-N m⁻² h⁻¹ (0.079 mg N₂O m⁻² h⁻¹, own conversion). This contributed to discrepancies in cumulated N₂O emission estimates in a similar chamber-based method comparison campaign on Danish arable soils. They explained this discrepancy mainly by the different enclosure times. In contrast to our experimental design, enclosure times were not the same for the two different measurement modes. The dynamic chambers, used with the OF-CEAS, had much shorter enclosure times (2 to 16 min), which minimized the effect of water vapor changes, whereas static chambers used for GC sampling remained closed for 45 to 110 min and were typically not corrected for water vapor at all. In addition, there might have been also a seasonal effect since the experiment from Kong et al. (2025) which is most comparable to ours had been performed in summer. The water vapor correction effect is highest at low CO₂ fluxes at wet soils under sunny, dry conditions. We have now added a few sentences to include this information and now also report values of absolute humidity from the two measurement days in our study (lines 417–431).

I. 371: However, for all three gases, a pattern of slightly higher fluxes, measured by GC, was observed. Unfortunately, the authors don't provide any reason for that phenomenon. Are there any ideas?

Thank you for your comment. This slight pattern should not be overinterpreted. For CO₂, the confidence is by far the highest, as all GC values are above the MDF, even though the median differences compared to LAS values are only slight (Fig. 2c). For N₂O, the differences in medians (Fig. 3c) are even smaller, and the confidence is lower because the small estimated GC fluxes for N₂O are less reliable (< MDF). For CH₄, the median deviation between GC and LAS values is largest (Fig. 4c), but almost all GC CH₄ values (except one) fall below the MDF. Regarding CO₂, Christiansen et al. (2015, doi: 10.1016/j.agrformet.2015.06.004) reported that at very high CO₂ concentrations (> 2000 ppm), GC measurements tended to overestimate gas concentrations compared to cavity ring-down spectroscopy (CRDS), which they attributed to the non-linear response of the detector (TCD). In our dataset, however, the highest measured CO₂ concentration is only 1337 ppm, well below the levels at which (Christiansen et al., 2015, doi:10.1016/j.agrformet.2015.06.004) clearly observed this effect. Therefore, the exact cause of the slight overestimation in our CO₂ flux data remains unclear. Nevertheless, it is conceivable that the detector-related non-linearity reported by Christiansen et al. (2015, doi:10.1016/j.agrformet.2015.06.004) could play a role, implying that GC-based CO₂ flux measurements may slightly overestimate fluxes under certain conditions. We have included this information in line 460–471 of the revised manuscript.

I. 379: At the same time, practical limitations of the LAS include high acquisition costs... Costs for the CO₂/N₂O laser are approximately 45,000 US \$ and this is by far lower than the costs for a GC. If you also consider that the laser system does not

require any sample preparation (evacuating gas vials), no costs for GC operating gases, and no actual GC measurement, the costs for the laser will definitely be lower than for the GC!

Thank you for pointing that out. We agree that the costs for the LAS system (~ 90,000 USD when two analyzers for all three gases are needed) can be lower compared to GC or at least comparable, especially when considering operating costs. We have revised the discussion in the manuscript (line 487–488) and clarified that the acquisition costs for LAS systems are generally reasonable, especially when considering operating costs of GC systems.

Review comment #2

1. Scientific significance

The manuscript presents a direct comparison between GC and a portable mid-infrared laser system for closed-chamber flux measurements of CO₂, N₂O, and CH₄. This is an important and timely study: many institutions are currently transitioning from GC-based methods to laser analyzers, and independent validation is valuable. The results clearly demonstrate high agreement for CO₂ and N₂O, while highlighting the challenges for CH₄. The work will be of interest to the Biogeosciences readership, particularly those involved in field flux measurements and monitoring networks.

Thank you very much for this positive evaluation. We are glad that you consider our study timely and relevant for the Biogeosciences readership. We appreciate your recognition of the strengths of the comparison due to the simultaneous chamber measurements and will carefully revise the manuscript according to your detailed comments.

2. Scientific quality

The experimental design is sound, with simultaneous chamber measurements across two contrasting field days. Data handling and statistical treatment (RMSE, nRMSE, flux model selection) are appropriate. However, several methodological details are still unclear or inconsistent and need to be addressed before the results can be fully trusted:

- The timing of fertilizer application relative to the flux campaigns should be reported and discussed, as it directly affects interpretation of the N₂O pulse.

We respond the same as to RC1 (l. 91):

Point well taken. Fertilizer applications and also soil tillage interventions are an important factor which requires specific and dedicated mentioning in the manuscript, in both the material and methods, and discussion. Accordingly, we have included a few sentences addressing this in lines 92–101 and lines 354–360.

- The choice of regression approaches should be justified more clearly. Given the richness of the LAS dataset, it would be useful to explain why parametric models were favored and why non-parametric approaches (e.g. GAMs) were not pursued beyond citation. Even a short discussion would help.

We respond the same as to RC1 (Chapter 2.2):

Thank you. As suggested, we have added the following statement to the discussion section (line 451–453) of the revised manuscript: “Given the amount and quality of data that LAS analyzers can provide, a suitable alternative approach might be the use of a generalized additive model for calculating fluxes from individual chamber measurements (Themistokleous et al., 2024), which is recommended for future assessments of LAS data.”

- The discussion of CH₄ fluxes should be made consistent across the text and figures. At some points the manuscript refers to “comparatively high” oxidation rates (line 332), while the figures clearly show values in the common range for aerated soils. Please ensure that the wording throughout matches the actual flux magnitudes.

We respond the same as to RC1 (l. 331):

Thank you for pointing out this mistake. We accidentally wrote 0.8 mg m⁻² h⁻¹ instead of 0.08 mg m⁻² h⁻¹ and therefore overstated CH₄ oxidation rates. Figure 4 shows the correct values of CH₄ fluxes which are within the commonly reported range for arable soils and not exceptional high (Le Mer and Roger, 2001, doi: 10.1016/S1164-5563(01)01067-6). According to Le Mer and Roger (2001), CH₄ oxidation rates in aerobic upland soils rarely exceed 0.1 mg m⁻² h⁻¹; in our study, we observed rates up to 0.08 mg m⁻² h⁻¹. This suggests that a more extensive dataset of simultaneous chamber measurements would probably not have provided further clarity, as our values were already close to this typical upper limit. We have now corrected the corresponding lines (386–389).

- It would be helpful if the authors could reflect on whether the observed GC > LAS (line 375) could be related to chamber handling, calibration gases, or model selection differences, even if only speculative. This would provide more context for readers, especially those considering method choice in their own work.

We respond the same as to RC1 (l. 371):

Thank you for your comment. This slight pattern should not be overinterpreted. For CO₂, the confidence is by far the highest, as all GC values are above the MDF, even though the median differences compared to LAS values are only slight (Fig. 2c). For N₂O, the differences in medians (Fig. 3c) are even smaller, and the confidence is lower because the small estimated GC fluxes for N₂O are less reliable (< MDF). For CH₄, the median deviation between GC and LAS values is largest (Fig. 4c), but almost all GC CH₄ values (except one) fall below the MDF. Regarding CO₂, Christiansen et al. (2015,

doi:10.1016/j.agrformet.2015.06.004) reported that at very high CO₂ concentrations (> 2000 ppm), GC measurements tended to overestimate gas concentrations compared to cavity ring-down spectroscopy (CRDS), which they attributed to the non-linear response of the detector (TCD). In our dataset, however, the highest measured CO₂ concentration is only 1337 ppm, well below the levels at which (Christiansen et al., 2015, doi:10.1016/j.agrformet.2015.06.004) clearly observed this effect. Therefore, the exact cause of the slight overestimation in our CO₂ flux data remains unclear. Nevertheless, it is conceivable that the detector-related non-linearity reported by Christiansen et al. (2015, doi:10.1016/j.agrformet.2015.06.004) could play a role, implying that GC-based CO₂ flux measurements may slightly overestimate fluxes under certain conditions. We have included this information in line 460–471 of the revised manuscript.

3. Presentation quality

The manuscript is generally well written and easy to follow, but a number of technical clarifications are needed:

- Ensure consistency of terminology (GA, LGA, LAS). At present, the switching between terms could confuse the readers.

Thank you for recognizing this confusing mistake. The abbreviations “LGA” (Laser Gas Analyzer) and “GA” (Gas Analyzer) were used accidentally and are no longer in use. We have updated the text to (line 170 and 233) replace them with our current abbreviation “LAS” (Laser Absorption Spectroscopy).

- Describe the laser systems in more detail: principle of operation, pump rate, cell volume, tubing volume, and whether the cells were heated. Many readers will not be familiar with this instrumentation.

We respond the same as to RC1 (l. 108/109):

Thank you for this helpful suggestion. To give more detailed information about the analyzers, we have rearranged the paragraph and added further information about the LAS systems to the material and methods section (line 130–137) of the revised manuscript.

- GC methods: include additional details such as detector temperatures and a clearer explanation and justification for the overpressurization of vials.

We respond the same as to RC1 (l. 113-116):

Thank you for the important note. The GC used was a *Bruker Model 450 (Bruker Corp., USA)* connected to an autosampler and equipped with three separate detectors: a thermal conductivity detector (TCD, 200 °C) for CO₂, a flame ionization detector (FID, 300 °C) for CH₄, and an electron capture detector (ECD, 300 °C) for N₂O. The column oven was maintained at 50 °C.

In the first channel, which carried both the TCD and FID, the gas first passed through a 1.0 m × 1/8" Hayesep Q (80/100) guard column, followed by a 1.5 m × 1/8" Molsieve 13X (80/100) column for separation of CO₂ and CH₄. The second channel dedicated to the ECD, was equipped with a 0.5 m × 1/8" Hayesep N 80/100 guard column and a 2.0 m × 1/8" Hayesep D 80/100 column for N₂O separation. No water flushing or additional pre-columns were used, and Ar/CH₄ (10 mL min⁻¹) was applied as make-up gas for the ECD. We have now added this and further details on the GC measurements, as can be seen in 142–154.

We respond the same as to RC1 (l. 112):

Thank you for your valuable comment. Since neither the gas chromatograph nor the autosampler has its own pump to draw the sample into the system, overpressure is used in the sample vials (~20 ml gas sample was transferred into a 12 ml vial). This ensures that the sample is pushed into the sample loop and also pushes any residues from the previous sample out of the sample loop to prevent the samples from mixing. Another important reason for overpressure is the protection of the gas sample during storage: if the septum becomes leaky, the gas will escape outward first, preventing contamination from ambient air. Minor leaks therefore pose no risk to the sample gas. We have now added this information in line 150–154 of the revised manuscript.

- Tables and figures:

> Table 1 shows identical LAS values for CO₂ and N₂O, please check again since it seems likely to be a copy error.

We respond the same as to RC1 (Table 1):

We understand that this question arises, as the values appear almost identical except for the decimal places (CO₂ is reported in g m⁻² h⁻¹ and N₂O in mg m⁻² h⁻¹). This similarity is due to the analytical precision of the instruments (200 ppb for CO₂ and 0.2 ppb for N₂O) and the fact that the two gases have very similar molar masses.

> As pointed out by Reviewer 1, units could be presented in more standard forms (e.g. mg CO₂ m⁻² h⁻¹, or µg N₂O m⁻² h⁻¹) for easier comparison with literature.

We respond the same as to RC1 (Table 1):

We appreciate this helpful comment and understand the reviewer's concern regarding comparability with literature values. However, we prefer to keep the current units (g m⁻² h⁻¹ for CO₂ and mg m⁻² h⁻¹ for N₂O and CH₄) to ensure consistency with all other figures, tables, and results presented in the manuscript, where the same units are used. Maintaining identical units across all sections facilitates comparability within the paper and improves readability. Changing the units only in Table 1 could cause confusion, while the current representation adequately reflects flux magnitudes observed in our study.

> Please check Figure 3c labeling (as already noted by the Reviewer 1).

We respond the same as to RCI (Figure 3c):

Thank you for pointing that out. Yes, “GC” is absolutely incorrect here, and have removed it from the axis label. Now it reads “N₂O flux (mg m⁻² h⁻¹)”.

4. Further comments

The paper convincingly shows that LAS is a reliable alternative to GC for CO₂ and N₂O, and highlights the sensitivity limitations of GC for CH₄. To strengthen the impact for a Biogeosciences audience, I suggest the authors add a short reflection on how these findings apply to long-term monitoring networks and national GHG inventories (e.g. ICOS). This would underline the broader significance of the comparison.

Thank you very much for this valuable comment. While analyzers are already used at long-term monitoring sites (e.g. ICOS Ecosystem Stations) (Pavelka et al., 2018, doi: 10.1515/intag-2017-0045) and have also been applied in field experiments using manual (e.g. Dix et al., 2024, doi: 10.1016/j.agee.2024.108951) or rarely automated chambers (Brümmer et al., 2017, doi: 10.5194/bg-14-1365-2017; Bond-Lamberty et al., 2020, doi: 10.1111/gcb.15353), their limited mobility makes such measurements logistically demanding and constrains spatial coverage and therefore limits the ability to capture spatial variability. Recent advances in LAS techniques have led to the development of smaller and more portable analyzers, which now make high-frequency flux measurements less challenging and more feasible over a broader spatial scale with manual chamber applications, including at plot or ecosystem scale. Our results demonstrate that these portable LAS systems can detect very small CH₄ and N₂O fluxes that are often below the detection limit of conventional GC-based approaches. Consequently, portable LAS analyzers represent an important methodological advancement and can substantially improve the accuracy of annual and long-term GHG budget calculations, especially with respect to CH₄ and N₂O. We have added this paragraph in the revised manuscript in line 475–484.