

Authors' point-by-point response

Responses are shown in **blue**, and excerpts from the revised manuscript are shown in **red**.

Response to Reviewer 3

This paper by Beattie et al. presents a new version of a lightweight open-path methane sensor for use on a UAV. The new version of the sensor is significantly lighter and lower power consumption than the previous version. The sensor performance is characterized in the lab and field and then is used for flux quantification during a controlled release. The authors did a thorough job addressing the first round of reviewer comments, and I support publication. I have a few additional comments/suggestions:

We thank the reviewer for their positive feedback and helpful suggestions, which we address individually below.

1. I would suggest including Conley et al. AMT 2017 (doi:10.5194/amt-10-3345-2017) in the airborne measurement techniques.

We have added this reference to the text that introduces crewed aircraft measurements in revised manuscript.

L. 40-44: “This extrapolation requires assumptions about plume behavior and local meteorological conditions, potentially resulting in inaccuracies when deriving plume emission rates (Khaleghi et al., 2024; Gordon et al., 2015; Conley et al., 2017). Consequently, crewed aircraft measurements must be performed at much greater distances from the emission source than is required for ground-based measurements to capture as much of the plume as possible (Conley et al., 2017).”

2. Around line 30, it should be mentioned that the need for an atmospheric model can be eliminated by a tracer release (e.g., Roscioli AMT 2015 doi: 10.5194/amt-8-2017-2015). Of course, this requires site access so introduces other challenges.

We have revised the text to include reference to the tracer release method.

L. 32-34: “The reliance on atmospheric models can be removed through the use of a tracer gas release (Roscioli et al., 2015; Mønster et al., 2015, 2014; Scheutz et al., 2011); however, this requires direct access to the source location to release the tracer gas and instrumentation that responds to and distinguishes the source and tracer gases.”

3. It should be noted that Donahue 2026 (referenced on line 46) is a LIDAR system, so it does sample the full vertical extent of the plume.

We thank the reviewer for highlighting this distinction. We have now clarified this point in the text.

L. 47-49: “...though 90% probability of detection thresholds have been demonstrated for emission rates as low as 1 kg h⁻¹ in column-integrated detection systems that capture the full extent of the plume (Donahue et al., 2026).”

4. Several open-path measurements have been demonstrated for methane using a reflector on a UAV and should be referenced: Cossel AMT 2023 doi: 10.5194/amt-16-5697-2023 and Soskind Remote Sensing of Environment 2023 doi: 10.1016/j.rse.2023.113513

We agree with the reviewer that this omission was an oversight. Reference to ground-to-UAV systems using an onboard reflector has now been added.

L. 63-66: “In remote sensing, column-integrated gas concentrations are usually inferred from surface-scattered light or thermal emissions detected from the aerial platform (Thorpe et al., 2017; Yang et al., 2018), though measurements have also been conducted using ground-based instrumentation coupled with a UAV-mounted retroreflector (Soskind et al., 2023; Cossel et al., 2023).”

5. Another open-path methane sensor: <https://www.mdpi.com/1424-8220/20/2/454>

We thank the reviewer for bringing this work to our attention. This instrument is now referenced in the text.

L. 88-91: “Another near-infrared open-path methane sensor developed for UAV deployment uses the cavity ring-down spectroscopy (CRDS) technique (Martinez et al., 2020). This instrument weighs 4 kg with a 1 Hz precision in the range of 10-30 ppb and a power consumption of 12 W.”

6. A reference should be provided for WMS when first discussed

We have added a reference to Reid and Labrie (1981) in line 122, where WMS is discussed.

7. Do you do anything to protect the mirrors?

No, the mirrors are not protected. We do monitor the mirrors for soiling and clean when necessary, however this is generally not an issue for short-duration field deployments in favourable weather conditions.

8. What does the statement that "the number and approximate positions of the reflected spots are confirmed by systematically blocking the laser beam" mean? Was each spot blocked at the face of the mirror in the cell?

We thank the reviewer for their question. We have revised the text to improve clarity.

L. 160-162: “The number and approximate positions of the reflected spots are confirmed by systematically blocking the laser beam at the face of one mirror and monitoring the detector response.”

9. Why was this particular methane line chosen? In particular, the temperature dependence of absorbance seems rather larger, could this be improved with a different line choice?

This methane absorption line was chosen due to its line strength near room temperature and to minimize interference from other gases, as described in Norooz Oliaee et al. (2022). It is also a viable choice because suitable lasers are commercially available. We believe that the temperature dependence shown in Fig. 5 arises from the combined effects of temperature-dependent absorbance and thermal expansion of components within the sensor’s optical head, which can affect the optical alignment. Further investigation is needed to de-couple these effects, quantify their individual contributions, and identify the most effective approach to mitigate or correct for signal variations caused by changes in ambient temperature. However, the reviewer is correct that this particular absorption line has a relatively strong temperature dependence and choosing a different absorption line could mitigate the impact of temperature-dependent absorbance. We have added the following text to the manuscript:

L. 390-392: “The impact of temperature-dependent absorbance could potentially be mitigated by selecting a different methane absorption line (Li et al., 2020), though there may be associated trade-offs such as reduced line strength and more limited commercial availability of lasers.”

10. Line 341 typo "for by"

We thank the reviewer. This typo has been corrected (removed “for”).

11. Line 385 add ref for $2f/1f$ (i.e., Rieker et al 2009, already cited). Why was this not implemented?

The citation has been added, per the reviewer’s recommendation. We did not implement $2f/1f$ normalization for the sensor presented here due to problems integrating the more demanding computation into the instrument code, which runs on the onboard single-board computer. Though we expect that by optimizing resource allocations on the single board computer, this normalization scheme could be implemented with the same hardware. We determined that $2f$ alone was sufficient for the work presented here, but we hope to add $2f/1f$ normalization in the future. The following text has been added to the manuscript:

L. 397-399: “Though $2f/1f$ normalization is more computationally demanding than the $2f$ calculation used in this work, we expect that it could be implemented with the same hardware by optimizing resource allocations on the single board computer.”

12. What is the warm up/stabilization time?

The typical stabilization time is around 10 minutes (see revised manuscript L. 171).

13. Line 533: I assume the statistical measurement uncertainty (3%) was calculated by the quadrature sum of the calibration and drift, correct?

Yes, the reviewer is correct. We have modified the text to clarify this point.

L. 547: “...accounts for the 1σ calibration uncertainty ($< 1.5\%$) and in-flight measurement drift ($< 2.5\%$) added in quadrature.”

14. Could you elaborate more about where the 5% syst uncertainty estimate came from?

As described in the Appendix (L. 650-651), the 5% systematic uncertainty accounts for “error introduced by variations in temperature, pressure, and relative humidity.” Lab measurements indicated a maximum change in signal strength of approximately 5% when the ambient temperature changed by 7.8°C. For variations in relative humidity, we saw a 1.9% change in signal strength for a 1% change in water vapour mole fraction at 21°C. Though we corrected the field data for the effect of relative humidity compared to the calibration with dry gas, there was uncertainty in the actual in-flight temperature and relative humidity due to problems with the on-board BME280 sensor, as described in Section 3.3.3. We also did not verify how the effects of temperature and relative humidity are inter-related, and we did not quantify the impact of ambient pressure on the signal strength. As indicated in the manuscript (L. 620-622) “Detailed characterization of these effects and improved on-board temperature and relative humidity measurements could facilitate the implementation of robust on-line corrections, which would enable more accurate quantification of background methane levels.” Though 5% is likely an over-estimate of the true systematic uncertainty introduced by the variable ambient conditions in this study, we felt that a conservative estimate was warranted until we obtain experimental evidence to justify a smaller error bound. The following text was already included in the manuscript:

L. 549-552: “Additionally, though the measured data was corrected to compensate for the difference in relative humidity compared to calibration, uncertainty in the correction factor and in the environmental conditions remains. For this reason, we apply a conservative systematic uncertainty of 5% in addition to the statistical uncertainty.”

We have added the following new text to elaborate on the reason for selecting 5%:

L. 552-553: “This 5% is intended to account for variability in the signal strength observed with changing temperature and humidity, as discussed in Section 3.2, as well as potential pressure-related effects.”

Response to the Editor

The authors have carefully addressed the critical comments of the two reviewers in the first round and improved the manuscript substantially, especially in terms of

- Better referencing literature on other similar instruments and better putting the specifications of this instrument in perspective
- Better assessment of measurement uncertainties including a better assessment of performance in the field as opposed to performance under laboratory conditions. An appendix A with a detailed uncertainty assessment for emission flux quantification has been added.
- Analysis of etalon effects (only in their response to reviewer #2)
- A more comprehensive description and analysis of onboard wind measurements on the drone including a new figure comparing the measurements with a stationary sensor on the ground.
- An analysis of the impact of varying water vapor mixing ratios on the measurements including a new figure.
- An analysis of sensor response (only in their response to reviewer #2) in comparison with commercial Aeris MIRA Pico and Licor 7810 sensors.

A third reviewer evaluated the revised manuscript and overall rated this improved version very positively. Because no further reviewer could be found, I took the liberty to step in and provide an additional independent assessment in my role as editor.

I agree that the manuscript is now in good state and recommend publication subject to a few minor points as described below:

We thank the editor for their positive feedback and helpful comments, which we address individually below.

1. Please address the comments of the third reviewer.

A point-by-point response to Reviewer 3’s comments is included above.

2. Page 10, line 278 (revised manuscript): The formula explaining conversion from mixing ratio to mass density is missing the mixing ratio symbol. Please also note that the standard symbol for pressure is lowercase p not P.

To improve clarity in defining the methane flux density, we have revised equation 4 and the text that follows to read:

L. 284-288: $q(y, z) = \left(\chi_{\text{CH}_4}^{\text{dry}}(y, z) - \chi_{\text{CH}_4}^{\text{bkg,dry}}(y, z) \right) \left(1 - \chi_{\text{H}_2\text{O}}(y, z) \right) \cdot u_x(y, z) \cdot p(z) \frac{M_{\text{CH}_4}}{R T}$ (4)

where $\chi_{\text{CH}_4}^{\text{bkg,dry}}$ is the background dry methane mole fraction, u_x is the wind speed component orthogonal to the y-z flux curtain, $p(z)$ is the pressure at altitude z , $M_{\text{CH}_4} = 16.04$ g/mol is the molar mass of methane, R is the gas constant, and T is the average temperature. The pressure $p(z)$ is estimated by applying a linear fit to the data recorded by the onboard pressure sensor throughout the flight.”

3. Page 18, line 432: Ambient temperature was about 24°C rather than 17.5°. Thus, it seems more appropriate to call this "approximate thermal equilibrium with ambient temperature".

We have implemented this change per the editor’s recommendation.

L. 445: “... approximate thermal equilibrium with the ambient temperature.

4. CH₄ measurement units: Although the authors claim that data is reported in (moist) mole fractions and not in dry mole fractions, this is not sufficiently clear and consistently done in the manuscript. Here are few considerations:
 - a. CH₄ measurements are usually reported in dry mole fractions. It makes comparison of measurements easier because it is independent of variations in specific humidity, which are typically in the order of 0-3%. Dry mole fractions are conserved during atmospheric transport also in the presence of moisture processes (condensation, evaporation, etc.).
 - b. The laboratory calibrations were performed in dry conditions. The Li-7810 reference instrument likely reports dry mole fractions per default.
 - c. The new figure 7 shows how dry mole fractions change with increasing humidity relative to dry mole fractions measured by the LI-7810. Based on this, a uniform 2.1% correction is applied to all measurements during the controlled release experiment. However, this correction is based on an evaluation of dry mole fractions. For moist mole fractions the correction should be different because there is the additional effect of water dilution in the conversion between dry and moist (see line 415 on page 17).

We thank the editor for highlighting that the distinction between moist and dry mole fractions was not sufficiently clear in the manuscript, and for raising the point that dry mole fractions are more commonly reported in the literature. We have made the following changes to the manuscript to improve clarity and address the editor’s concerns:

L. 279-280: “Methane mole fractions χ_{CH_4} are recorded by the sensor at a rate of 100 Hz throughout the flight. They are converted to dry mole fractions using the relation $\chi_{\text{CH}_4}^{\text{dry}} = \chi_{\text{CH}_4} / (1 - \chi_{\text{H}_2\text{O}})$, where $\chi_{\text{H}_2\text{O}}$ is the water vapor mole fraction.”

L. 284: $q(y, z) = \left(\chi_{\text{CH}_4}^{\text{dry}}(y, z) - \chi_{\text{CH}_4}^{\text{bkg,dry}}(y, z) \right) \left(1 - \chi_{\text{H}_2\text{O}}(y, z) \right) \cdot u_x(y, z) \cdot p(z) \frac{M_{\text{CH}_4}}{R T}$ (4)”

L. 288-290: “In this work, equivalent $\chi_{\text{H}_2\text{O}}$ values were used for both the measured and background dry mole fractions $\chi_{\text{CH}_4}^{\text{dry}}$ and $\chi_{\text{CH}_4}^{\text{bkg,dry}}$ such that Eq. 4 simplifies to:

$q(y, z) = \left(\chi_{\text{CH}_4}(y, z) - \chi_{\text{CH}_4}^{\text{bkg}}(y, z) \right) \cdot u_x(y, z) \cdot p(z) \frac{M_{\text{CH}_4}}{R T}$ (5)”

Fig. 3: x-axis label changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppm)”. Equation 8 and other relevant text has been updated equivalently.

Fig. 4: y-axis label for panels (a) and (b) changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppb)”. In-flight data shown in panels (b) and (c) updated to dry mole fractions – corresponding precisions at 0.1 s, 1 s, and optimal averaging times have been updated accordingly throughout the manuscript.

Fig. 5: y-axis label changed to “ $\Delta \chi_{\text{CH}_4}^{\text{dry}}$ (%)”.

Fig. 6: y-axis label changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppm)”.

Fig. 7: x- and y-axis labels for panel (a) changed to “ $\chi_{\text{H}_2\text{O}}$ (ppm)” and “ $\Delta \chi_{\text{CH}_4}^{\text{dry}}$ (%)”, respectively.

Fig. 8: Color-scale label in (a) and y-axis labels for panels (b) and (c) changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppm)”. Data updated accordingly.

Fig. 10: y-axis labels changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppm)”, “ $\chi_{\text{CH}_4}^{\text{bkg,dry}}$ (ppm)”, and “ $\chi_{\text{CH}_4}^{\text{elev,dry}}$ (ppm)” for panels (a), (b), and (c), respectively. Data updated accordingly.

Fig. 11: y-axis label changed to “ $\chi_{\text{CH}_4}^{\text{dry}}$ (ppm)” for the top panels. Data updated accordingly.

5. The standard symbol for mole fractions is chi (χ). Brackets as in [CH₄] represent number densities e.g. in chemical equations. Thus, please use χ_{CH_4} instead.

Notation for the CH₄ mole fraction has been updated from [CH₄] to χ_{CH_4} throughout the manuscript.

6. The analysis of uncertainties in the emission flux quantification in Appendix A is thorough, but it does not cover one important source of uncertainty, which is not related to measurement issues but to the effects of atmospheric turbulence. Even if the CH₄ and wind measurements are perfect, the estimated flux in the curtain does not perfectly represent the emissions, because the flux through any vertical surface downwind of the source varies in time due to turbulent fluctuations. This is probably also why the third reviewer suggested adding a reference to Conley et al. (AMT 2017). This is also why other authors propose to perform many observations (curtain flights) in order to reduce this statistical error. There should be at least a brief discussion of this additional uncertainty.

We agree with the editor that statistical analysis over many curtain flights would be required to fully account for the uncertainty introduced by atmospheric turbulence. We have added the following text to “Section 3.3.4 Emission flux quantification” to address this point:

L. 587-595: “Though we aimed to include as many sources of error as possible in the uncertainty estimation, one uncertainty element that is not fully accounted for is due to atmospheric turbulence and the non-stationary nature of the measured plume. When considering a single curtain flight recorded over a period of approximately 15 minutes, atmospheric turbulence causes variation between the horizontal transects that is not reflected in the mass-balance flux estimation. The associated uncertainty contribution is therefore partially accounted for in the interpolation and extrapolation error estimates, which evaluate the impact of individual transects on the estimated emission flux (see Appendix A). However, a statistical analysis over many curtain flights would be required for rigorous accounting of the atmospheric turbulence effect, which is beyond the scope of this demonstration. A detailed discussion of how turbulent atmospheric conditions affect emission flux estimation can be found in Conley et al. (2017).”