2 Response to Reviewer Comments 2 (RC2)

Link to Original Submission: A New Technique for Airborne Measurements to Quantify Methane Emissions Over a Wide Range: Implementation and Validation – Dooley et al. (2024)

(RC 201) There is a lack of discussion around the accuracy of the on-board wind measurements. As the wind measurement is so critical to the mass balance methodology and there is other work discussing the challenges of accurate wind measurements from drones, I would have expected a more comprehensive discussion on the particular nature of the setup and the uncertainties associated with different wind speeds and movement speed of the drone.

(AC 201) ***L186-199. Section 2.2. Figure A1. Figure A2.*** We thank the reviewer for identifying this weakness in the methodology presentation. The accurate measurement of winds can be challenging, and we have added content to the paper that better describes how the flight data are screened to remove wind anomalies arising from vertical motions or rapid horizontal accelerations of the M600P platform. We have added two figures to a supplement section (figures A1 and A2), and section 2.2 in Methods now includes a more detailed discussion of static wind calculation and validation.

(RC 202) Have there been direct comparisons to mast or tower measurements of wind speeds and directions? Are there periods in flight where the winds are clearly no longer correct due to either drone motion or wind speeds? Should there be filtering applied to certain events (e.g., fast turns?). These bits of specific information are important as other groups may use papers such as this as templates for setting up their own systems, or alternatively commercial outfits may be referring back to work such as this for justifying uncertainties when performing legally complying work for future methane regulations.

(AC 202) ***L186-199.*** See final paragraph in section 2.2.

(RC 203) The introduction feels like it is somewhat out of date, there is a lack of recent references compared to the rest of the manuscript. Given the nature of the package being demonstrated here, I believe that there should also be reference to the platforms and packages that have been developed in the commercial sector as well as academia.

(AC 203) ***L64-94.*** An overview of other UAS methods has been added to section 1.

(RC 204) There is a bit too much general background in the abstract for my liking on the global importance of CH_4 which really just belongs in the introduction. I'd prefer to see the abstract with some extra important technical details on the package, such as flight time capability and the limitations of the flying conditions in which good results were achieved.

(AC 204) ***L16-31.*** First two paragraphs of section 1 were combined and merged into L16-31.

(RC 205) I have concerns that it is not completely clear to me how the extrapolation to the top and bottom of the plume are computed (e.g., Figure 9) when it is clear that none of the transects are in background air at the lowest and highest transect. Similarly, it is not clear that the plume edge is caught on the lowermost transects in Figure 9. How are these issues accounted for, and how are you able to ascribe uncertainty to that unknown?

(AC 205) ***L321-334. Section 2.6.*** The reviewer has identified a critical aspect which should be made more clear – that incomplete plume sampling is one of the major challenges of implementing this technique. Please see more detailed discussion in section 2.6.

(RC 206) L60: “Satellite systems” feels far too catch-all. Please separate out into point source and area mapper discussion and allocate ranges of capability accordingly (it doesn’t feel right to lump GHGsat with TROPOMI in this type of discussion).

(AC 206) ***Section 1. L44-50. Section 4. Figure 12.***

(RC 207) L84: Precision discrepancy with abstract and later in manuscript. Please check and be clear where numbers are quoted from field measurements and where just taking manufacturers stated values.

(AC 207) ***Table 1.*** This error is corrected in the updated manuscript, uncertainty values quotes throughout the document now match the uncertainties listed table 1.

(RC 208) L86: Define response time – is this a $1/e$ value, a 90% fall time, some other metric?

(AC 208) ***L112-115.*** The "response time" was referring to the time needed for samples to travel from inlet to MIRA. Section 2.1.4 has been rewritten for clarity.

(RC 209) L178: What criteria was used to determine the order of the polynomial fit? Are there issues with fitting to the beginning and end of the run using higher order polynomials?

(AC 209) ***L229-231.*** The polynomial order, and other parameters described in section 2.3 selected empirically (trial-and-error) during initial processing. Higher order polynomials can affect the edges of the fitted timeseries; generally, however, higher order polynomials are not required after appropriately setting the other threshold parameters used for the gradient and outlier filters.

(RC 210) L186 and Fig 5: Different references are given to the stability class references – please ensure that these are correct or if both should be referenced at both locations.

(AC 210) ***L251-254. Figure 6.*** The Gaussian model used in section 2.4 was taken from Seinfeld et al. 2006. The stability class parameters are available in Seinfeld et al. 2006 and further detailed in Woodward 2010. Both references should be cited whenever stability classes are discussed in the text.

(RC 211) More information on what is going into the stability class selection would be helpful (maybe SI worthy rather than main script?).

(AC 211) ***Section 2.4.*** The Gaussian plume models used in section 2.4 helped with flight preparation and visualization, but detailed discussion of modeling parameters is outside the scope of this work.

(RC 212) L265: Define standard flight conditions!

(AC 212) ***L366-369.*** We define standard flight conditions as periods of high solar insolation and steady windspeed between 2-6 $\mathrm{m/s}$. This is discussed on lines L366-369 in the context of calculating the Limit of Detection (LOD) for the system. The plume evolution is dependent on multiple environmental conditions, but a cross-sectional area of 100 m2is used in the LOD calculation.

(RC 213) Fig 8: It would feel better to quote the uncertainties (and maybe throughout) to 2σ – it would make the figure more compelling that the method is reliable to 95% confidence.

(AC 213) ***Throughout. Figure 8. Figure 12. Table B1.*** The 2σ (95%) confidence interval is used for all presented flux uncertainties and figures.

(RC 214) Fig 11 and Ethane:methane discussion. (Calibration?) The data within this figure looks fantastic, but belies an issue that at no point has calibration of the CH_4 or C_2H_6 been discussed. From personal experience, I have seen similar sensors have gain factors of 0.7 on one channel (which would then make a significant difference to the results) – so I would hope that the instrument has been functionally calibrated in the laboratory prior to deployment. It would be expected that the calibration routine is at least alluded to in the manuscript, and potentially the details of the calibration put in the SI. I am mainly asking for this so that other groups do not use these instruments expecting that absolutely zero calibration is required.

(AC 214) ***L265-272.*** A controlled release 'pulse' from a known natural gas source is used to measure any lag or gain offset between the CH_4 and C_2H_6 channels.