Review: Dooley et al. AMT 2024.

General Comments:

There is indeed a gap that requires filling for methodologies to accurately quantify methane at point and site level for sub 50kg/hr emission rates – which this type of method can help to address. There is plenty of good work in the manuscript and really just needs some refinement and expansion to be suitable for full publication.

Main specific comments:

Wind:

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 set up and the uncertainties associated with different wind speeds and movement speed of the drone.

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.

Introduction:

The introduction feels like it is somewhat out of date, there is a lack of recent references compared to the rest of the manuscript.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.

Abstract:

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.

Methods / Uncertainties:

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 lower most transects in plume 9. How are these issues accounted for, and how are you able to ascribe uncertainty to that unknown?

Would it be possible to have an SI with figures to show the transects / plume cross-sections for all the calculated emission rates to inform the reader visually with the data quality?

Minor comments:

L40-55: More discussion on wind parameters and uncertainty within the context of mass-balance feels needed.

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).

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.

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

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?

L186 and Fig 4: Different references are given to the stability class references – please ensure that these are correct or if both should be referenced at both locations. More information on what is going into the stability class selection would be helpful (maybe SI worthy rather than main script?)

L265: Define standard flight conditions!

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

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.