

Author Response to Reviewer Comments

We thank both reviewers for the care they have taken with this manuscript. The comments are detailed and constructive, and several identified genuine errors that we are grateful to have caught before publication. We accept Referee #3's central criticism: the weather conditions during the campaign limit what can be concluded about the airborne systems, and the submitted manuscript did not state this plainly enough. We have revised the Discussion accordingly, and have added the airborne retrieval and validation literature that the referee notes was underrepresented to the Introduction, while keeping the paper focused on the comparison of vehicle- and aircraft-based systems.

We would also take this opportunity to restate what we believe the paper contributes. It is the companion to McManemin et al. (2026, *Atmos. Meas. Tech.*, 19, 923–934), already published, which evaluated commercial methane measurement technologies during the same controlled release campaign at TADI; the two papers were separated because the participants here are co-authors and because the academic teams comprised only vehicle- and aircraft-based platforms, with no overlap into the commercial satellite, drone and fixed-sensor groups. The data published in this publication is novel, and this structure is what makes this paper distinctive. Because the participating teams are authors, they were able to interrogate their own results after unblinding in a way that is not possible with proprietary methods — identifying, for example, a background-concentration error that inflated small-release estimates and demonstrating that substituting site wind lidar data for ERA5 reanalysis substantially improved airborne quantification accuracy. We think this evaluation is worth publishing alongside the commercial one, notwithstanding the limitations the referees identify.

Thank you for your time and thoughtful engagement with this manuscript. The comments pushed us to sharpen the framing and empirical support of several claims. We believe the manuscript is substantially stronger as a result, and we hope the revisions satisfy the Referees' concerns. We look forward to the Editor's decision.

Best,

Audrey McManemin (on behalf of all authors)

Response to Anonymous Referee #2 - Comments posted on August 3, 2026

Specific Corrections:

1. One methodological point on quantification accuracies (i.e. regressions Figure 8 and related analysis) that could be considered: All participating teams provide uncertainty estimates along with quantified emission rates; however, all regressions between estimated and true emission rates use *ordinary* least squares. The authors could consider utilizing generalized least squares (i.e. uncertainty-weighted), which can explicitly account for the variable uncertainty on each data point when calculating regressions for quantification accuracies. The primary benefit of this method would be to reduce the effect of outliers with large uncertainties on regressions (which doesn't seem to be driving quantification errors in this study), and better-use the uncertainty estimates provided for each quantification. As this study is a follow-on piece to McManemin 2026 which evaluated commercial methane detection and quantification technologies with similar methods during the same controlled release, it is acceptable if the authors wish to maintain consistency with this earlier paper.

RESPONSE: We thank the reviewer for this suggestion. We have retained ordinary least squares in the main text to maintain consistency with the companion evaluation of commercial technologies (McManemin et al., 2026), as the reviewer allows. MANUSCRIPT CHANGE: footnote (a) to Table 4 has been rewritten to state explicitly that the regressions use ordinary least squares, to give the consistency rationale, and to point to an uncertainty-weighted (generalized least squares) sensitivity analysis in the Supplementary Information (SI Sect. 6.3)

2. 75: "in situ sensors" are defined, but "active scanning systems" are left undefined. Do active scanning systems not also measure local concentrations? As all sensors in this paper are either vehicle based (in situ) or aircraft (remote imaging systems), I recommend defining active scanning systems or removing the discussion for this piece entirely.

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 1): all three sensor categories are now defined. A closing sentence has been added stating that the systems evaluated in this study fall into the first and third of these categories.

3. 281: RHUL's method undefined. "a traditional determination process based on the cloud coverage and the wind speed at the time of measurements" Please provide a citation or more description of the methodology used.

RESPONSE: Agreed; the description was insufficient. MANUSCRIPT CHANGE (Sect. 2.3.3): the sentence has been replaced by a description of the RHUL approach: the plume dispersion coefficients (σ_y and σ_z) were approximated using Briggs' assumptions with the Pasquill–Gifford atmospheric stability classification (Briggs, 1973), and a full description of the RHUL Gaussian plume procedure is given in Dowd et al. (2024). Both references have been added to the reference list.

4. Figure 3d is very hard to read, and missing labels or what the cells (Letters A-F?) indicate. Please improve readability of this figure.

RESPONSE: Agreed. MANUSCRIPT CHANGE: Regenerated the figure with larger text and clearer units. Added more text to the figure caption for a clearer explanation.

5. 341: Is the LGR precision at a given averaging time available? All other in situ sensors present a similar metric and would be helpful in comparing the different sensors used in this study.

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 2.3.5): the 1-sigma precision for 10 Hz CH₄ measurements (2.2 ppb) has been added. It was determined from random in-flight "target" calibrations, and the text points to Fig. S3 and Sect. S2.1.5 of the SI.

6. 436: The definition of "smaller releases" is ambiguous in this section. I believe that it refers to less than 100 kg/hr, but please clarify. Additionally, plots of POD are shown for less than 30kg/hr, but this section segments at 100kg/hr. Please provide a small justification for the different choices made in what constitutes a "smaller" release magnitude.

RESPONSE: Agreed; the reviewer's reading (below 100 kg h⁻¹) is correct. MANUSCRIPT CHANGES: (Sect. 3.1): a sentence was added to justify the threshold: 30 kg h⁻¹ is used for the probability-of-detection analysis in Fig. 7 because it spans the range over which the systems transition from non-detection to reliable detection - every non-zero release above 30 kg h⁻¹ that was measured was detected, so a wider range would add no information to the logistic fit. (Sect. 3.2): three sentences have been added at the start of the section. They define 'smaller releases' explicitly as releases below 100 kg h⁻¹ and justify the threshold: 100 kg h⁻¹ is used for the quantification regressions because it separates the bulk of the release distribution from the small number of very large releases while leaving enough estimates in each subset to support a regression.

7. 475: "Low wind speed conditions (below 1 m/s) are correlated with increased scatter and underestimation." The reader can look to Figure 9 and observe differences in r² across wind speed bins, however these are segmented from 0-2 m/s and the text claims "below 1 m/s". If the authors are attempting to comment on differences between the 2 m/s threshold and the displayed 0-2m/s binning, some additional statistics would be helpful to make this claim.

RESPONSE: Agreed - the text and the figure binning were inconsistent, and we do not wish to make a claim the figure does not support. MANUSCRIPT CHANGE (Sect. 3.3): the sentence now reads 'Low wind speed conditions (the lowest bin in Fig. 9, below 2 m s⁻¹) are correlated with increased scatter and underestimation', so that the threshold quoted in the text matches the 0-2 m s⁻¹ bin shown in Fig. 9. No additional statistics are claimed and no sub-2 m s⁻¹ threshold is asserted.

8. 478 and later: Here and after the term "stable wind conditions" may be confusing. At first read, I thought this was referring to atmospheric stability class, rather than the within-period variation. Recommend making this distinction clear, or using alternative language (i.e. consistent wind conditions). In this section is the CoV calculated over a specific time duration, or is this referring to the r² values in Figure 9? Some additional definitions and specificity would benefit this section.

RESPONSE: Agreed; the ambiguity was unintended. MANUSCRIPT CHANGES (Sect. 3.3 and Sect. 4.2.2): 'stable' has been replaced by 'consistent' throughout; CoV is now defined parenthetically at first use as the standard deviation of the wind speed divided by its mean over the duration of a release period; and an explicit sentence has been added stating that 'consistent' and 'variable' wind conditions refer to this within-period temporal variability of wind speed and direction, and not to atmospheric stability class.

9. 510 & 517: Be specific about "reproducing" and "on par" mean. Does this mean results demonstrating quantification accuracy with a similar order of magnitude? Within XXX%?

RESPONSE: Agreed; both terms were too vague. MANUSCRIPT CHANGES (Sect. 4.1). At line 510, 'reproducing' has been replaced by an explicit statement that, as in Liu et al. (2024), the tracer-correlation approach was the most accurate of the vehicle-based methods, with average quantification accuracy within 10% of the true release rate. At line 517, 'on par' has been replaced by quantitative comparisons: the AVIRIS-4 minimum detected emission rate of 5.5 kg CH₄ h⁻¹ is compared with AVIRIS-3, which detected plumes of ≥4 kg h⁻¹ but not ≤2.5 kg h⁻¹ in a controlled release at the University of California Riverside campus (Coleman et al., 2026), and with previous blinded controlled release tests of

hyperspectral aircraft (Sherwin et al., 2021; El Abbadi et al., 2024), in which aircraft consistently quantified releases above 10 kg CH₄ h⁻¹, with the smallest quantified plumes ranging from 2.9 to 33.8 kg CH₄ h⁻¹.

Technical Corrections:

1. 244: The authors use both British and American spellings of some words (i.e. modelling & modeling). Please be consistent throughout the manuscript

RESPONSE: Agreed. MANUSCRIPT CHANGE: the manuscript is predominantly in American spelling ('categorized', 'utilized', 'behavior', 'color'), so 'modelling' has been changed to 'modeling' in Sects. 2.3.2 and 2.3.3. Two further typographical errors were corrected at the same time: 'rigourously' to 'rigorously' and 'conccurent' to 'concurrent' (Sect. 4.2.2). Spellings inside reference titles are reproduced as published and have not been altered.

2. 329: It seems that the ddeq package has a corresponding GMD paper (<https://doi.org/10.5194/gmd-17-4773-2024>). Add this citation?

RESPONSE: Agreed, and thank you - the citation was in fact empty in the submitted manuscript, reading '(ddeq;)'. MANUSCRIPT CHANGE (Sect. 2.3.4): it now reads '(ddeq; Kuhlmann et al., 2024)' and the GMD paper has been added to the reference list: Kuhlmann, G., Koene, E., Meier, S., Santaren, D., Broquet, G., Chevallier, F., Hakkarainen, J., Nurmela, J., Amoros, L., Tamminen, J., and Brunner, D.: The ddeq Python library for point source quantification from remote sensing images (version 1.0), Geosci. Model Dev., 17, 4773-4789, <https://doi.org/10.5194/gmd-17-4773-2024>, 2024.

3. 480: please define CoV parenthetically.

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 3.3): CoV is now defined parenthetically at its first use in the wind analysis - see the response to comment 478 above.

Response to Anonymous Referee #3 - Comments posted on September 11, 2026

General Comments

The authors report on a experiment evaluating the ability of various sensors and approaches for the detection of methane plumes and estimation of emission rates. A particular strength is an evaluation of the important role of wind speed on the ability to detect methane and accuracy of emissions rates as expressed by r^2 and slope. The authors compared five approaches, three using mobile facilities and two using airborne sensors.

Specific to AMT

In terms of criteria specific by the journal, I would rate the paper as follows

1. Does the paper address relevant scientific questions within the scope of AMT?: Yes
2. Does the paper present novel concepts, ideas, tools, or data?: Some of the data, such as AVIRIS-4 is novel
3. Are substantial conclusions reached?: Yes.
4. Are the scientific methods and assumptions valid and clearly outlined?: No. More detail is required for many of the approaches.
5. Are the results sufficient to support the interpretations and conclusions?: No. The assessment of airborne systems is insufficient.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?: No. Details are lacking for a number of approaches tested.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?: No. The references are a major weakness
8. Does the title clearly reflect the contents of the paper?: No. The title should be revised (see below)
9. Does the abstract provide a concise and complete summary?: Yes. The abstract is sufficient
- 10) Is the overall presentation well structured and clear?: Yes.
- 11) Is the language fluent and precise?: Yes.
- 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?: Mixed. The paper is pretty short on equations and not every figure has unit defined.
- 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?: Yes. Figure 5 should be revised, and the airborne assessment is flawed by poor atmospheric conditions.
- 14) Are the number and quality of references appropriate?: No. The references are in need of improvement
- 15) Is the amount and quality of supplementary material appropriate?: Yes. The supplement is quite nice.

In terms of my own review, the paper appears similar to a number of double blind detection studies and is particularly important given the importance of methane. I particularly enjoyed the supplement and found

some of the results (e.g. high accuracies using tracers and a mobile lab, importance of wind) good contributions to the literature. However, there are a number of other areas where the paper has significant weaknesses. These include:

RESPONSE: We thank the reviewer for the detailed assessment against the AMT criteria. Each of the criteria marked 'No' is addressed in the corresponding correction below: criteria 4 and 6 (methods and reproducibility) by the added method descriptions in Sects. 2.3.3 and 2.3.5 and the clarified definitions in Sects. 3.2 and 3.3; criterion 5 (airborne assessment) by the revised aircraft discussion in Sect. 4.1, which states the cloud and precipitation limitations and the small number of usable measurements; criterion 7 (credit to related work) by the revised Introduction and ten additional references on airborne retrievals and validation; criterion 8 by the revised title; criterion 12 by the added units in the Fig. 9 caption; criterion 13 by the updated figures; and criterion 14 by the reference additions and corrections.

General Corrections

1. The title. I am not really sure what “From Research to Reality” means, nor why the focus is on “Academic methane measurements”. From Research to Operations might be a better description and I would remove the word “academic” which can be interpreted as pejorative.

RESPONSE: Agreed. MANUSCRIPT CHANGE: the title is now 'Single-blind controlled release testing of vehicle- and aircraft- based methane measurement systems at the TADI facility'. 'From research to reality' and 'academic' have both been removed. The term 'academic teams' is retained in the body text, where it is used descriptively to distinguish these participants from the commercial operators evaluated in the companion paper (McManemin et al., 2026) and carries no evaluative sense; we are happy to change it there as well if the reviewer prefers.

2. The Introduction is in need of significant revisions. As it is written, a reader would not be aware of how much actual work has gone into airborne estimation, nor validation of those estimates. Few of what I would consider to be the definitive papers on the subject are cited, with the authors focusing on a fairly narrow selection of articles that include almost all of the airborne work, including some with controlled releases and field measures to validate estimates. A selection that should be considered [appears in the Suggested References section below].

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 1): the Introduction now reflects the airborne retrieval and validation literature while remaining focused on the comparison of vehicle- and aircraft-based systems.

3. Some of the papers that are cited appear to have incomplete references, such as El Abbadi et al., which includes authors, a title, and date but not a publication venue. Wilson et al., 1976 also appears incomplete

RESPONSE: Agreed, and thank you for catching these. MANUSCRIPT CHANGES. (1) El Abbadi et al. erroneously cited the preprint and has been corrected to El Abbadi, S. H., Chen, Z., Burdeau, P. M., Rutherford, J. S., Chen, Y., Zhang, Z., Sherwin, E. D., and Brandt, A. R.: Technological maturity of aircraft-based methane sensing for greenhouse gas mitigation, *Environ. Sci. Technol.*, 58, 9591-9600, <https://doi.org/10.1021/acs.est.4c02439>, 2024, and the in-text citation year has been changed from 2023 to 2024 in both places where it appears. (2) Wilson et al. (1976) has been completed to Wilson, R. B., Start, G. E., Dickson, C. R., and Ricks, N. R.: Diffusion under low windspeed conditions near Oak Ridge, Tennessee, NOAA Technical Memorandum ERL ARL-61, National Oceanic and Atmospheric Administration, Air Resources Laboratory, 83 pp., 1976.

4. A number of the figures are in need of improvement. This includes Figure 5 (the left frame is very difficult to read), Figure 9 (What are the units on the y axis, estimated release rate)

RESPONSE: Agreed. MANUSCRIPT CHANGE: the Figure 9 caption now states in text that both axes are in kg CH₄ h⁻¹, with the metered (true) release rate on the x-axis and the participant-reported emission rate estimate on the y-axis. Updated the figure to include clearer y-axis labels and labels and units in the legends.

5. Many plumes are not really gaussian. This needs to be stated as a weakness. Given wind speed and direction that is often highly variable, the gaussian assumption is typically poor.

RESPONSE: Agreed; this was not stated clearly enough. MANUSCRIPT CHANGES Sect. 4.1: a new paragraph treats this as a limitation of the study, links it to the underestimation and increased scatter in the lowest wind speed bin of Fig. 9, and notes that the airborne mass-balance and dispersion-model approaches share a modified form of the same assumption.

6. Experimental design. There are a number of issues regarding measurement conditions that complicate the results. These include

- a. Cloud cover. Poor atmospheric conditions seriously hampered the ability of the authors to truly assess the performance of airborne systems, with one system having only one successful retrieval, and the other (AVIRIS-4) only small number. Given the highly successful campaigns described for California (Duren et al., 2019. California's Super Emitters, Nature, the US, Sherwin et al, 2024, US oil and gas system emissions from nearly one million aerial site measurements, Nature, and landfills (with validation), Cusworth et al., 2024, Science, the assessments here are not sufficient to truly evaluate those systems.

RESPONSE: Agreed, and we accept this limitation. MANUSCRIPT CHANGE (Sect. 4.1): the aircraft discussion now states that only a limited number of AVIRIS-4 measurements and a single FAAM measurement could be assessed, that both operators filtered substantial portions of potential measurements because of clouds and precipitation, and that these conditions affected the remote sensing participants in all four weeks. FAAM's detection limit could not be assessed. The text notes that testing under more favorable conditions has achieved higher acquisition yields.

- b. In assessing the airborne results, more needs to be said about the surface composition of the area. For example, asphalt surfaces tend to be confusers even for matched filters, and the accuracy varies with surface albedo (See Ayasse et al., 2018).

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 4.1): surface properties are now discussed as a possible contributor to the AVIRIS-4 retrieval performance (Ayasse et al., 2018; Foote et al., 2021). The text notes that the imaged scene over TADI contains vegetated, bare and built surfaces, and that stratifying performance by surface type would be a valuable addition to future controlled release tests.

- c. The mobile results are very interesting, especially the one using the tracers. However, it needs to be pointed out that those systems need to be guided to a potential source, especially if a tracer has to be released at the potential source. A vast majority of the sources will not be found using that approach.

RESPONSE: Agreed. MANUSCRIPT CHANGE (Sect. 4.1): a new paragraph states that the tracer gas dispersion method requires a tracer to be released at or close to the source and therefore presupposes that the source has already been located and is physically accessible; that in this experiment the emission was known to originate from within a small, defined platform so the tracer could be placed near its centre; and that in operational use the method is a quantification tool applied to sources already found by other means, rather than a survey tool

capable of discovering unknown emitters across a large area; the same consideration applies, to a lesser degree, to all vehicle-based approaches evaluated here, which require public road access downwind of the source.

I understand these types of studies can be difficult, but the assessment of the airborne systems is flawed because of the environmental conditions during acquisition. This needs to be carefully considered in any discussion.

RESPONSE: Accepted. See the revised Discussion section.

Specific Corrections

1. Section 1: See above. The entire introduction is in need of significant revision to better reflect the state of the science for methane retrievals, especially using airborne and spaceborne systems.
RESPONSE: See the response to General Correction 2 above. The Introduction has been substantially revised with a new paragraph on airborne retrievals and their validation, and new verified references have been added.
2. Line 38: I would add a “the” to “the Technical... “
RESPONSE: Corrected. MANUSCRIPT CHANGE (Abstract): now reads 'Vehicle-based teams from the Technical University of Denmark'.
3. Line 60. IPCC 2023 is missing from the references
RESPONSE: Corrected. MANUSCRIPT CHANGE: Updated to Forster et al. reference.
4. Line 64. This statement needs a reference to support it,
RESPONSE: Corrected. MANUSCRIPT CHANGE (Sect. 1): ' IEA (2022), already in the reference list, reports the value of methane lost from the energy sector. Added two more references on methane's impact on human health.
5. Line 71. Revise to read “making quantifying methane emissions accurately a critical”
RESPONSE: Corrected exactly as suggested. MANUSCRIPT CHANGE (Sect. 1): now reads 'making quantifying methane emissions accurately a critical challenge for the O&G industry.'
6. Line 73. Commonplace should be followed with citations. See some of the references above.
RESPONSE: Corrected. MANUSCRIPT CHANGE (Sect. 1): Added references
7. Line 175. Table 2 is cited twice
RESPONSE: Corrected. MANUSCRIPT CHANGE (Sect. 2.3): we interpreted this comment as referring to the sentence at this line duplicating the content of the Table 2 caption immediately below it. The sentence has been shortened to 'The following sections describe the sensor technology, systems, and measurement procedures of the participating academic teams, which are summarized in Table 2.' If the reviewer meant a different duplication, we would be grateful for clarification.
8. Line 220. See comment on Gaussian plumes
RESPONSE: See the response to General Correction 5 above. MANUSCRIPT CHANGE (Sect. 2.3.2): the Gaussian assumption and its limitations under the conditions of this campaign are discussed more fully in a new paragraph in Sect. 4.1.
9. Line 269. There is an error here.
RESPONSE: Corrected. MANUSCRIPT CHANGE (Sect. 2.3.3): the Picarro G2311-f precision specification was typed in the Symbol font, in which the 'less than or equal to' glyph is encoded as the character '£'; it consequently rendered as a pound sign in the submitted PDF. It has been replaced with a true '<=' character in the body font, so the specification now reads 'CH₄ precision (1 sigma, 10 Hz) <= 3 ppb'. If the reviewer intended a different error at this line, we would be grateful for clarification.
10. Line 302. There is an excess “for” here. Revise to read “members were converging”

RESPONSE: Corrected exactly as suggested. MANUSCRIPT CHANGE (Sect. 2.3.3): now reads 'when the estimates from two members were converging'.

11. Line 354. Please use metric. Even in the US metric units are used in most scientific papers.

RESPONSE: Corrected. MANUSCRIPT CHANGES (Sects. 2.3.4 and 2.3.5): flight altitudes are now given as 'approximately 1000 and 1280 m (3300 and 4200 feet) above ground level (AGL)'; the minimum flight level is now 'at least 150 m (500 feet) above ground level'; and the orbital survey radius is now '3.7 km (2 nautical miles)'.

12. Line 358. Revise to read "it is acknowledged that"

RESPONSE: Corrected exactly as suggested. MANUSCRIPT CHANGE (Sect. 2.3.5): now reads 'it is acknowledged that this methodology'.

13. Discussion. The discussion section needs to be revised to better acknowledge prior work on this subject (e.g. Thorpe et al., 2016) and better discuss issues with the design of the study, for example, which place significant constraints on airborne acquisitions due to clouds. Many large emissions sources are not as limited by clouds.

14. RESPONSE: Agreed. MANUSCRIPT CHANGES (Sect. 4.1): the aircraft discussion has been revised. It now places the AVIRIS-4 result against previous controlled release tests of airborne systems (Thorpe et al., 2016; Sherwin et al., 2021; El Abbadi et al., 2024; Coleman et al., 2026), states the weather constraints on the airborne acquisitions and the small number of usable measurements, discusses surface properties (Ayasse et al., 2018; Foote et al., 2021), and states that many large emission sources occur in arid and sunny environments.

15. I did like Section 4.2.3. Background concentrations are a serious issue. For some regions, such as the Los Angeles Basin, they are remarkably high due to numerous seep sources. I also liked Section 4.2.2 (Although it is not clear why this section comes after 4.2.3). Wind is a very serious issue and virtually all of the airborne and spaceborne approaches are highly dependent on accurate estimates of wind speeds. Emission rates can only be retrieved if wind speed is accurately determined, and coarse spatial resolution data sets such as ERA5 are clearly insufficient.

RESPONSE: Thank you - the subsection numbering was simply in error, which is why 4.2.2 appeared after 4.2.3. MANUSCRIPT CHANGE: 'Background concentration calculation issues' has been renumbered from 4.2.3 to 4.2.1, so the subsections now read 4.2.1 (background concentrations) followed by 4.2.2 (wind measurement and low wind speed challenges), in the order in which they appear. A related numbering error has also been corrected in Sect. 2: the subsections under the heading '2.3 Technology descriptions' were numbered 2.2.1 to 2.2.5 and are now 2.3.1 to 2.3.5.

Suggested References:

RESPONSE: All the following references have been added and cited in the text – either in the introduction or in the discussion section. We have not cited the spaceborne literature, in order to keep the Introduction focused on the platform classes evaluated in this study. We are happy to add specific references, including the review by Jacob et al. (2022), if the reviewer considers this insufficient.

Controlled release and field validation

Thorpe et al., 2016, Mapping methane concentrations from a controlled release experiment using the next generation airborne visible/infrared imaging spectrometer (AVIRIS-NG), Remote Sensing of Environment, 179, pgs 104-115.

Given the subject matter, I do not understand why this paper was not cited.

Ayasse AK, Cusworth DH, Howell K, O'Neill K, Conrad BM, Johnson MR, Heckler J, Asner GP, Duren R. 2024, Probability of Detection and Multi-Sensor Persistence of Methane Emissions from Coincident Airborne and Satellite Observations. *Environ Sci Technol.* 2024 Dec 10;58(49):21536-21544.

Cusworth *et al.* 2024, Quantifying methane emissions from United States landfills. *Science* **383**,1499-1504(2024).DOI:10.11

Methods for airborne retrievals

DOAS (most accurate)

Thorpe et al., 2017, Airborne DOAS retrievals of methane, carbon dioxide, and water vapor concentrations at high spatial resolution: application to AVIRIS-NG, *Atmos. Meas. Tech.*, 10, 3833–3850

Cluster-tuned Matched Filters

Thorpe et al., 2013, High resolution mapping of methane emissions from marine and terrestrial sources using a Cluster-Tuned Matched Filter technique and imaging spectrometry, *Remote Sensing of Environment*, Volume 134,2013,Pages 305-318, ISSN 0034-4257,

Albedo corrected retrievals

Foote et al., 2021; ,Impact of scene-specific enhancement spectra on matched filter greenhouse gas retrievals from imaging spectroscopy, *Remote Sensing of Environment*, Volume 264,2021,

Real-time plume detection

Thompson et al., 2015, Real-time remote detection and measurement for airborne imaging spectroscopy: a case study with methane, *Atmos. Meas. Tech.*, 8, 4383–4397, <https://doi.org/10.5194/amt-8-4383-2015>, 2015.

Sensitivity analysis

Ayasse et al., 2018, Evaluating the effects of surface properties on methane retrievals using a synthetic airborne visible/infrared imaging spectrometer next generation (AVIRIS-NG) image, *Remote Sensing of Environment*, Volume 215, 2018, Pages 386-397,

Review articles and high profile articles on airborne retrievals

Review article: Spaceborne retrievals

Jacob, D. J., Varon, D. J., Cusworth, D. H., Dennison, P. E., Frankenberg, C., Gautam, R., Guanter, L., Kelley, J., McKeever, J., Ott, L. E., Poulter, B., Qu, Z., Thorpe, A. K., Worden, J. R., and Duren, R. M.2022, Quantifying methane emissions from the global scale down to point sources using satellite observations of atmospheric methane, *Atmos. Chem. Phys.*, 22, 9617–9646, <https://doi.org/10.5194/acp-22-9617-2022>

Sherwin, E.D., Rutherford, J.S., Zhang, Z. *et al.* 2024, US oil and gas system emissions from nearly one million aerial site measurements. *Nature* **627**, 328–334. <https://doi.org/10.1038/s41586-024-07117-5>

Daniel H. Cusworth *et al.* 2024, Quantifying methane emissions from United States landfills. *Science***383**,1499-1504(2024).DOI:10.11

There is also a wealth of spaceborne retrievals, although those might be a bit off topic. Finally, airborne methane retrievals date all the way back to 2010, although much of that work did not include accuracy assessment.