Reviewer 1

We thank the reviewer for the helpful comments and have addressed them below and in the updated manuscript. Please refer to the updated manuscript where additions are shown in blue and red indicates text that will be removed. Section 2 has been largely rewritten. We include the red/blue highlighting where appropriate in that section, but some rearrangements are not noted by the highlighting.

Overall assessment. This paper presents a new Gaussian plume method to infer the normalized emission rate Q/u from point sources using remote sensing observations of plumes, also including sensitivity/uncertainty (S/U) information for individual pixels. The method is a valuable addition to current methods, the use of S/U is an important new advance, and the presentation is clear. I have two general concerns expressed in my specific comments that can easily be corrected to the extent that the authors deem appropriate: **(1)** the authors overstate the success of their method over the IME method, which they also seem to confuse with the cross-sectional flux method; **(2)** the references are very JPL-centric and ignore or shortchange a lot of literature on inferring emissions from point sources including by Gaussian plume methods.

Specific comments

1. Abstract is a fair representation of the paper but should mention that the method uses a Gaussian plume model. Also, it seems to me that the "arbitrary plume boundaries" also apply to SPIIRE since the plume is manually defined.

We have updated the abstract.

Regarding the use of the plume boundaries, we have clarified the text and the figures. The essential point is that SPIIRE only loosely uses the manually delineated plumes. In each figure showing a plume, we show all the pixels used in the retrieval (except the right size of Figure 6 which is zoomed in). We have updated the text to better explain this and the figures so that the plume boundaries are no longer displayed to help reduce this confusion.

2. Introduction and elsewhere: references are exceedingly JPL-centric. Frankenberg2016 needs to be cited as the original reference on the IME method. Jacob2022 gives a review of point source inference methods with a helpful summary figure. Maasakkers has several papers on point source retrievals from TROPOMI. Jongaramrungruang2022, Bruno2024, and others have applied machine learning to delineate plumes and infer emissions. Fioletov and others have

extensively published on Gaussian plume inference of NOx and SO2 point sources from OMI and other UV/Vis satellite sensors.

We updated the manuscript to include these references.

3. Lines 23-24, 27-28: IME is presented here in its crudest implementation. Most IME methods starting with Varon2018 use LES to parameterize the effective wind speed and a length scale as sqrt(area) to allow for cross-wind dispersion.

We don't intend to disparage IME methods and we want to present them fairly (we are also the creators of the AVIRIS and EMIT IME method). We refer to the use of effective wind speed in line 33 and the sqrt(area) method of Varon in line 30 (line numbers from originally submitted manuscript).

4. Line 39: previous work in particular Varon2018 should be acknowledged as finding that Gaussian plume models are inadequate for instantaneous methane plumes because these plumes are too small and instantaneous so that they don't achieve the statistically representative sampling of small-scale eddies implicit in the Gaussian plume formulation. The authors should explain why they disagree.

We do not disagree with Varon2018. In fact, we added the ability for the plume model to vary in the crosswind direction specifically to address the type of issues that Varon2018 describes. We hope that the comparison against the controlled release experimental data shown in the manuscript demonstrates that we have overcome this challenge.

5. Line 40: why "yet"?

Thank you for pointing this out. We fixed it.

6. Line 59: but what if the plume isn't Gaussian? Is there a metric for the quality of the fit? It seems that the method has no prior information for the spread parameters, which seems odd. What is the safeguard that they remain within physical bounds?

The cost function in Eq. 5 does provide a metric of quality of fit, although we acknowledge that error remains in line 157 in the original manuscript. We have also added the fraction of controlled release experiment overpasses whose emission rates are within their uncertainty of the truth. This analysis indicates that the additional uncertainty due to model-data mismatch is not large. We use a bounded optimizer with bounds chosen to keep the parameters within a reasonable range based on prior work with Gaussian plume models (Carrascal 1993, for example).

7. Lines 72-73: there is no problem in principle with applying the IME or the cross-sectional flux method to an area source.

We agree that these methods can work when the plume is large compared the area source. In many cases, however, the apparent source area is relatively large compared to the plume length, as in the plume in Figure 8. The essential point for IME and cross-sectional flux is that conservation of mass hold at all downwind points. This is not true over the area source but may be true downwind.

8. Line 115: I don't know what uncorrected uncertainty means. The quotation marks don't help.

We removed the offending text as it is unnecessary.

9. Line 134, also 166-167: Varon2018 showed that S/N >1 was an objective criterion for delineating the plumes (including in the IME method, so this is not "arbitrary"), is this wrong? It seems odd to include pixels that introduce more noise than signal.

Varon2018's criterion is not necessarily wrong, it is just not relevant in SPIIRE's probabilistic formulation: pixels with S/N < 1 may not have much gas enhancement, but they still provide information to the optimizer regarding the shape of the plume. We developed SPIIRE's probabilistic formulation to specifically address this very issue. Figure 3 in Varon2018 demonstrates the issue clearly: S/N > 1 yields many false alarms that subsequent filtering removes. Pixels with S/N > 1 that are far from the plume are obvious false alarms. However, those near the plume may not be so obvious and other choices of the filtering parameters may yield different decisions. (It is for this reason that we use the term "arbitrary".) SPIIRE, on the other hand, requires no such determination. The model predicts a signal level in each pixel that is compared to the data without need for any prior determination of whether the pixel is in or out of the plume.

10. Line 160: "Traditional IME" in this section actually seems to describe the cross-sectional flux method. There's a lot of mention of "traditional" or "simple" implementations of the IME method, how about more advanced?

The purpose of Section 2.3 was to try to formulate the IME method into a maximum likelihood formulation in order to contrast it with SPIIRE. Based on this question and comments by Reviewer 2, we have removed it as confusing and unnecessary.

We agree that our use of "traditional" and "simple" are superfluous and could be

interpreted as pejorative, so we have removed them.

11. Lines 194-195: choosing to simulate Gaussian plumes would seem to force a successful outcome, because indeed a key question is whether small instantaneous plumes can be represented as Gaussian. The controlled-release experiments were also conducted for steady moderate winds and flat surfaces where a Gaussian plume model would shine. Not all plumes are like that.

We have clarified the text to better explain the intent of the simulations in Section 3.1. The purpose is to show that the 2D MLE formulation is better able to reduce the detrimental effects of sensor noise than is IME when the assumptions of both methods are true. Section 3.2 demonstrates that SPIIRE is still accurate even when assumptions about the plume shape are not strictly met. For the many plumes that are non-Gaussian, we attempt to show a path forward with the section on Distributed Sources. We added "We hope that future work can expand on this idea and demonstrate good performance over a large class of amorphous plumes commonly observed in plume databases" at the end of this section to help clarify this point.

12. Figures showing plumes need spatial dimension information – like a legend scale. Axis labels seem to be pixel ID numbers (?) which is not very useful.

Agreed, we have updated the figures to use physical units.

13. Line 241: I don't understand why "…the validation shown here using AVIRIS-3 and EMIT controlled release data is more likely to apply to the full EMIT database". It seems to me that the controlled release conditions were particularly well suited for a Gaussian plume treatment in a way that other conditions would not be.

We have clarified the text in the last sentence before Section 3.3 to indicate that the validation is intended to mean for other point source plumes like those in the controlled releases.

On a related note, we have included additional results from the AVIRIS-3 and EMIT controlled release experiment using the IME approach (see new Figure 4 and Figure 6) for comparison to emission rates derived from the SPIIRE algorithm.

14. Line 306: I don't understand "the 2D model is defined over an infinite geographic area, removing the boundary as a source of error". Don't you still do a manual delineation of the plume?

The manual delineation is only used to guide the determination of the retrieval region and we include a buffer around the boundary to avoid biasing the retrieval. (Please see our responses to comments #1 and #9.) We have removed the manual delineation shown in the figures and rewritten Section 2.2 to make this more clear.

15. Line 310: “turbulent wind effects that can distort the observed plume shapes and degrade the accuracy of traditional IME-based approaches” Again I’m not sure what “traditional” refers too, but the standard LES-calibrated approaches specifically factor in the effect of turbulence.

We have removed “traditional” and add that we recognize the intent of the LES-based effective wind speed.

16. Line 316: “The algorithms that detect pixels to include in the IME calculation may fail to incorporate these holes…” I don’t see why they would fail to.

Figure 7 demonstrates that albedo variation can create the appearance of “holes” in the gas enhancement data that may fail to satisfy the S/N thresholds. Although the sensitivity correction can correct this bias, the correction comes with a significant noise inflation [Fahlen 2024]. SPIIRE’s probabilistic formulation incorporates this effect while downweighting noisy pixels in a statisticall- sensible way that IME cannot.

Reviewer 2

We appreciate the reviewer comments and have addressed them below and in the updated manuscript. Please refer to the updated manuscript where additions are shown in blue and red indicates text that will be removed. Section 2 has been largely rewritten. We include the red/blue highlighting where appropriate in that section, but some rearrangements are not noted by the highlighting.

The manuscript introduces a novel method to quantify methane emissions. Not only individual point source emissions, but also overlapping plumes and diffuse emissions. Results show good agreement with controlled releases for the AVIRIS-3 instrument, which supports the validity of this method. The removal of user-defined parameters such as those needed in other quantification strategies - i.e. the IME method - allows for a more unambiguous methodology pipeline that could further benefit the automatization from ML models. The flexibility of the method to adapt to a large variety of plume shapes and distributions is remarkable and the corrections after accounting for the sensitivity metric seem very important. I want to congratulate the authors for this work, which I consider that introduces an important added-value to the field. However, before publication there are important (and not so important) comments that the authors should address before I recommend this work for publication.

Abstract

- Please, add some information about whether SPIIRE outperforms state-of-the-art methods (IME) for quantification.

We have added this information, including additional results from the AVIRIS-3 and EMIT controlled release experiment using the IME approach (see new Figure 4 and Figure 6) for direct comparison to emission rates derived from the SPIIRE algorithm.

Introduction

L19 - The concept of 'gas enhancement imagery' should be rephrase to reach a broader audience (express it more plainly). Please, include a little explanation of what you mean with this concept.

We have added this to the manuscript in the second paragraph of the Introduction.

L32 - The effective wind speed is not only accounting for errors from plume length. It is an empirical way for also accounting for other aspects such as the acquisition angular configuration (Gorroño et al., 2026), turbulence effects, plume mask criteria, etc. Please, take into account in the text the broader role of the effective wind speed.

We have added this to the manuscript in the third paragraph of the Introduction: "The effective wind speed is also used to compensate for turbulence, angular acquisition configuration (Gorroño et al., 2026), plume mask criteria and other similar effect."

L36 - Please take also into account the instrument-scene angular configuration (Gorroño et al., (2026).

We have added this to the manuscript in paragraph three of the Introduction: "Consequently, IME-based methods frequently depend on tuned parameters that can significantly influence emission estimates and may require adjustments for different instruments, platforms (such as airborne versus spaceborne), collection geometries (Gorroño et al., 2026) or environmental conditions.

L52 - What about those plumes that cannot be modeled as a Gaussian plume? Limitations should be explained later in the text.

We have added a paragraph describing limitation immediately before Section 3: "The principal limitations of SPIIRE are the challenge of estimating wind speed and the difficulty in fully quantifying the detrimental effects of applying SPIIRE to non-Gaussian plumes. The former is discussed below in Section 5.1. Regarding the latter, we suggest a path forward in Section 4.3 in which we generalize the model to adapt to apparently non-Gaussian plumes. Other limitations include the requirement for a parameterized downwind spreading model σ_y , the unknown 3D shape of the plume and the light's path through it (the 2 in Eq. 4) (Gorroño et al., 2026), and numerical difficulties associated with the optimization."

L55 - This could not be true. At this point, I think the author aims to remove the arbitrary and user-defined parameters to provide a more systematic basis for plume quantification. In this context, it is more objective. However, it is assumed that the emission can be described as a Gaussian plume, and this is not always true. Therefore, 'objective' is true within the author's proposed framework, but this limitation should be mentioned from the beginning.

We rephrased the text to remove "more objective" in paragraph 5 of the Introduction.

L58 - 'Highly accurate' seems something that must be proven. I'd talk about the advantages of this method later on the discussion, after showing the results.

We removed the "highly" as superfluous.

L61 – 'variations in wind velocity' - However, previously in the text, it was said that Gaussian plume models typically assume constant wind speed. Please, clarify.

We have changed "velocity" to "direction". The intent of the cross-wind flexibility is to adapt to apparent changes in wind direction.

L63 - Again, as 'highly accurate' (line 58), 'precise' is something that still has to be evaluated. Please, consider to limit the success of your method only after showing the results.

We have changed the manuscript to "Together, these enhancements yield quantitative models of observed plumes…"

L63 - What happens if the whole plume is affected by noisy, low-signal pixels? This should be later explained in the text.

Such plumes would likely not be detected in the first place, but supposing they were, SPIIRE would produce an estimate having higher uncertainty that is more likely to be corrupted by noise and surface artifacts. We have updated the text following Eq. 6 to better address this question.

L75 - I'm skeptical about this, since landfill (example of area source) pixels might be highly biased due to surface sensitivity/artifacts.

The use of the sensitivity and uncertainty will, to some extent, address this bias. Remaining artifacts may bias the results, but it seems likely that IME methods will also suffer. We have modified the text at the end of Section 4.3: "We hope that future work can expand on this idea and demonstrate good performance over a large class of amorphous plumes commonly observed in plume databases."

L79 - Again, 'excellent fits' is an appreciation of the methodology that should be acknowledge after showing the results.

We have removed the offending line.

L85 - I'd write 'further discussion and conclusions' because discussions go first

We have changed the manuscript.

Plume Model Inversion

L101 - d=1000 m. Why? This is then motivated from some reference?

We updated the text immediately after Eq. 2: "The choice of 1000 m follows Varon et al. (2018) but, based on the parameterization in Eq. 2, this choice is arbitrary and only for convenience in comparing the retrieved parameters a and b to previous work (Carrascal et al., 1993)."

L101 - Not sure what you mean with 'Pasquill-Gifford'. Please, clarify.

The comment refers to the Pasquill-Gifford atmospheric state models described in Seinfeld and Pandis (2016). We have clarified the text following Eq. 2: "Different authors find different values for parameters a, b, and d and often recommend that they change with wind speed or atmospheric state (like those of the Pasquill-Gifford atmospheric state models in, for example, Seinfeld and Pandis (2016))."

L102 - the spreading model we proposed... Adding 'we proposed' clarifies that the wind dependence of your selected spreading model does not exist.

We have added this to the manuscript after Eq. 2: "*Importantly, the spreading model we propose does not include an explicit dependence on the wind speed."*

L107 – 'concentration length' - Please, specify units.

We have added that ppm-m are the units of concentration length.

L108 - 'at hand'. What do you mean? Please, clarify.

We have rewritten Section 2.2 to be more clear.

Eq.3 - Isn't the integration of the term dependent of z (assuming 0 - surface- to infinity - TOA) equal to 1/2? If you need to integrate from -infinity to + infinity to obtain the integral = 1, please, clarify why.

Good catch. We have rewritten Section 2.1 immediately after Eq. 1 to include a discussion of this point by including a height for the origin and assuming that the vertical spread sigma_y is comparatively small. See the first paragraph of Section 2.2.

L111 - Please. clarify what you mean with ‘the data are noisy’. I'd rephrase it as sth like 'the model also needs to account for the uncertainty of the data'.

We have rewritten Section 2.2 to better explain.

L115 - Please, explain the concepts of sensitivity and uncertainty with further detail. It must be clear why you build the probability distribution function in that way.

We have rewritten Section 2.2 to clarify the purpose and value of the sensitivity and uncertainty in this work.

L121 - Maybe you should comment something about double plumes to emphasize the limitations of this assumption (Gorroño, 2026; Sanchez-Garcia, 2022)

We have added a comment and these references in the text following Eq. 4.

L129 - So, a previous step is to manually delineate the plume. This first involves identifying the plume (detection). Please, explain the whole process in a more step-to-step manner, so the reader can better understand the algorithm.

We have modified the first paragraph of Section 2.2 to: “We seek to estimate CQ by comparing the Gaussian plume model with the remote measurements. In the remainder of the paper, we assume that prior processing of the observations has estimated the concentration length ˆ li,j in ppm-m, where i, j are spatial pixel indices, and that potential plumes have been detected and queued for emission rate estimation. Since this data is a 2D overhead map of the gas concentration, the comparison requires adapting the 3D model to the 2D remote measurement...”.

We also clarified in the paragraph after Eq. 6 that the manual delineations are JPL’s standard delivered product for EMIT.

L133 - Then, you previously set a per-pixel noise threshold? This has not been previously explained. Please, clarify.

There is no noise threshold used in the method. All pixels in a region surrounding the manual delineation are used in the optimization. We have removed the "per-pixel threshold" and largely rewritten Section 2.2 to clarify the method.

L135 - This is not the emission rate

We have changed the text to say that "the wind-normalized emission rate".

L141 - This assumption only allows for variations in wind direction, not wind speed value. Allowing variation of direction, but not in the value is something consistent? Please, clarify.

We recognize that there is an inconsistency. The data provides a clear observation of changes in wind direction but very little leverage on the wind speed. We have adapted the model to accommodate the direction variation while leaving the effects of speed variation for future work. That this is reasonable is seen in the controlled release experimental results. We changed the manuscript to "variations in wind direction" in paragraph six of the Introduction. We note that SPIIRE was developed to be used with global point source datasets that forces us to rely on wind reanalysis data that does not have the spatial and temporal resolution needed to accommodate wind speed variation. This remains a topic for future work.

Section 2 - Please, consider a more structured explanation of the methodology.

We have rearranged and rewritten Section 2 to attempt a more coherent explanation.

L152 - Please, could you explain why this happens?

We have changed the text in the paragraph before Eq. 7 to explain: "In practice, we optimize the log likelihood to simplify the cost function and we estimate ln CQ and ln a to keep the numerical range of those parameters similar to that of the others."

L159 - This is very important. If the underestimation is quite large, we cannot rely on this methodology. Please, address this point.

We have updated the manuscript to include an empirical assessment of the uncertainty using the controlled release experimental data. We added a paragraph before Section 3.3

that describes the fraction of the overpasses whose estimated emission rate is within the uncertainty of the metered rate: "Section 2.2 noted that the uncertainty calculation in Eq. 7 and used in Figures 3 and 5 neglects the uncertainty due to model-data mismatch because it is difficult to quantify. However, the experimental results provide a means to empirically assess the degree to which Eq. 7 underestimates the true uncertainty, at least for the point-source plumes provided by the experiment. We find that the emission rates fall within their uncertainty range of the true value in 50%, 62%, and 80% of the overpasses at the AVIRIS Arizona, AVIRIS Wyoming, and EMIT Arizona sites, respectively. Since Eq. 7 represents a 1-sigma uncertainty, we would expect approximately 68% of the overpasses to be within their uncertainty of the true emission rate if the uncertainty were not biased. The results indicate that neglecting model-data mismatch in the uncertainty calculation does not significantly bias the resulting uncertainty estimate"

2.3. - Here the authors explain the IME method as some method where the user must first decide which is the downwind direction and then extract the corss-wind transects for the summatory. Typically, this isn't a requirement in the IME formulation. However, you could mention that this is somewhat equivalent to the IME formulation.

The intent of Section 2.3 was to attempt to put IME in the same kind of MLE formulation as SPIIRE to contrast the methods, but we recognize that it was more confusing that helpful. We have removed the section entirely to make the manuscript more concise and clear.

L174 - I think how the variables and their correspoding units are expressed is somewhat confusing. k1D must convert Q/u (kg/m) into ppm·m units. However, it is said that k1D converts the line concentration (which I expect to be li) into concentration length. However, the text says that the line concentration has units of kg/m2, which I think it's false.

We have removed this section.

Eq9. Please, explain with more detail how you obtained this expression. I expect this to be the part of the log-likelihood that is used for optimization in order to extract the the Q. This should be better explained. Moreover, you use the log-likelihood because it is simpler for optimization? This should also be explained.

We have removed this section.

Section 2. In general, I'd recommend the authors to invest more time in improving Section 2, since it is key to understand why this method works.

We have rewritten Section 2 to better explain the method.

L175. How is the fetch length determined. Please, explain.

We have removed this section.

Results

L185 – ‘other gases’ - However, there might be exceptions. For instance, ammonia and ethylene plumes, which can also be detected with EMIT-like instruments, are somewhat different from methane plumes. These 2 gases exhibit a much lower atmospheric lifetime and they react more rapidly. This also happens with NO2. Despite of this difference, is this method still valid?

We have updated the text in the first paragraph of the Results section to indicate that the method applies to other atmospherically-stable gases. We also noted in the discussion section that “The Gaussian model could also be generalized to model gases with short atmospheric lifetimes.”

L193 - What do you mean with superior noise performance? Please, explain. In Eq. 7, it was previously commented that the uncertainty of Q following this model is underestimated, which I think is the major flaw from this work.

Section 3.1’s purpose is to demonstrate that SPIIRE has reduced uncertainty due to measurement noise than IME does when the plume satisfies the assumptions of both methods. This is apparent in the narrower vertical spread in Figure 1b relative to 1a. Section 3.1 does not address the overall uncertainty in the method. We have clarified this point in the text in Section 3.1and addressed the uncertainty underestimation elsewhere (see comment above regarding L151). Please see the multiple additions in Section 3.1 that we hope clarify the intent.

L195 - Please, use the units of CQ and also justify why you selected this range. I guess this range was selected since it satisfied emission rates that were enough to detect the related emision from EMIT.

We have added the units to the text and explained the range. You are correct that we chose the range to span typical emission rates when including the range of typical wind speeds. We added “The simulated CQ are uniformly distributed between 200 and 3000 kg/hr per m/s to span a typical range of emission rates and wind speeds observed with EMIT” in section 3.1 to clarify.

L197 - How did you inject the simulations into the retrieval data? Simulations are first introduced into L1 data end then the retrieval is carried out? Or are they simply added to the retrieval? If the latter, then you should comment the limitations and risks of doing this.

The simulations are performed following Figure 8 of Fahlen 2024. We have updated the text to clarify.

L199 - As it is mentioned in the text, this improvement in CQ characterization must be interpreted carefully. In my opinion, this comparison should have been done with plumes that are not coming from a Gaussian model, since it is exactly the model that SPIIRE assumes. In contrast, there are publicly available WRF-LES simulations (see Gorroño, 2026) with plumes with more different shapes, accounting for realistic turbulence, changing winds, etc. Repeating this comparison with this kind of plumes would add more confidence in the improvement of SPIIRE in reference to the IME method.

We agree that this might be helpful, but LES simulations fail to include the albedo and atmospheric effects characterized by the sensitivity and per-pixel uncertainty and they also do not include realistic background variation. Due to these limitations we chose to demonstrate the noise resilience obtained from SPIIRE's probabilistic framework in the contrived simulations of Section 3.1 and then move directly to real plumes from the controlled release experiments. Notably, the Gaussian plumes in our simulation are consistent with the transport assumptions of both IME and SPIIRE, so they do not unduly penalize the IME method.

Fig1. What can you say about the dispersion of values in reference to the 1:1 line? Why is not perfect for SPIIRE? Please, extend the analysis in this regard.

The dispersion is the uncertainty due to the fact that the plumes are injected into real, noisy data. The purpose of the figure is to demonstrate that SPIIRE's probabilistic formulation, including the sensitivity and uncertainty, leads to better accuracy than IME under the condition that the plumes satisfy the assumptions of both methods. If the emission rate were retrieved from the simulated plumes without injecting them into real data then both IME and SPIIRE would fall directly on the 1:1 line. We have updated Section 3.1 of the manuscript to better reflect our intent and to better explain the result.

L119 - Why only 30 s? Is this time enough to be representative of the plume that is captured by the instrument? Please, clarify.

We chose 30 seconds in an attempt to balance sufficient wind smoothing with the approximate length of the various plumes. For example, in Figure 2, 30 s at 4 m/s wind makes an approximately 120 m plume, which in this case is longer than the observed plume, but others have lower winds and/or longer/shorter plumes. We felt that 30 seconds was a reasonable choice given this diversity. We have clarified this in the text in the third paragraph of Section 3.2: "We chose 30 seconds because it represents a reasonable propagation time for the range of plumes included in experiment while providing sufficient smoothing."

* There is also the CSF method. Does SPIIRE also outperform this method? I think this is important because, if so, it would add a very nice added-value.

We have not compared SPIIRE against the CSF method but we note that published results using CSF have found similar or slightly worse performance relative to IME methods. One of the limitations of CSF is that the emission rate estimate can vary depending on where the cross section is taken. One of the reasons we designed SPIIRE to include pixels beyond the end of visible extent of the plume was to address this limitation.*

**For example: Varon2018 "The cross-sectional flux method has slightly larger errors (0.07–0.26 t h %–12 %) but a simpler physical basis" and "The LIME estimates are also aligned with the state-of-the-art IME emission estimates, which are calculated as byproducts in the LIME emission estimation process". Janne Hakkarainen, Iolanda Ialongo, Daniel J. Varon, Gerrit Kuhlmann, Maarten C. Krol, Remote Sensing of Environment, Volume 319, 2025, 114623, ISSN 0034-4257, https://doi.org/10.1016/j.rse.2025.*

L227 - This is a very important point. One of the assumptions is the steady-state which implies constant Q and u, right? These conditions are probably not met in real cases. Please, address this point.

We removed the line "We note that SPIIRE's performance may not be representative in this case because the plumes do not satisfy the steady-state assumption" as it is an incorrect holdover from a previous draft. This would have been a problem had we not changed the cost function as described in the subsequent sentences. We believe that the lack of steady state in general falls into the general category of model-data mismatch addressed elsewhere.

L239 - One of the point of this paper is the improvement of SPIIRE in reference to the IME method. However, the comparison with the EMIT instrument has not been carried out. The authors explain that this cannot be done since the controlled releases were not enough to tuned the IME parameters for EMIT. However, there are other ways of tuning this parameters such as simulations. In this regard, I also saw in the CRs paper that the JPL team submitted emission quantification based on Simple IME with EMIT for this very same emissions. It'd be nice to compare with these values.

We have added JPL IME method results to the paper for comparison (see new Figures 4 and 6).

L246 - I'm not sure whether you can confirm this. You can say that the sensitivity correction yields a plume that can be fit into a Gaussian plume that assumes constant Q. However, this is only a possibility. Please, clarify.

We have changed the text to say that the plumes are modeled well by a Gaussian plume, suggesting that surface albedo effects are sufficient to explain the apparent plume shapes without further assuming intermittency or turbulent winds.

In the first paragraph of Section 3.3, we removed "Instead, the 2D Gaussian plume model adapted to the data and combined with the sensitivity indicates that the emission source is constant in time, with the apparent intermittency caused by background albedo effects captured by the sensitivity" and replaced it with "We find instead that these plumes can be well modeled with a steady-state Gaussian plume that includes albedo effects through the sensitivity, suggesting that in some cases intermittency or turbulence need not be considered to understand complicated plume shapes."

L249 - Please, better refer to Q instead of CQ.

We have changed the text.

L251 - 'excellent' might be too much since the transects from Figure 5 do not follow a perfect match. Again, accounting for the uncertainty due to the mismatch is very important here.

We have revised the text to "good" agreement in the first paragraph of Section 3.3: "In this case the model-data agreement is good, suggesting SPIIRE's estimate is likely a more accurate representation of the true emission rate."

3.3. In this regard, it is very important to first contrain the uncertainty of your emission estimate (see Eq.7). Once you can constrain the uncertainty related to the mismatch you would be able to really analyze the quantification using SPIIRE.

Please see the additions described in response to your question regarding L151 above.

* Please, at some point clarify all the limitations of SPIIRE.

We added a paragraph with describing limitations immediately before Section 2.3.

* Consider including somewhere in the text, what happens when there is overlap of plume and artifact, or a surrounding artifact.

We added a comment regarding artifacts in the paragraph following Eq. 6: "We limit the buffer to 10 pixels both to limit computational time but also to reduce the likelihood that the plume model be degraded by inadvertently including scene artifacts that may be mistaken as true gas enhancements."

4.3. The resulting plume from the model seems to match the original retrieval. However, I'm skeptical whether this methodology can be trusted. Using each landfill pixel as the origin of a Gaussian plume and then do the combined inversion might provide an unstable result, meaning that e.g. including/removing one pixel as one of the sources would yield a totally different result. Maybe you can use some reference data: Madrid (MEDUSA project) or Romania landfills? If it is not feasible to access real reference data from landfills, you might do an analysis about the stability of your method by removing/adding pixels and see the stability of your result, or similar. As you suggest, IME-based quantification (or even CSF) is not really good for landfills, so this method could be a very good alternative. Moreover, what can you say about the potential artifacts arising from the landfill surface?

We agree that further work is needed to better test this generalization of the Gaussian plume model. We intended this section to show a potential path forward and indicate a valuable next step for future work. To help clarify the intent of the Distrbuted Sources section, we add the following sentence to the end of the section: "Although it is possible that this method is susceptible to artifacts not compensated for with the sensitivity, we hope that future work can expand on this idea and demonstrate good performance over a

large class of amorphous plumes commonly observed in plume databases." We agree that this method may be polluted by artifacts, but SPIIRE's inclusion of the sensitivity to correct the bias associated with them is an improvement over IME methods. In future work, we will look for opportunities to validate EMIT derived emission estimates from landfills with independent, ground based estimates when available.