Author response to Referee #1 (RC1)

Radiative Constraints on VIIRS Nighttime Detectability of Lower-Tropospheric Methane (egusphere-2026-4225)

We thank Referee #1 for a careful, constructive review. Each comment is addressed below.

1. General comments

Comment 1.1

The remote detection of methane leaks or releases during the nighttime is an important topic, as the majority of airborne and space-based sensors operate in the SWIR and therefore require solar illumination. Hence we have very limited ability for nighttime methane measurement. This is an unfortunate gap not just because methane emissions are episodic and we are not looking more than half the time, but also because the lower boundary layer and lower turbulent mixing significantly changes dispersal dynamics. The MWIR (~3.3-3.4um) methane absorption band for remote sensing is not widely studied, and the competing effects of strong absorption but low ground thermal radiance (relative to LWIR) make its utility unclear. So the authors' investigation of whether an existing space asset (VIIRS), specifically the M12 filter band which touches only the low-line-strength edge of the band, could provide utility, even though the result is essentially a negative one, is a useful contribution to the community. Incorporating measured CH4 profiles and satellite-derived ground scene variability adds important realism to the modeling.

Author response: We thank the referee for this assessment and agree with its framing: the study tests whether an existing operational asset can provide nighttime methane information, and the answer is largely negative. We have kept that framing throughout. The referee's observation that measured profiles and satellite-derived scene variability add realism is appreciated; in the revision this is strengthened by a corrected and expanded treatment of the CAMEL-derived emissivity term (Sect. 2.4) and by a new SNR versus scale-factor figure (Fig. 8) that makes the distance to the detection threshold explicit.

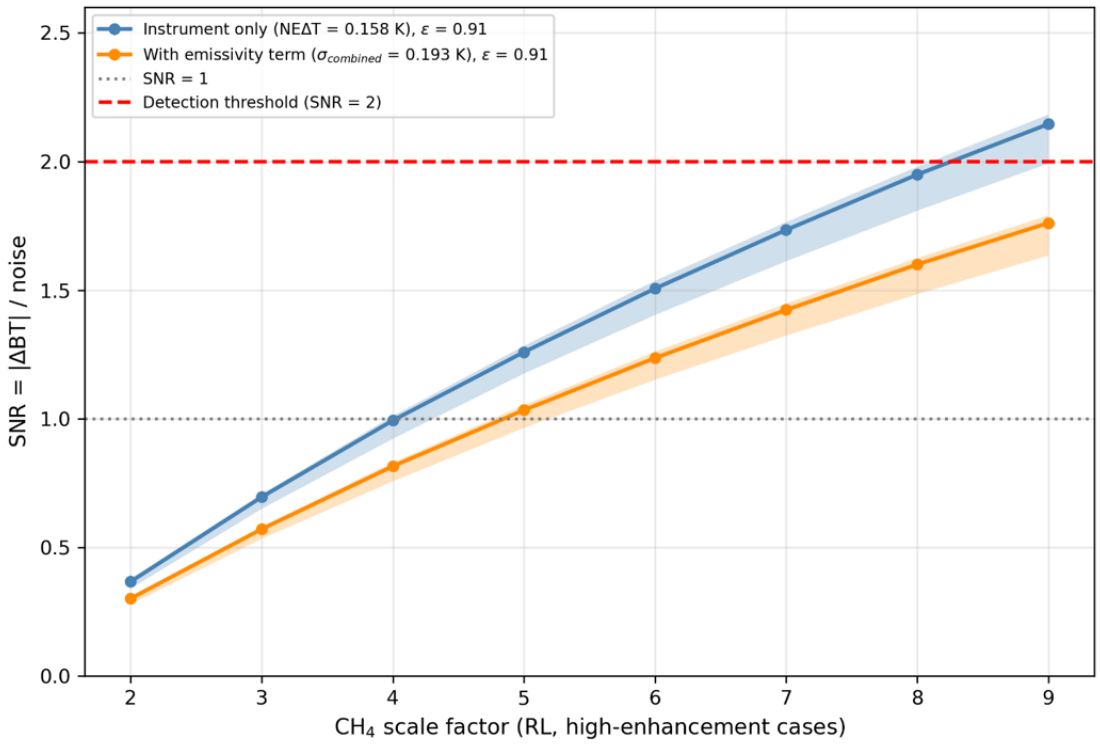


Figure 8. Residual-layer SNR versus CH4 scale factor (2-9x) for the instrument-only noise floor (NEdT = 0.158 K) and the operational noise floor including surface-emissivity variability (sigma_combined = 0.193 K, 3x3 window). Lines show eps = 0.91 (CAMEL-matched); shading spans eps = 0.89-0.99. The dotted line marks SNR = 1 and the dashed line marks the detection threshold (SNR = 2). With the emissivity term included, no case reaches SNR = 2.

Changes in the manuscript:

- **Sect. 2.4, L. 335-350:** CAMEL emissivity treatment clarified and numbers corrected (sigma_emis 0.185 K; sigma_combined 0.193 K for 3x3).
- **Fig. 8, L. 350-355:** New SNR versus CH4 scale factor figure.

Comment 1.2

In addition to references already included, the article comparing MWIR and LWIR methane detection from an airborne hyperspectral system (SEBASS), Scafutto and Filho, Remote Sensing 10, p.1237 (2018) could be added for background.

Author response: We thank the referee and have added the suggested reference. It is cited where MWIR methane detection is first discussed, in the context of airborne SEBASS hyperspectral data comparing MWIR with LWIR detection.

Changes in the manuscript:

- **L. 42-44:** Added: "Airborne hyperspectral SEBASS data have been used to detect methane plumes in the MWIR (3-5 um) and to compare MWIR with LWIR detection (Scafutto and De Souza Filho, 2018)."
- **Reference list (~L. 1146):** Scafutto, R. D. M. and De Souza Filho, C. R.: Detection of Methane Plumes Using Airborne Midwave Infrared (3-5 um) Hyperspectral Data, Remote Sensing, 10, 1237, https://doi.org/10.3390/rs10081237, 2018.

2. Specific comments

Comment 2.1

Comment 2.1 (first paragraph)

2.1: The authors' approach of using the vetted and established MODTRAN platform, which solves the multiple-scattering problem, to do the radiative transfer is reasonable and provides a trustworthy result that can be repeated/verified by others in the community. However, it also draws attention to one limitation of the paper, which is the failure to connect the methane "enhancement factor" relative to background levels to a broadly understood release rate (kg/hr, etc.). While the authors caveat (lines 164-165) that "the perturbations are intended to approximate super-emitter-like conditions, rather than to convert the EPA threshold directly into a column scaling factor," the submission would be strengthened if a stronger connection could be made between the enhancement factors modeled and the range of release rates that might yield the same absorption.

Author response: We thank the referee for the endorsement of the MODTRAN approach and for identifying the central limitation. We agree that connecting profile enhancement factors to a source rate would improve physical intuition, and we address it as described under the following paragraphs.

Comment 2.1 (second paragraph)

The authors label an enhancement factor of 2 (that is, twice the background level up to boundary layer of either 350m or 1,850m) as representing the bottom of the "super-emitter" regime, which is generally defined as 100 kg/hr. This assertion should be supported or explained further. Further explanation is requested because the corresponding release rates may be much higher than what is implied. First, the reviewer accepts that in the small-signal limit, a large quantity of methane, such as that caused by a release plume, that fills a volume much smaller than the sensor pixel IFOV, will result in the same signal that the same quantity of gas spread out uniformly throughout the pixel volume would. Because of this, the approach of using MODTRAN, which treats every layer as a uniform concentration, is valid even in the case where complete mixing has not had time to occur, such as from a point release. However, it may take a lot of release to get to the enhancement factors the authors model. As an example, consider a 100 kg/hr release (the lower limit of the "super-emitter" definition) in a 1 meter/s wind. In this low-wind case, even with some mixing, much of the release would clear the 750-meter pixel in at most 750 seconds, depositing just 21 kg of methane into the pixel volume in steady state. 21 extra kg of methane in the 350-meter high pixel (NBL case) represents a change in average concentration across the entire pixel of just 162 ppbv, or just 31 ppbv across the taller 1,850-meter pixel volume (RL case). The authors'

lowest labeled super-emitter enhancement value of 100% represents a concentration increases around 1,800 ppbv, more than an order of magnitude higher than this 100 kg/hr case. In the limiting case of near-zero wind, and very strong turbulent mixing and advection, the same 100 kg/hr of release would of course stay in the single pixel volume much longer, increasing the change in average concentration. How realistic is this case? (The reviewer is not an expert here.)

Author response: We thank the referee for this worked example. We checked its arithmetic: 21 kg of methane distributed over a 750 m by 750 m pixel with a 350 m or 1,850 m column corresponds to a pixel-mean mixing-ratio change of roughly 160 and 30 ppbv respectively, which is consistent with the referee's figures. We cannot verify the assumptions behind the 21 kg figure (residence time of the release in the pixel, mixing, and the steady-state assumption), because doing so would require the dispersion modeling that the referee also considers outside the scope. If those assumptions are representative, the modeled enhancements (+100% corresponds to about 1.9 ppmv in the perturbed layers, Table 1) are large compared with the pixel-mean change from a 100 kg/hr release, which would reinforce, not weaken, our conclusion that M12 detectability is marginal at best. We have accordingly removed the statement that a factor of 2 represents the lower end of a super-emitter regime. The 100 kg/hr value is now cited only as the EPA operational definition, with an explicit statement that no conversion to a column scaling factor is made.

On the question of how realistic the low-wind, strong-mixing limit is: we agree that near-zero wind with strong mixing would retain the release in the pixel longer and raise the pixel-mean enhancement, but quantifying that regime requires specifying wind, turbulent diffusion, entrainment, release height, plume age, and subpixel coverage. We therefore treat it as a limitation and do not draw conclusions about its frequency.

Changes in the manuscript:

- **Abstract L. 17-18:** Wording uses "extreme high-enhancement scenario at a 9x baseline concentration (~800% enhancement)".
- **L. 150-160:** Scenario ranges labeled "moderate" and "high-enhancement".
- **L. 181-184:** Added: "The 100-800% enhancement range (scale factors 2x-9x) represents idealized high-enhancement sensitivity scenarios, not a direct representation of a specific source-rate threshold. For reference, the EPA defines a methane super-emitter event operationally as a remote-detected source emitting 100 kg/hr or more (U.S. Environmental Protection Agency, 2025); no direct conversion of this threshold into a column scaling factor is made here."
- **Tables 2 and 3; Figs. 5, 6, A2, A5:** "Super-emitter" labels replaced by "high-enhancement" in text, tables, and figure titles. Figures 5 and 6 are otherwise unchanged.

Comment 2.1 (third paragraph)

A detailed modeling of steady-state point-release plume dispersal under realistic atmospheric conditions, identifying what is typical along the spectrum of low-wind/strong mixing to strong wind/lower mixing, is likely outside of the scope of the article. However, because a back of the envelope calculation suggests that even a relatively low-wind case may result in

concentration enhancements much lower than those considered, some effort to connect the regime modeled to release rate is warranted, ideally by introducing relevant references and presenting some discussion of atmospheric dynamics that would allow connecting the release rate to accumulation of gas in the pixel volume. As the overall conclusions are essentially negative (that the VIIRS sensor cannot detect or can only just barely detect the lower tropospheric concentration enhancements modeled), this addition would only strengthen that conclusion.

Author response: We agree that a full steady-state point-release dispersion analysis is outside the scope of this study, and that some effort to connect the modeled regime to release rate is warranted. A quantitative conversion would require plume geometry, subpixel coverage, wind, turbulent diffusion, entrainment, release height, and temporal evolution. There is a recognized pathway from column enhancement to source rate, but it cannot be applied uniquely to the idealized, vertically uniform MODTRAN perturbations used here, and the conversion would require a coupled dispersion and radiative-transfer framework. We have therefore clarified that the prescribed methane enhancements are radiative sensitivity cases rather than source-rate equivalents, and we added (i) a limitations statement describing the meteorological and plume-transport dependencies that preclude a one-to-one conversion, and (ii) a real-world example of an extreme release, the 2015-2016 Aliso Canyon blowout, to give readers a sense of the scale of events that do occur, with the explicit caveat that this is an extreme case and not a global upper bound. We agree with the referee that these additions can only strengthen the negative conclusion.

Changes in the manuscript:

- **L. 379-382 (Discussion, limitations):** Added: "Furthermore, the prescribed enhancement factors are radiative sensitivity cases rather than source-rate equivalents, because the pixel-mean enhancement produced by a given emission rate depends on wind, turbulent mixing, source height, plume age, and subpixel coverage. Resolving that relationship would require a coupled plume-dispersion and radiative-transfer framework, which is beyond the scope of this study."
- **L. 391-396 (Discussion):** Added Aliso Canyon paragraph: peak emission of about 60 t CH4 per hour and about 97 kt released over roughly four months (Conley et al., 2016), described as extreme cases rather than a global upper bound, with the enhancement cases interpreted as idealized radiative sensitivity scenarios and not emission-rate equivalents.
- **Reference list (~L. 1064):** Conley, S., et al.: Methane emissions from the 2015 Aliso Canyon blowout in Los Angeles, CA, Science, 351, 1317-1320, https://doi.org/10.1126/science.aaf2348, 2016.

Comment 2.2

Comment 2.2

2.2: The introduction to the discussion of spatial averaging from lines 280 – 308 is confusing, and could be clearer if tightened up. Essentially, the authors highlight that averaging multiple pixel measurements together will reduce the sensor noise by the square root of the number of pixels, but that this will add in quadrature with the ground emissivity variability. Then, they

show from satellite measurements that it the ground emissivity variability is indeed the limiting variance, an important result. Table 4 and the caption are mostly sufficient to explain this. Absent from the discussion of spatial averaging is that the single pixel IFOV of 750 meter is already a large pixel relative to a typical point emission plume, and that averaging multiple pixels will reduce the measured concentration if the plume does not fill the entire larger effective pixel.

Author response: We thank the referee for the accurate summary of the section and for the point about plume dilution. We agree the introduction to spatial averaging needed to separate random-noise reduction from the effect of averaging on a compact plume. We kept the existing structure, Table 4, and its caption, and made three short, targeted edits. First, Eq. (5) is now attributed to the standard error of the mean for independent, equal random noise (Bevington and Robinson, 2003), and is stated to apply to random instrument noise only and not to fixed-pattern, calibration, or scene-dependent terms. Second, we added the referee's point that the native 750 m footprint is already large relative to a typical point-source plume, so averaging dilutes the plume signal unless the plume fills the averaged footprint, and that the gains therefore apply to spatially extensive enhancements. Third, the CAMEL-derived emissivity term (0.185 K) is described as a proxy for scene-scale emissivity heterogeneity that is not assumed to average down, because its spatial covariance at the M12 scale is not resolved by CAMEL (about 5 km native resolution). The analysis was also rerun with corrected brightness-temperature conversion, and Fig. 7, Table 4, the abstract, and the Conclusion were updated accordingly.

Changes in the manuscript:

- **L. 310-312:** Eq. (5) limited to independent random instrument noise, with citation.
- **L. 326-334:** Dilution of the plume signal under averaging (L. 330-331); instrument-only noise reduction noted as not characterizing the detectability limit.
- **L. 343-345, 347-349:** Emissivity term not assumed to decrease with averaging.
- **L. 313-325 (Fig. 7):** Histogram, caption, and text updated (mean 279.19 K, central pixel 279.39 K, sigma 0.46 K, range 1.50 K).
- **L. 368-373 (Table 4):** Caption and values updated (sigma_combined 0.193, 0.188, 0.187 K for 3x3, 5x5, 7x7).
- **Abstract L. 19-23; Conclusion L. 449-455:** Revised to match; combined SNR 1.76 at the CAMEL-matched emissivity, below the detection threshold.
- **Reference list (~L. 1030):** Bevington, P. R. and Robinson, D. K.: Data Reduction and Error Analysis for the Physical Sciences, 3rd ed., McGraw-Hill, 2003.

3. Technical corrections

Comment 3.1

Line 66 "confounding" typo.

Author response: Corrected; the word now reads "confounding".

Changes in the manuscript:

- **L. 70:** Typo corrected.

Author response to Referee #2 (RC2)

Radiative Constraints on VIIRS Nighttime Detectability of Lower-Tropospheric Methane (egusphere-2026-4225)

We thank Referee #2 for the constructive review and the encouraging assessment. Each comment is addressed below.

General comments

General comment

This is an interesting paper exploring the feasibility of methane detection using the VIIRS satellite's MWIR band. Overall, the technical approach is sound and conclusions are generally well-evidenced. My main comments concern clarity, I did have some confusion reading the 'spatial averaging' section. I also think that the discussion of the location and strength of methane absorption lines is underdeveloped and could use an additional figure (e.g. pulling HITRAN and showing the lines within the spectral response of that particular VIIRS channel). This also would inform some of the saturation discussion. I identify the main weakness of the paper is a lack of observational validation. The data to do so readily exists, but I understand the argument that such analysis is outside the theoretical scope of the paper. Still, I think at least pulling an image to show what the MWIR channel looks like, maybe finding an example of a 'super emitter' case would help ground the reader. But overall, the conclusions are well written and would be quite interesting to those developing the next generation of overhead sensors for gas detection. Great work!

Author response: We thank the referee. The three main requests are addressed as follows: (1) a HITRAN-based figure of the methane absorption within the M12 spectral response (new Fig. 1); (2) clarification of the spatial averaging section (Eq. 5 limitations, plume dilution, and treatment of the emissivity term); and (3) observational grounding through a VIIRS M12 image over the study site (new Fig. 2) and a real-world extreme-release example (Aliso Canyon). The new Fig. 1 also supports the saturation discussion, because it shows that M12 sits on a weak wing of the methane feature. We agree that observational validation is the main remaining weakness. Because this study is a theoretical radiative sensitivity analysis, validation against satellite observations of real plumes is outside its scope, and we now state explicitly in the Discussion (L. 375-384) that the simulations are not observationally validated. We have added a figure (Fig. 8) that makes clear how far the results are from detection.

Changes in the manuscript:

- **Figs. 1, 2, 8:** New figures (see below).
- **L. 378-384:** Limitations paragraph states the lack of observational validation.

Specific comments

Specific comment 1

More detail is needed on the absorption/emission spectra in the MWIR. The specific structure is referenced throughout "… VIIRS M12 band, which has a small overlap with the wing of methane's MWIR absorption feature". Pulling HITRAN and plotting spectral absorption

along with the spectral response of the instrument would be a helpful thing to refer back to throughout the paper. Also, there is more than one MWIR band in VIIRS and I'm assuming M12 (3.61-3.79um) is the one that spans the strongest absorption feature but that needs to be shown explicitly? After all, there is a channel that spans very close to that range that is much higher resolution (I4: 3.55-3.93um – 375m resolution versus 750m for M12). Also, I didn't see a mention of M12's spectral range (just a note its centered around 3.7um) – that needs to be in there somewhere, preferably via plotting the spectral response.

Author response: We thank the referee. We added the M12 spectral range (3.61 to 3.79 um) to the Introduction and added a new Figure 1 with HITRAN2024 methane absorption plotted against the NOAA-21 M12 relative spectral response. Panel (a) spans 3.0 to 4.0 um and shows that M12 does not span the strongest absorption feature; it overlaps only the weak long-wavelength wing of the 3.3 um band, where the response-weighted mean normalized absorbance is about 0.1% of the maximum. This is the premise of the hypothesis tested. Panel (b) shows the line-by-line cross-section from 3.5 to 3.9 um. On I4: we selected M12 because it has lower noise on NOAA-21; at a typical scene temperature of 270 K the NEdT is 0.158 K for M12 and 0.416 K for I4 (Wang et al., 2024), which outweighs the finer 375 m resolution for a detection-limited problem. The statement is now in Sect. 1.5.

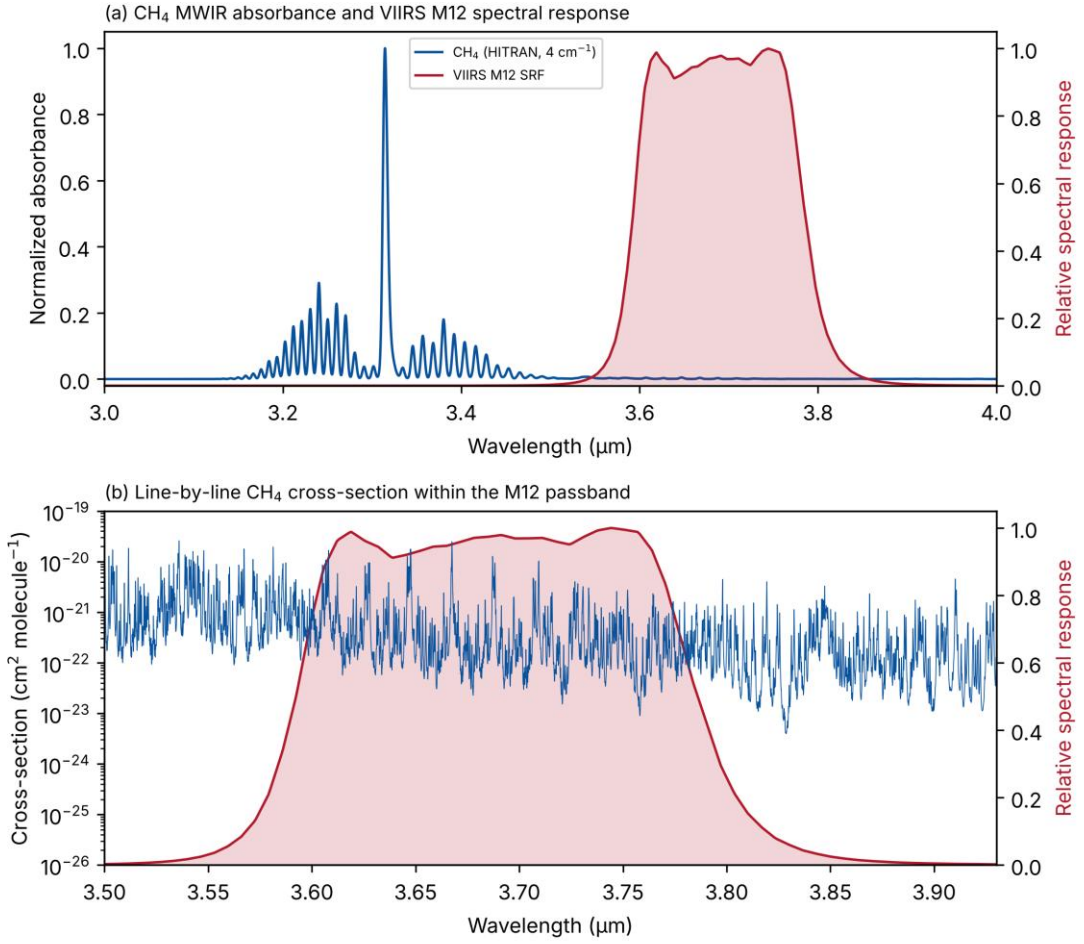


Figure 1. (a) Normalized methane absorbance from 3.0 to 4.0 um, computed from HITRAN2024 line parameters (Gordon et al., 2026) at 296 K and 1 atm, optically thin limit, smoothed with a 4 cm-1 Gaussian and normalized to its maximum near 3.3 um. The red curve is the VIIRS M12 relative spectral response. (b) Line-by-line methane absorption cross-section from 3.5 to 3.9 um under the same conditions, with the M12 response. The M12 band (3.61 to 3.79 um) lies on the long-wavelength wing of the methane MWIR feature, where its response-weighted mean normalized absorbance in (a) is about 0.1% of the maximum.

Changes in the manuscript:

- **L. 60:** "VIIRS M12 (3.61-3.79 um) band".
- **L. 77-86:** Figure 1 callout (L. 78), figure and caption (L. 80-86).
- **L. 211-213:** I4 rationale with NEdT values and citation.
- **Reference list:** Gordon et al. (2026), JQSRT, 353, 109807, https://doi.org/10.1016/j.jqsrt.2026.109807; Wang, Cao, and Blonski (2024), IEEE TGRS, 62, 5006313, https://doi.org/10.1109/TGRS.2024.3458869.

Specific comment 2

Overall, I'm not totally following the spatial averaging discussion. It makes sense that you'd give up some spatial resolution in order to improve your NETD and therefore have a lower detection threshold, but is just diving by the square root of the number of pixels valid? I'd show a reference or a derivation. Do you get an enhancement in all kinds of sensor noise? (shot noise, read noise, fixed pattern noise, thermal noise, etc).

Author response: We thank the referee. Division by the square root of N is the standard error of the mean and is valid for N independent measurements with equal random noise (Bevington and Robinson, 2003). It therefore applies to random instrument noise such as uncorrelated shot, read, and thermal noise. It does not apply to fixed-pattern noise, calibration uncertainty, correlated noise, or scene-dependent surface-emissivity variability, none of which need decrease at this rate. We now state this where Eq. (5) is introduced. We also added that the 750 m footprint is already large relative to a typical point-source plume, so averaging adjacent pixels dilutes the plume signal unless the plume fills the averaged footprint. Finally, the CAMEL-derived term of 0.185 K is a proxy for scene-scale emissivity heterogeneity and is not assumed to average down, because the spatial covariance at the M12 scale and the scale mismatch between CAMEL (about 5 km) and M12 (750 m) are not resolved here.

Changes in the manuscript:

- **L. 306-312:** Eq. (5), citation, and limitation to random instrument noise.
- **L. 330-331:** Dilution of the plume signal.
- **L. 343-345, 347-349:** Emissivity term not assumed to decrease with averaging; scale mismatch stated.
- **L. 368-373:** Table 4 caption updated.

Specific comment 3

An overall weakness of the study is a lack of observational grounding. I get that it's a

theoretical paper, and so a real data processing effort would be out of scope but I think even showing an image of VIIRS in the MWIR over HUBC would be helpful to ground things.

Author response: We agree and added a new Figure 2 showing the NOAA-21 VIIRS M12 brightness temperature for the 19 October 2023 nighttime overpass, with the study site marked. Panel (a) is a regional view and panel (b) an enlarged view with native 750 m pixels. The site pixel is 279.39 K. A full observational detection analysis is outside the scope of this theoretical study.

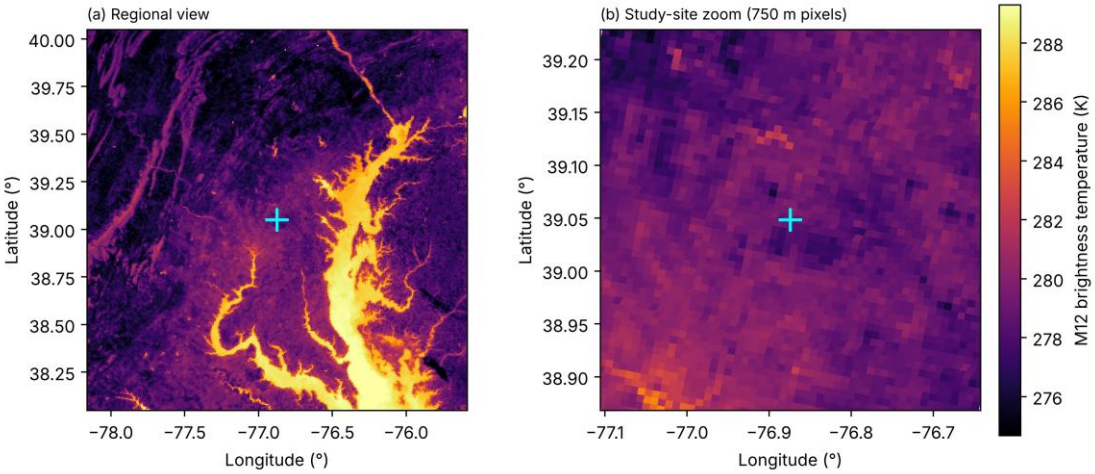


Figure 2. NOAA-21 VIIRS M12 brightness temperature for the nighttime overpass of 19 October 2023 (about 06:56 UTC) at a near-nadir view angle (4.9 deg). The cross marks the study site. (a) Regional view. (b) Enlarged view of the study site showing native 750 m pixels. Brightness temperatures were computed from the M12 sensor data record using the standard scale and offset, and the color scale spans the 1st to 99th percentile of the regional view.

Changes in the manuscript:

- **L. 106-107:** Figure 2 callout.
- **L. 111-116:** Figure 2 and caption.

Specific comment 4

Can you ground the 'enhancement profiles' in something real? Maybe find a real-world example of a super emitter event and say, maybe, what surface value that corresponded to? Just so the readers can gauge. 9x sounds like a lot to me, but has that ever happened/does it happen all the time?

Author response: We thank the referee. We added the 2015-2016 Aliso Canyon blowout as an observational example of an extreme release, with a peak emission of about 60 t CH4 per hour and about 97 kt released over roughly four months (Conley et al., 2016), noting that it is an extreme case and not a global upper bound. On whether 9x occurs: we do not claim that a pixel-mean 9x enhancement is routine. The 2x to 9x cases are idealized radiative sensitivity scenarios, not emission-rate equivalents, because the mapping from emission rate to a pixel-mean vertical enhancement depends on wind, source height, mixing depth, turbulent diffusion, plume age, and

fractional M12-pixel coverage and would require a coupled dispersion and radiative-transfer model. To help readers gauge magnitudes, Table 1 gives the resulting mixing ratios (for example 17.05 ppmv at the surface for 9x).

Changes in the manuscript:

- **L. 181-184:** High-enhancement scenarios described as idealized sensitivity cases; EPA definition as reference only.
- **L. 379-382:** Limitation on source-rate equivalence.
- **L. 391-396:** Aliso Canyon paragraph.
- **Reference list (~L. 1064):** Conley et al. (2016), Science, 351, 1317-1320.

Specific comment 5 (abstract)

I don't quite understand this sentence in the abstract "the detectable signal is therefore driven by the plume's thermal emission (path radiance) rather than surface absorption". Do you mean surface emission here? Also, I think of path radiance as being light scattered into/emitted by the path in between your target and your sensor. Here, your target is the gas plume so I'd think that phrase would be "the detectable signal is therefore driven by the plume's thermal emission rather than surface emission".

Author response: We agree; the wording was imprecise. "Path radiance" has been removed. The sentence now contrasts the plume's own thermal emission with absorption of surface-emitted radiation, which is the mechanism tested and not supported by our results.

Changes in the manuscript:

- **Abstract L. 12-14:** "...the detectable signal is therefore driven by the plume's own thermal emission rather than by absorption of surface-emitted radiation."

Technical corrections

Technical correction 1

I'd put a plot of SNR vs CH4 scale factor somewhere, with a horizontal line at 1 to show just how far away we are from detection. Just a suggestion though, maybe plot it and see how it looks. A comment in the captions of Figure 4 and 5 restating the NETD goal you're trying to reach would also be welcome, to drive home the point that those plots are nowhere even close.

Author response: We thank the referee and added the plot as new Figure 8. It shows residual-layer SNR versus CH4 scale factor (2x to 9x) for the instrument-only noise floor (NEdT = 0.158 K) and for the operational noise floor including surface-emissivity variability (sigma_combined = 0.193 K, 3x3 window). Lines show the CAMEL-matched emissivity (0.91) and shading spans emissivities 0.89 to 0.99. A dotted line marks SNR = 1 and a dashed line marks the SNR = 2 detection threshold. With the emissivity term included, no case reaches SNR = 2 (maximum 1.79). Regarding restating the NEdT goal in the captions of the NBL and RL figures (now Figures 5 and 6): the NEdT of 0.158 K and the SNR >= 2 criterion are stated in Sect. 1.5 (L. 206-212) and

Sect. 2.2 (L. 250-252), and Figure 8 shows the distance to the detection goal directly, so those two figures are left unchanged.

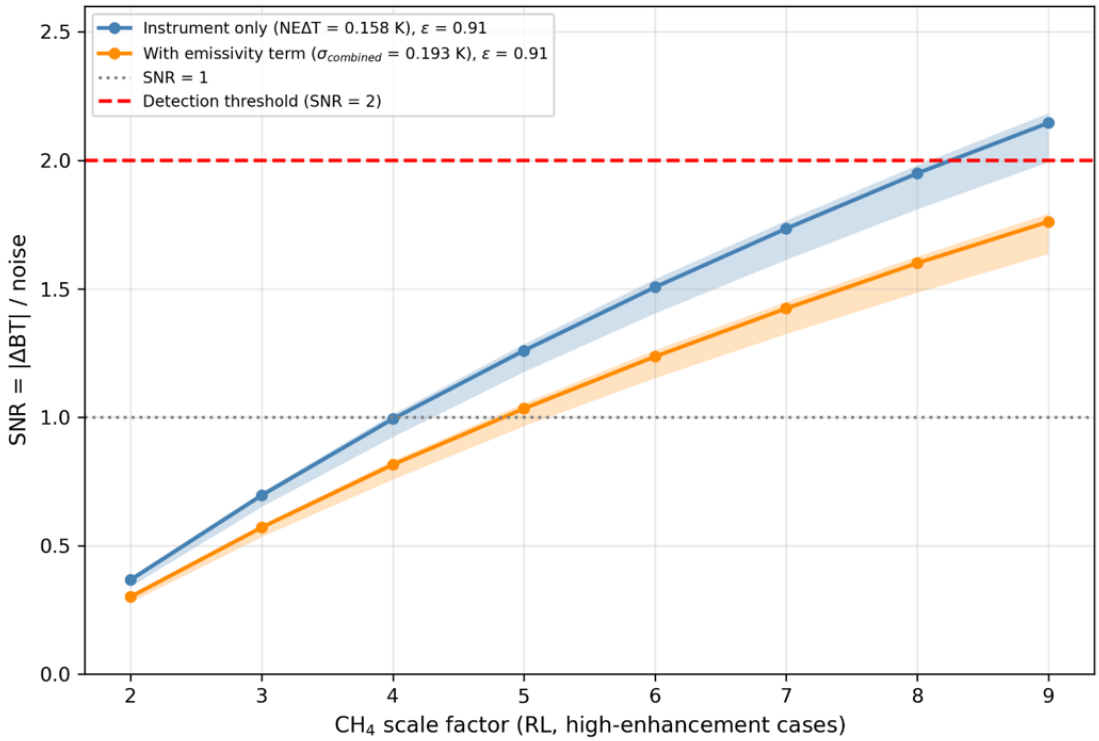


Figure 8. Residual-layer SNR versus CH4 scale factor (2-9x) for the instrument-only noise floor (NEdT = 0.158 K) and the operational noise floor including surface-emissivity variability (sigma_combined = 0.193 K, 3x3 window). Lines show eps = 0.91 (CAMEL-matched); shading spans eps = 0.89-0.99. The dotted line marks SNR = 1 and the dashed line marks the detection threshold (SNR = 2). With the emissivity term included, no case reaches SNR = 2.

Changes in the manuscript:

- **L. 350:** Figure 8 callout.
- **L. 351-355:** Figure 8 and caption.
- **L. 356-363:** Combined SNR reported: 1.76 at eps = 0.91 (0.339 K); overall maximum 1.79 at eps = 0.89 and 9x.

Technical correction 2

Cite MethaneSAT somewhere. Maybe where you talk about SWIR/daytime detection work.

Author response: MethaneSAT is now cited where SWIR and daytime methane retrievals are discussed.

Changes in the manuscript:

- **L. 33-34:** "...including from dedicated methane missions such as MethaneSAT (Chan Miller et al., 2024)."

- **Reference list (~L. 1054):** Chan Miller, C., et al.: Methane retrieval from MethaneAIR using the CO2 proxy approach: a demonstration for the upcoming MethaneSAT mission, Atmos. Meas. Tech., 17, 5429-5454, https://doi.org/10.5194/amt-17-5429-2024, 2024.

Technical correction 3
Don't need to capitalize the three key variables on line 84

Author response: Corrected; the three variables are now lowercase.

Changes in the manuscript:

- **L. 96-98:** "(1) plume vertical extent; (2) methane concentration; and (3) surface properties".

Technical correction 4
What's an 'optically active gas plume'? As opposed to what? This could be a common term, I've just never heard of it.

Author response: We agree the term was unclear and replaced it with "methane gas plume".

Changes in the manuscript:

- **L. 40:** "...a methane gas plume creates a thermal contrast signal..."

Summary of new and revised figures

- **Fig. 1 (new, L. 80-86):** HITRAN2024 CH4 absorbance and cross-section with the NOAA-21 M12 response.
- **Fig. 2 (new, L. 111-116):** NOAA-21 M12 brightness temperature scene over the study site.
- **Figs. 3, 4:** Renumbered; content unchanged.
- **Figs. 5, 6:** Retained; only "super-emitter" wording changed.
- **Fig. 7 (updated, L. 321-325):** 3x3 histogram with corrected brightness temperatures.
- **Fig. 8 (new, L. 351-355):** RL SNR versus CH4 scale factor with SNR = 1 and 2 lines.
- **Figs. A2, A5:** Relabeled to "high-enhancement".

*Additional (non-reviewer) edits

- Corrected spatial averaging analysis: During revision, we identified and corrected an error in our code and reran the spatial averaging analysis. The emissivity-derived uncertainty is now 0.185 K, and the combined noise floor is 0.193 K for a 3x3 window. The maximum combined SNR is 1.79 (1.76 at the CAMEL-matched emissivity of 0.91), which is below the detection threshold. The instrument-only single-pixel maximum remains 2.18. The abstract, Table 4, Figure 7, and the Conclusion have been updated accordingly.