

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 (~3.3-3.4 μ m) 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.

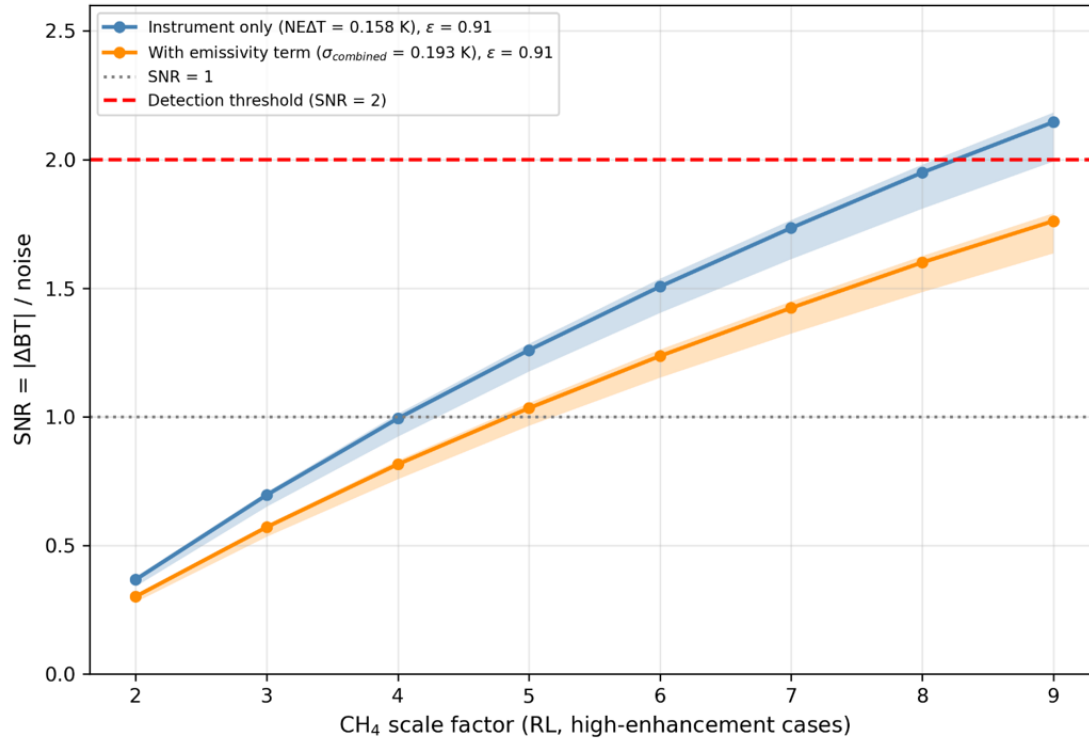


Figure 8. Residual-layer SNR versus CH₄ scale factor (2-9x) for the instrument-only noise floor (NEΔT = 0.158 K) and the operational noise floor including surface-emissivity variability (sigma_{combined} = 0.193 K, 3x3 window). Lines show εps = 0.91 (CAMEL-matched); shading spans εps = 0.89-0.99. The 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 μm) 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 μm) 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.