

We appreciate the editor and two anonymous referees for their time and constructive review of our manuscript. We greatly value the feedback received, have thoroughly reviewed and addressed all comments, suggestions, and concerns, and implemented necessary revisions into the manuscript. We believe that these modifications have significantly enhanced the clarity, accuracy, and overall readability of our manuscript - Thank you.

The modified texts in the revised manuscript are given in *blue italic* font.

RC: Referees' Comment (in black font)

AR: Authors' Responses (in blue regular font)

Detailed Response to Referee #1

General Comments:

RC: Choice of inversion method: Why was an analytical inversion chosen instead of an ensemble Kalman filter, which would allow for a much finer resolution of the state vector? India's political states are quite large, and it would be valuable to assess the redistribution of emissions relative to the prior also at a sub-state scale.

AR: Thank you for the comment. We acknowledge that different inverse techniques exist depending on observational type/density and spatiotemporal scales of state vectors. We agree that ensemble-based approaches like the Ensemble Kalman Filter (EnKF) offer potential for improving state vectors at finer scales, especially advantageous while utilizing large sets of observations (without the demand of an adjoint model). Our decision to use an analytical inversion was driven by the following factors: First: EnKF is often opted for a dense observational network and is a better choice in order to solve large inversion problems. While refining sub-state level state vectors would be beneficial with the extent of satellite-based column observations of methane, the region is limited with the availability of ground-based measurements that contain more information on near-field flux influences. We believe that the information derived from the assimilated observations, as done in this study, may not be adequate for a reliable reporting of emissions at finer scales; hence focusing primarily on deriving emissions on a regional scale in the present study. Second: An analytical solution is feasible for linear tracer flux mapping, like for methane, providing error statistics and information (note that EnKF approximates error statistics based on ensembles), especially for the dimension of the inversion problem (again limited by the observation space) analyzed in the present study. The analytical inversion, as applied here, ensures computational efficiency much better

than EnKF and interpretability while incorporating state-wise spatial constraints, effectively capturing nationwide emission redistribution.

At the same time, we acknowledge the advantage of EnKF and the value of sub-state space resolution in the inverse-based studies. By establishing more comprehensive ground-based observation networks that offer accurate and precise measurements, in addition to column observations and high-quality atmospheric transport models, future investigations can explore various inverse methodologies such as the ensemble Kalman filter (EnKF) and 4D variational inversion (4D-Var), which are capable of managing highly resolved state vectors, resulting in improved emissions at a significantly finer sub-scale.

To explicitly represent this point, we revise the conclusion as follows- Line 492-495:

“With the advancements of denser observational coverage and the high-quality atmospheric transport models, future research can thus explore and evaluate different inverse techniques like ensemble Kalman filter (EnKF) and 4D variational inversion (4D-Var) that can handle highly resolved state vectors, leading to improved emissions at a much finer sub-scale.”

RC: Use of observations: It is unclear how exactly the observations are used in the inversion. On page 9, line 164, the authors write that the measurement vector y contains m elements, representing the total column observations *where m represents the total number of observations in each political state*. Does this imply that only observations at a given state are used to constrain emissions within that same state? Ideally, all observations should be used in the inversion. If this is only a wording issue, the sentence should be rephrased to avoid confusion.

AR: All available observations across the domain were used simultaneously in the inversion process to constrain emissions. The phrase “ m observations in each political state” (now Line 180) was intended to describe the spatial grouping of observations within the target region, which is used for constructing Jacobian matrices. With this set-up, the inversion primarily captures near-field observational influence by giving negligible weights to far-field signals. Also, it is expected to minimize spatial inconsistency in the inferred emissions while avoiding over-interpretation of distant observational signals. Please note that the scope of this study is to interpret the influence of regional fluxes (>100 km).

RC: Presentation of results: The inversion results are currently presented only as total national emissions. This is insufficient. The changes in emissions at the state level should be shown. A map illustrating the posterior emissions by state would be very helpful.

AR: Thank you for the comment. Although sub-national scale data would be ideal, the focus of the present study is to provide national-scale anthropogenic CH₄ emission estimates, considering the coverage of the satellite observations, with the limited ground-based measurements (including column observations) in India. The inversion framework we employed in principle allows for posterior emission analysis at finer spatial and sectoral scales. However, we chose to report only the aggregated national totals, as we expect that the information content deduced from the available assimilated observations can be insufficient to obtain the robust reporting of the sectoral or sub-scale emissions. With denser observation networks, we aim to refine the inversion setup and report state-wise emissions in the future. For instance, Pillai et al. (2016) highlight the limitations of satellite-based inverse modeling to derive highly resolved emissions due to insufficient observational density, suggesting that denser observation networks could enable more accurate state-wise estimates. Similar studies, such as Nisbet et al. (2016), Zhang et al. (2017), and Zhao et al. (2019), also emphasize the challenges of observational density and the potential for improved accuracy with enhanced data networks.

However, we agree with the usefulness of providing region-wise emission estimates. Those have now included monthly region-wise prior and posterior emissions for 2018 and 2019 - please see Figure S13 and Figure S14 in the Supplementary Material.

Revised the Sect. 3.5 as follows:

Line 406 - 409:

“Although we focus on the national inversion estimates, owing to the inversion approach incorporated, a regional analysis (consistent with Sect. 3.1) of prior and posterior estimates has been presented in Fig. S13 and Fig. S14. ”

RC: Could the state vector also be resolved temporally to illustrate seasonal adjustments? This would be particularly interesting given the strong seasonality of emission sectors in some states.

AR: We perform monthly inversion. State vectors are optimized on a monthly scale. Please see our response above (see Fig. (s) S13 and S14 in the revised manuscript).

Revised the Sect. 3.5 as follows:

Line 406 - 409:

“Although we focus on the national inversion estimates, owing to the inversion approach incorporated, a regional analysis (consistent with Sect. 3.1) of prior and posterior estimates has been presented in Fig. S13 and Fig. S14.”

”

RC: Moreover, the comparison of model results to TROPOMI data is limited to monthly means. A more detailed comparison is needed: how well does the model reproduce individual observations?

AR: For the comparison with individual TROPOMI retrievals, we emphasize that daily-scale validation is limited by both observational sparsity and random retrieval errors. TROPOMI retrievals exhibit inherent noise at individual overpass levels, particularly over cloudy regions, which limits their usefulness in model evaluation. Because of the above, the evaluation strategy considered the monthly means that reduce the random errors; thereby, we report model uncertainties at a monthly scale, which is beneficial for inversion studies.

RC: Much of the variability may already be explained by seasonal changes in concentrations, meaning that comparisons of total CH₄ concentrations are more indicative of agreement in the CAMS product than of the model performance. What do the statistics look like for enhancements only? Summary statistics such as bias, RMSE, and correlation before and after the inversion would add substantial value.

AR: To examine the potential of our model to represent the variability from the regional fluxes compared to background (based on CAMS), we performed the following comparisons: TROPOMI vs. Enhancement (i.e. removing background contribution from total) and TROPOMI vs. Background. Figure R1 presents the results, indicating that observed variability aligns with the variability in enhancements (contributed by regional fluxes) than with the background variability. The correlation between the enhancement and observed XCH₄ is consistently higher than that between the background and observed XCH₄. The above analysis reconfirms the potential of our model to represent the regional variations at the monthly scale.

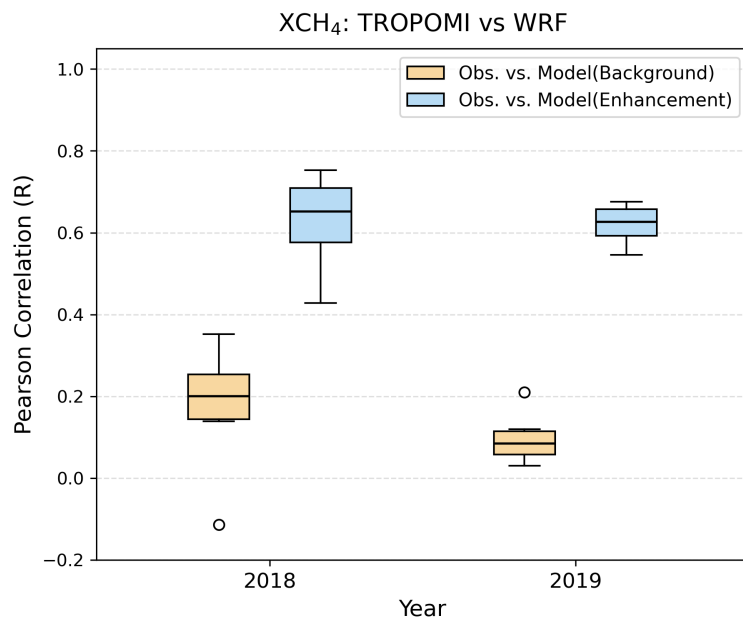


Figure R1: Contribution of regional and background flux fields on observed XCH_4 variations, for 2018 and 2019. Correlations were calculated spatially excluding spin-up time: the boxes represent the interquartile range (25th–75th percentiles) with the median shown as a horizontal line, while the whiskers denote the minimum and maximum values of the correlation coefficient.

Additionally, for the comparison with the Thumba site, we decoupled the contributions from regional fluxes and compared the variability captured by our model.

The text is revised as follows:

Line 319-321

“The comparison has also been done by removing the boundary contributions from CAMS (see Fig. S8), showing that enhancements correlate ($R^2 = 0.48$) with observed variability. Thus, the above comparison suggests the potential of our model in representing the regional and seasonal variations.”

RC: Use of additional TROPOMI products: It would be highly desirable to include the other two TROPOMI XCH₄ products: the operational and the GOSAT-blended product. These can differ considerably. Since the authors employ an analytical inversion (which requires only a single forward model run), incorporating these additional datasets seems feasible. This could also help better assess the posterior uncertainty, which currently appears rather low in this study.

AR: We thank the reviewer for this thoughtful suggestion. We fully acknowledge the value of multi-product assessments in improving robustness. We have carefully examined the differences among the three TROPOMI XCH₄ products, including the scientific (used in our study), operational, and GOSAT-blended datasets. While there are regional differences among them, we found that these variations generally fall within the 16 ppb observational uncertainty (Figure S1). Given that the analytical inversion method used here formally incorporates observational uncertainty (16 ppb), the posterior flux adjustments, as done in the present study, may not reflect this impact, but lie within the posterior error bound. i.e. the differences between products do not exceed the uncertainty threshold built into the inversion; hence cannot be considered as a suitable setup to assess their impacts on posterior or its uncertainty.

However, we agree with the systematic observational discrepancies seen in Fig. S1 and see the importance of examining their maximum possible impact on posterior flux uncertainty in the estimations. Hence, we performed further analysis by redefining the measurement uncertainty that encompasses worst-case retrieval differences, by incorporating 50% more measurement uncertainty than originally assigned in inversion, in order to assess the maximum potential impact of these retrieval differences on posterior flux estimates at a national scale. The results of the above test are included, and the text is revised as follows:

The following text has been added in section 2.4.1:

Line: 211-220:

“The chosen measurement uncertainty encompasses the variability across TROPOMI XCH₄ products (≤ 16 ppb; Fig. S1). Nonetheless, as illustrated in Fig. S1, systematic differences exist among XCH₄ products, which are likely to impact the optimization of fine-scale state vectors. Understanding how differences in various retrieval products influence flux estimations is vital for characterizing posterior uncertainty in inverse studies. This necessitates a more sophisticated inverse configuration that also includes a higher discretization of state space and a detailed sensitivity analysis of prior and

posterior fluxes across the region, which we consider as a future direction for this study. However, we considered the systematic observational differences due to different retrieval algorithms, as seen in Fig. S1, in a separate inversion by redefining the measurement uncertainty (>50 % more than the originally defined, ~ 25 ppb) that more-or-less represents the worst case scenario for retrieval differences across the region. The above setup can thus allow us to examine the maximum likelihood impact of such retrieval differences on posterior flux estimates at a national scale.”

Also, in section 3.5- Edited Line 408-412:

“As explained in Sect. 2.4.1, we examined the impact of differences in the retrieval algorithm (see Fig. S1), considering the worst-case scenario, on annual posterior flux estimates for the year 2018, showing that the posterior uncertainty increased to 4.4 Tgyr⁻¹ from 3.5 Tg yr⁻¹ at the national scale. Future research is required for fully characterizing the impact of retrieval error uncertainty on posterior flux estimations at a fine scale by employing a more advanced inverse configuration that incorporates higher state space discretization and a detailed sensitivity analysis.”

Technical Comments

RC: Page 2, Line 18: nearly --> more than

AR: Done (Line 20)

RC: Page 2, Line 28: OH- --> OH

AR: Done (Line 30)

RC: Page 2, Line 36: "largest" in the world?

AR: The line:

“The country holds high CH₄ emission potential with its largest cattle population, intense flood irrigation practices, ever-increasing fuel demand, and large wetland extent (nearly 4.7% of its total geographical area) (Ganesan et al., 2017; Garg et al., 2011; Ministry of Environment and Change, 2015; Myhre et al., 2013b).”

Edited to Lines 38-42:

“The country has the largest cattle (including bovine) population (Robinson et al. (2014) in the world. Along with this, its huge intense flood irrigation practices, ever-increasing fuel demand, and large wetland extent (nearly 4.7% of its total geographical area) contribute to its high CH₄ emission potential (Ganesan et al., 2017; Garg et al., 2011; Ministry of Environment and Change, 2015; Myhre et al., 2013b). “

RC: Page 3, Line 49: Jones et al. (2021) and Cusworth et al. (2022) are not really appropriate references at this point

AR: Yes. Removed and retained the other citation (Line 53).

RC: Page 3, Line 64: "more advanced" is disputable. It's certainly different than GOSAT.

AR: Edited to “more recent”. (Line 69)

RC: Page 4, Line 74: Is there really no other in-situ site available in India to validate the simulation?

AR: We are unable to obtain highly precise and accurate observational records over this analysis period (two-year period) across India that can act as reference observations for model evaluation, largely due to the absence of such measurements and in part because of the individually owned data policy that hinders open access.

RC: Page 4, Line 87: "single-pass" --> "single overpass"

AR: Done (Line 96)

Page 5, Line 104: This likely refers to the temporal resolution of the output, not of the model itself.

AR: Edited to “We have used the WRF-GHG 3.9.1.1 version with a horizontal resolution of 10 × 10 km² (Lambert conformal conic projection grid) and an output temporal resolution of 1 hour. (Line 114-115)

RC: Page 5, Line 108: Does this mean that the simulated concentrations are only affected by emissions up to 18:00 the previous day? Or are the tracer concentrations (from emissions) propagated to the following day? And is also the background/CAMS tracer re-initialized every day?

AR: The statement in line 108 (now line 117) implies that the first 6 hours are excluded from the simulation to allow for model spin-up, specifically for meteorology. The tracer

concentrations, including those from emissions, are propagated to the following day, and the background/CAMS tracer is not re-initialized each day, but instead continues to evolve based on the initial conditions provided.

RC: Page 5, Line 110: Have the authors considered using a different CAMS product, such as one that is fully inversion-optimized? This might have substantially lower biases than the GQIQ product which is crucial when subtracting the background concentrations from the observations for assimilation.

AR: There is a correction in the information -it is not the *GQIQ* product. We have used the CAMS Global Greenhouse Gas Reanalysis (EGG4). This has been updated in Table 1. We have not used the fully inversion-optimized product. However, we have compared these two products and found that the differences remain within approximately ~ 13 ppb (Figure R2). Note that the inversion-optimized product utilized SCIAMACHY satellite measurements and surface flux observations, both of which are sparse over India, the former due to cloud/aerosol issues, and the latter due to the absence of long-term observation stations over India.

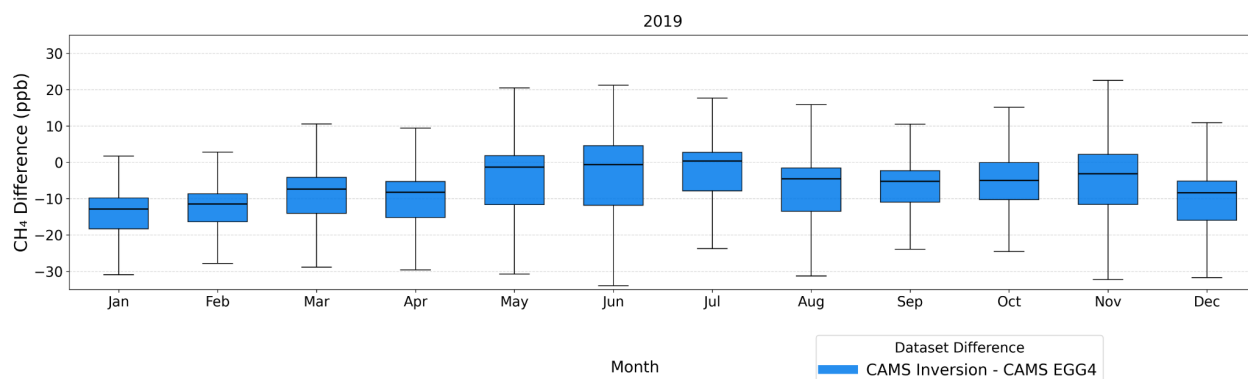


Figure R2: Difference in CAMS EEG4 (reanalysis) and the latest version (data not available for 2018) of inversion optimised dataset (using satellite+ surface-air sampled data) for 2019.

RC: Page 5, Line 111: Only initial fields? Is the CAMS product not used to also provide the boundary condition of the background tracer?

AR: It has been used to provide the boundary condition of the background tracer as well, which has already been given in lines 118-120:

“The model is re-initialized each day with ERA5 meteorology, in which a 6-hour spin-up time was configured. For CH₄ mixing ratio fields, initial and boundary conditions are

prescribed from the Copernicus Atmosphere Monitoring Service (CAMS re-analysis data)."

RC: Figure 1: The state boundaries are very difficult to distinguish. Since these abbreviations are used numerous times, clearer boundary delineation is important.

AR: Fig. 1 has been edited to show two figures in one panel: one with the regional delineation and the other with the topography height contour, to ensure clearer boundary delineation.

Table 7: The table formatting needs to be revised! Column titles must be visually separated.

AR: Edited

RC: Page 8, Line 137: "Calibration was performed periodically": How often was the calibration performed? This type of instrument is known to be temperature-sensitive, hence frequent calibration is important.

AR: Revised as follows, Line 149-150:

"Calibration was performed periodically. However, it should be noted that the instrument can be sensitive to temperature, requiring frequent calibration, which was not regularly met."

RC: Equation 1: The summation sign is missing.

AR: The document we submitted has the summation sign, but it is missing in the preprint. We will communicate with the journal regarding this.

Equation 9: Why does "TR" appear in the equation? Should the expression not just be " Φ perturbed – Φ " ?

AR: As explained earlier, TR refers to the target region that we use for constraining K-matrices.

RC: Page 9, Line 182: Does the optimization of anthropogenic emissions alone not lead to potential systematic errors in regions with very high wetland emissions in the northeast of the country? Have the authors considered optimizing these as well?

AR: Natural wetland emissions were excluded from the inverse optimization. The impact of natural wetland emissions on the column mixing ratio enhancement was minimal, smaller than the uncertainty in the satellite measurements. This has been clarified in the following lines:

Line 441-443 has been modified to:

“We excluded natural wetland emissions from the inverse optimization as they have resulted in negligible impacts on the column mixing ratio enhancement (Fig. S10), which is smaller than the uncertainty of the satellite measurement.”

RC: Page 10, Line 184: "biomass emissions" --> "biomass burning emissions"?

AR: Done (Line 201)

RC: Page 10, Line 187: Are these background mixing ratios simulated by the model or derived from observations?

AR: Edited to (Line 204):

“Simulated background mixing ratios”

RC: Page 10, Line 193: Is also S_a a diagonal matrix?

AR: Yes. S_a is a diagonal matrix.

Edited line 210-211:

“We have not considered cross-correlations; hence, only the diagonal elements of the matrices S_e and S_a are non-zero.”

RC: Page 10, Line 199: Do the authors mean that the emissions are indeed "spatially averaged" or are they saying that one parameter per state represents the total emissions for that state?

AR: Yes, we agree that “spatially averaged” is confusing here. It is rewritten as follows:

Line no: 200 - 202

“ x_A represents the prior fluxes, which consist of monthly anthropogenic (major contributions from enteric fermentation, agricultural soil, waste water handling, and fuel exploitation) and biomass burning emissions. For the optimization, we average the

emissions per state. i.e. optimizing one parameter per state representing the total emissions for that state.”

RC: Equation 9: The summation signs are missing. Also, what unit does x have?

AR: Similar to the previous case, the preprint we submitted had the summation, but it is missing in the one available online. We will look into this. The annual optimised fluxes have the unit of Tgy^{-1} . The text has been modified thus:

Line 233:

“The national budget for annual optimized fluxes $\hat{x}_{\text{annual}}$ (in Tgy^{-1}) is derived as:“

RC: Page 11, Line 211: "Regional distribution" --> "Regional and sectoral distribution"

AR: Done (line 238)

RC: Section 4.1: This section presents interesting content, but it is quite lengthy and somewhat tedious to read. A more concise and focused text would greatly enhance readability.

AR: The content has been made more concise.

RC: Page 11, Line 213: Why is EDGARv8 used here when the inversion was performed using EDGARv7?

AR: We used the updated version at the time.

RC: Figure 2: Subfigures c–f are too small to discern meaningful details in the emission maps.

AR: Fig.2 c-f has been moved to the supplementary section (Fig. S2) for clarity, and the text has been modified accordingly.

RC: Page 12, Line 240: Please add a reference to Fig. 3 when discussing the GFAS emissions.

AR: Done (line 267)

RC: Page 14, Line 260: Both links are invalid.

AR: On cross-checking, the links seem to be working fine in India, Europe and the US. They have been copied here for easy reference.

- 1) <https://rangareddy.telangana.gov.in/animal-husbandry> (line 281)
- 2) <https://khammam.telangana.gov.in/economy> (line 283)

RC: Page 15, Line 295: Vertical mixing does not reduce column mean concentrations. Could this be due to the seasonality of OH instead? Or does this seasonality really result from the combination of increased mixing and the higher sensitivity towards near-surface concentrations of TROPOMI?

AR: Thank you. Edited. Line no: 345-349:

“The minimum enhancement for the whole Indian domain occurs during the monsoon season (June–September), likely due to a combination of higher boundary layer heights and stronger winds, which enhance vertical and horizontal transport affecting column CH₄ concentrations. The concentrations may also be impacted by the seasonal changes in regional or larger fluxes (>1000 km); however, it needs further investigation to assess their contributions.”

RC: Figure 4: The upper end of the color scale is too low. Seasonal patterns described in the text (p.15, line 294) are not visible in the current plot.

AR: Thank you for pointing this out. Figure 4 (now no. 5) is replaced with an annual mean map instead of the monthly figure panel. The monthly figure panel (now placed as Fig. S9) has been modified to show higher upper ends of the color scale and thereby represent the seasonal features in a better manner.

RC: Page 20, Line 339: How do the authors derive the values of 75 (72) ppb? These are not clearly traceable from the table.

AR: This is a typo, which has been modified in the manuscript to 13 (29) ppb. (now in line 396).

RC: Page 20, Line 349: The fact that the model explains 56% and 79% of the observed variability is largely due to the seasonality already captured in the CAMS product. What do these statistics look like for the enhancements only?

AR: A supplementary figure has been added, and it has been mentioned in the text: Line 319-321:

“The comparison has also been done by removing the boundary contributions from CAMS (see Fig. S8), showing that enhancements correlate ($R^2=0.48$) with observed variability. Thus, the above comparison suggests the potential of our model in representing the regional and seasonal variations.”

RC: Page 20, Line 350: This sentence does not make sense.

AR: The line has been removed.

RC: Section 4.4: This section would make more sense if it came right after Section 4.1. This would mean that the analysis of the enhancements and the comparison with the satellite data would come before the inversion results.

AR: Done. It comes after section 4.1 (now 3.1)

RC: Figure 9: It is difficult to reconcile Fig. 7b with Fig. S7b. They do not seem consistent. In the supplementary figure, the comparison appears much better.

AR: We cannot expect a one-to-one compatibility, as Figure 7b (now 4b) shows the median, 25th, and 75th percentiles for both 2018 and 2019 data for each month, while Figure S7b presents the time series from January 2018 to December 2019.

RC: Page 21, Line 376: How do the authors arrive at the uncertainty range of 14 to 23%? Based on the total a posteriori emissions and uncertainty, I calculate a reduction of 3.8 to 26.8%.

AR: The overestimation by the EDGAR emission inventory is calculated by comparing the EDGAR-reported annual emissions for 2018 and 2019 with our derived posterior anthropogenic emissions for India. EDGAR reported 28.7 Tg emissions in both 2018 and 2019, whereas our posterior estimates are 25.2 Tg and 23.3 Tg, respectively. The range 14% - 23% is derived from this.

Uncertainty = ((Posterior - EDGAR) / Posterior) * 100

Uncertainties for 2018 and 2019 were calculated using the above equation.

RC: Page 23, Line 429: The study's estimate is 24% higher than the IPCC value. This means the IPCC estimate is 19% lower than the study's result, not the other way around.

AR: Thank you for pointing out this confusion. Edited:

"At the same time, our estimate is 19 % higher than what the Government of India reported to the UNFCCC for the same period but close to the latest Global Methane Budget 2000-2020." (Line 488-489)

More context has been added/revised in Lines 415-429 in section 3.5:

"Previous studies also reported an overestimation of the global emission inventories over India. For instance, Qu et al. (2021) report 41–57 Tg yr⁻¹ anthropogenic CH₄ emission from India, which is significantly higher than our estimations. Also, Zhang et al. (2021) estimate Indian anthropogenic methane emissions of 33±0.6 Tg yr⁻¹, higher than this study estimates. However, the Global Methane Budget (2000-2017, Saunio et al. (2019)), based on top-down approaches using in-situ and GOSAT observations, suggest 25 Tg yr⁻¹ of anthropogenic CH₄ emission from India, but acknowledging large uncertainty ranges in their estimations. Also, bottom-up models' estimates that are compiled in Saunio et al. (2019) and Jackson et al. (2020) indicate a mean anthropogenic CH₄ emission of 21-24 Tg yr⁻¹ from India. The above two estimates align with our results, though we used independent observations and a different modeling approach. The recent updates on the Global Methane Budget (2000-2020, Saunio et al. (2025)) indicate anthropogenic methane emissions of 37-49 Tg yr⁻¹ for South Asia (including Afghanistan, Bangladesh, Bhutan, India, Nepal, Pakistan, and Sri Lanka), in which around 21.7 Tg yr⁻¹ are contributed from the Indian region (calculated using the data prescribed from Martinez et al. (2024)). Janardanan et al. (2024) reported the annual averaged (2009 - 2020) CH₄ emissions from anthropogenic sectors over India as 24.2 ± 2.1 Tg yr⁻¹ which is close to our results. The total CH₄ emissions derived from a combination of satellite data (GOSAT), surface and aircraft measurements, and the atmospheric transport model for 2010–2015 were found to be 22 Tg yr⁻¹, which is substantially lower than the emissions reported by the EDGAR v4.2 inventory (Ganesan et al., 2017). "

RC: Page 23, Line 422: Would this large effect of wetlands on XCH₄ not suggest that these emissions should be optimised?

AR: Thank you for the feedback. There has been a modification.

Line 422 (now 479-481) has been modified to:

"The total XCH₄ along the eastern coast reflects the influence of agricultural soil emissions on column-averaged methane. Although wetland emissions peak in this region, their contribution to atmospheric mixing ratios is negligible."

Given the small magnitude of the wetland contribution in our simulation setup, we chose not to explicitly optimize this component in the inversion. However, we acknowledge that optimizing wetland emissions, particularly during monsoon and post-monsoon seasons when their influence is expected to be higher, could improve model–observation agreement. This is a promising direction for future work, especially with improved wetland emission inventories or dynamic wetland models in conjunction with a good number of ground-based measurements.