Referee Report

The authors have revised the manuscript noticeably, and both the text and the figures are now clearer. The readability of the paper has noticeably improved, and several formulations have been sharpened. However, despite these improvements, I still have major concerns regarding the inversion methodology and the presentation of the inversion results. Several points remain unclear or insufficiently addressed, and in my view, these issues must be rigorously clarified or corrected before the manuscript can be considered for publication. Below I summarize the most critical points, ordered by importance.

1. Use of observations and construction of the Jacobian

The manuscript still does not clearly explain how the observations are used in the inversion. The authors' response suggests that observations are grouped by political state when constructing the Jacobian. This raises the concern that each state may be constrained only by observations within that same state. This would be some form of extreme localization, which is not needed in analytical inversions.

This point must be clarified unambiguously. If the inversion is indeed structured in this way, the methodology must be revised.

2. Insufficient presentation of inversion results

Only national annual total emissions are shown in the manuscript. Even though the appendix now includes prior and posterior values for five larger regions, a spatially resolved plot of posterior adjustments at the political-state level (i.e., for each element of the state vector) is essential. Without such a figure, it is impossible to assess whether the inversion leads to reasonable spatial adjustments.

In addition, no statistics are presented that quantify model–observation agreement in the prior and posterior. Such diagnostics are fundamental to evaluating the credibility of the inversion.

Without these results, it is not possible to evaluate or trust the posterior emission estimates.

3. Explanations of Equation (4)

The motivation, derivation, and interpretation of Eq. (4) remain unclear. It is not explained why perturbations are used instead of defining a tracer per state, nor why a summation over states appears in this formulation.

This equation must be clearly and transparently explained.

4. Model initialization and treatment of the CH_4 fields

The simulations are reinitialized daily with ERA5 and a 6-hour spin-up. It remains unclear whether the CH_4 fields are also reinitialized each day. If so, only emissions from the current day could be used to constrain emissions, which would strongly reduce the inversion's capability. Furthermore, the manuscript states that CAMS fields are corrected by a monthly factor of 1–3%, but the method for applying these corrections is not described.

Both the CH_4 initialization procedure and the implementation of the CAMS corrections must be explicitly explained.

5. Abandonment of the stratospheric extension

The absence of a stratospheric extension introduces a systematic offset of 2–3% in the CH_4 columns, this cannot simply be ignored.

This issue must be addressed.

6. Very coarse spatial resolution of the state vector
The state vector consists of only 36 political regions, which is extremely coarse for a country the size of India. Even with an analytical inversion, higher state-vector dimensionality are easily possible. Higher-resolution inversions using TROPOMI data have been demonstrated in the literature (e.g. https://egusphere.copernicus.org/preprints/2025/egusphere-2025-2622/). There is more spatial information in TROPOMI data than the authors suggest.
The resolution should be increased or at least the choice of such a coarse resolution needs a very strong justification and discussion.

7. Non-inclusion of alternative TROPOMI/CAMS products and wetland optimization
The authors reject sensitivity tests with additional TROPOMI products, CAMS products, and wetland emissions on the grounds that differences between datasets are “within 16 ppb”, the assumed observation uncertainty. This is not a convincing argument.
Systematic differences between products, particularly in spatial patterns and bias structures, can influence posterior emissions even if pointwise differences are smaller than the nominal uncertainty. Recent studies (e.g. https://egusphere.copernicus.org/preprints/2025/egusphere-2025-2622/) show large discrepancies among the TROPOMI products.
At minimum, the product product differences and their potential impact must be discussed more rigorously.

8. Inconsistent description of emission sources in the Introduction
Line 43 states that enteric fermentation accounts for 8% of India’s total GHG emissions, while Figure 2 shows a contribution of 42%. This is confusing. It would be clearer to express contributions relative to total CH_4 emissions, not total GHG emissions.
This discrepancy should be corrected for clarity.

9. Unclear whether one or two inversions were performed
It is not clear whether the authors conducted one inversion (including both anthropogenic and biomass-burning fluxes) or two separate inversions for EDGAR and EDGAR+GFAS. This needs to be stated explicitly.
Please clarify how many inversions were performed and how the components were treated.

In summary, at least the first six points require substantial clarification and improvement. These issues are central to the validity and interpretability of the inversion results. I recommend major revisions. If these methodological uncertainties cannot be resolved convincingly, I would not recommend the manuscript for publication.