

Authors' Response to Referee comment on "High-resolution inversions of Methane over Europe using the Community inversion Framework and FLEXPART" by Mengistu et. al.

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Authors' Response to Anonymous Referee #1 comments:

We thank the anonymous referee for the time spent reading our manuscript and for providing important comments and suggestions. They are enormously constructive and are used to improve the quality of the manuscript. Our reply included an extra analyses and a figures that are included and discussed in this document. Below, we address each comment in detail and indicate the corresponding changes made in the revised manuscript. Reviewer comments are shown in blue, our responses in black, and additions to the manuscript in *italics*.

Overview

In "High-resolution inversion of methane emissions over Europe using the Community Inversion Framework and FLEXPART," Mengistu and co-authors use the CIF system and Flexpart model to perform a 4D-Var data assimilation over Europe. The state vector includes CH₄ concentrations and CH₄ surface fluxes. Authors distinguish different sectors in the inversions and perform the inversions at 0.2x0.2 degrees. Results are compared to observations and compared to other inventories, including countries' UNFCCC reporting. Overall I think this is a valuable study and has potential to be of strong interest to ACP readers. However, improvement is needed in the handling of uncertainty and description of the methods, particularly how prior error covariances are developed for the sectoral attribution.

Response: We thank the reviewer for the positive assessment of the study and for emphasizing the need for improved clarity in the treatment of uncertainty. In response, we have revised the manuscript to provide a more detailed and transparent description of the construction of the prior error covariance matrices. In addition, we have clarified the methodology used to compute posterior uncertainties and their associated analysis.

General comments

Sectoral results heavily rely on sectoral correlations B, which contains correlation structure C. How is matrix C constructed? Are the same assumptions used for all sectors? There are many subjective choices for building this correlation matrix. It is not clear from the manuscript what these choices are.

Response: Yes, the correlation matrix \mathbf{C} is constructed using the same assumption as the total flux inversion. Specifically, we construct the correlation matrix \mathbf{C} using an isotropic Gaussian function that decays with distance as $\exp(-r^2/l^2)$ (Gaspari and Cohn, 1999; Peters et al., 2005). A spatial correlation length of 200 km over land and 500 km over oceans is prescribed, together with a temporal correlation length of 90 days. These assumptions are applied uniformly across all sectors, without sector-specific tuning. We acknowledge that inversion results are sensitive to the specification of \mathbf{C} and the prior error covariance. To assess this, we performed sensitivity experiments with alternative correlation lengths and prior uncertainties. These tests indicate that while the magnitude and spatial distribution of sectoral adjustments can vary, the main large-scale patterns remain qualitatively consistent, highlighting both the influence of prior assumptions and the limits of sectoral attribution.

In the revised manuscript, we now explicitly describe the construction of the prior covariance for the sector-specific inversions as:

"...The prior covariance \mathbf{B} for the sector-specific inversion is constructed with diagonal elements set to 50% of the sectoral prior flux. A corresponding correlation matrix \mathbf{C} , consistent with the total flux inversion, is also assumed. The matrix \mathbf{C} is defined using isotropic Gaussian spatial correlations, $\exp(-r^2/l^2)$, with $l_{land} = 200$ km and $l_{ocean} = 500$ km, together with a temporal correlation length of 90 days. These assumptions are applied uniformly across all sectors, without sector-specific covariance tuning."

This change is made on page 6, lines 164–168.

How sensitive are the optimized fluxes to the observations? Can authors show the footprint map from the Flexpart simulations, are all areas of the map sensitive to the observations? I am particularly curious about Italy and the southern Europe region, where there are few observing stations but very large corrections.

Response: We thank the reviewer for this important comment. We agree that the sensitivity of the optimized fluxes to the observational network and the spatial representativeness of FLEXPART footprints should be clarified. In response, we have analysed and included footprint sensitivities from FLEXPART simulations, shown as annual mean for 2021 (Fig. 1Ra). The results indicate a heterogeneous observational constraint, with relatively lower sensitivities over Italy and southern Europe due to the sparse station coverage.

Despite this, notable flux corrections are inferred in these regions, driven by the combination of moderate footprint sensitivities and relatively large prior geological emissions. The resulting emission sensitivities (Fig. 1Rb), defined as the product of footprint sensitivity and prior emissions, remain sufficient to inform the inversion. Similar strong posterior reductions over Italy and Romania have been reported by Steiner et al. (2024), attributed to inflated geological prior emissions. This clarification has been incorporated in the revised manuscript on page 17, lines 399–408. The updated text reads:

"...Reductions in the GEO sector over Italy and Romania are consistent with previously reported overestimations arising from the global scaling of geological emission factors. Steiner et al. (2024) likewise identified strong posterior reductions in these regions, attributing them to inflated geological prior emissions. Although prior geological emissions were harmonized to 23 Tg yr^{-1} at the global scale, the inversion highlights the need for regionally differentiated estimates. Footprint sensitivities from FLEXPART (Appendix, Fig. 1a–1b) show a heterogeneous observational constraint, with moderate sensitivities over Italy

and southern Europe. Nevertheless, substantial flux corrections are inferred due to the combination of these sensitivities and relatively large prior emissions, yielding sufficient emission sensitivity to drive posterior adjustments."

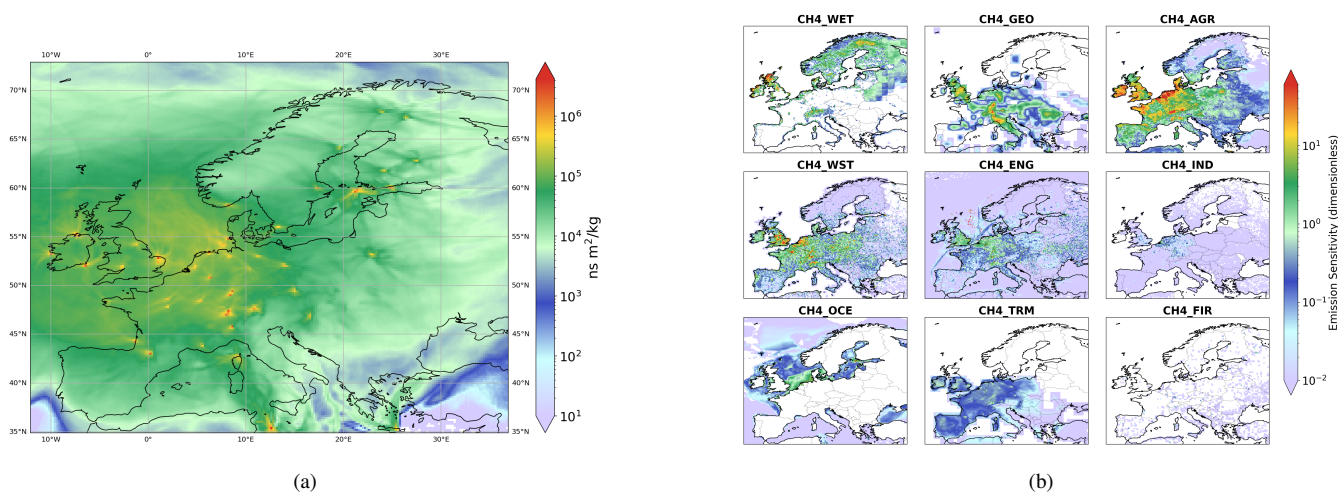


Figure 1. (a) FLEXPART footprint sensitivity to the assimilated observations, shown as the mean sensitivity field for July 2021, illustrating the spatial heterogeneity of observational constraints across Europe. Regions with denser measurement coverage exhibit stronger sensitivities. (b) Emission sensitivity, defined as the product of the footprint sensitivity and the monthly mean prior CH₄ flux for each emission sector, indicating where prior emissions and observational sensitivity jointly contribute to constraining the inversion.

Uncertainty on the results is not stated when results are given. The authors performed a sensitivity inversion, but only for 1 month due to computational cost. What are the implications for overall uncertainty on the year-to-year estimates? In addition to discussing the uncertainty in the 1-month estimates, can authors describing the implications for uncertainty levels on the overall results, and include that with the results? For example, on the bar chart in Figure 9 and time series in Figure 5. Or, can authors use posterior uncertainties described in lines 274-276 to present the results with their uncertainties?

Response: The one month sensitivity inversion serves as a diagnostic to illustrate uncertainty reduction within the inversion framework. We acknowledge that this analysis alone is insufficient to quantify uncertainties at annual or interannual scales, and this limitation is now explicitly clarified in the revised manuscript (page 12, lines 312–314):

"...Due to computational constraints, the sensitivity experiments were carried out for July 2021 only. This summer-month analysis is intended as a diagnostic evaluation of inversion-framework sensitivity and does not capture the full seasonal variability."

To provide an uncertainty limit on our overall results, we extended the uncertainty analysis of the total flux inversion to cover a full year and applied it across the entire study period. Posterior uncertainty estimates are now propagated to the aggregated results and explicitly included in the time series (Fig. 5). Accordingly, the reported total flux estimates are accompanied by their corresponding uncertainty ranges.

In general, using 2 decimal places for CH₄ concentrations in ppb implies that the model and measurements have precision down to the hundredth of a ppb. I doubt this is the case, and in my opinion using 0 or 1 decimal places would be much better. Similar for emissions, describing emissions down to 0.01 Tg precision implies a high degree of confidence in the results, but I am not convinced this level of precision is warranted. Including uncertainty ranges as mentioned above could help with this.

Response: We thank the reviewer for this valuable observation, and we agree that the numerical precision used in the original manuscript may have implied an unrealistic level of confidence in both the modeled concentrations and emission estimates. In response, we have revised the numerical precision reported throughout the manuscript. CH₄ concentrations are now presented with one decimal place. Similarly, emission estimates are reported with a precision of 0.1 Tg. Furthermore, consistent with the reviewer's earlier comments, uncertainty ranges are now explicitly reported alongside the main results.

Specific comments

Line 16 – there is a more recent citation for the Global Methane Budget (Saunois et al. 2025 <https://doi.org/10.5194/essd-17-1873-2025>)

Response: We thank the reviewer for pointing this out. The manuscript has been updated to cite the most recent Global Methane Budget: (Saunois et al. 2025 <https://doi.org/10.5194/essd-17-1873-2025>). Change is made on page 1, line 16.

Line 112-113 – the sentence is confusing. Is the purpose of the sentence to state that analytical methods give a closed form expression for the posterior error covariance, while variational methods do not provide a posterior error covariance? If so, this should be stated. Does CIF-Flexpart give a posterior error covariance? Please clarify.

Response: We thank the reviewer for this important clarification request. The original sentence was intended to distinguish between analytical and variational inversion frameworks. Analytical inversion methods provide a closed-form expression for the posterior error covariance matrix. In contrast, variational approaches such as 4D-Var do not explicitly form the full posterior error covariance during the optimization. CIF-FLEXPART is capable of estimating the posterior error covariance when the analytical inversion option is used. However, for the high-resolution configuration applied in this study, the dimensionality of the problem makes this approach computationally intractable. Therefore, we employ a variational 4D-Var framework to obtain the maximum a posteriori flux estimate, and we estimate posterior uncertainties using a Monte Carlo ensemble approach, as described in the Methods section.

This clarification has been added to the revised manuscript (pages 4–5, lines 115–131).

Line 143 – The methods are a bit unclear. Are two separate inversions being performed, one for total flux and another for specific sectors? Or are all sectors included in the state vector and all optimized with concentrations in a single inversion?

Response: We apologise for this lack of clarity. Two separate inversions are performed: one for total methane flux and another for sector-specific fluxes, carried out as independent optimizations. In the total-flux inversion, only the aggregated flux is included in the state vector, whereas in the sector-specific inversion all emission sectors are included and optimized simultaneously using the same observations. This distinction has been clarified in the revised manuscript (page 6, lines 156–157).

"...Sector-specific inversion, conducted separately from the total-flux inversion, all emission sectors are explicitly included in the state vector and optimized simultaneously using the same concentration observations."

Line 161 – some more detail on the 3D OH fields are needed as these vary quite a bit (a citation to the study or model version is probably sufficient).

Response: We thank the reviewer for this comment. To clarify the treatment of OH, we have revised the manuscript to include additional detail and an appropriate citation describing the prescribed three-dimensional OH fields used in the inversion. This revision is included in the updated manuscript on page 7, lines 179–183.

110 *"...The dominant atmospheric sink of CH₄ through oxidation by OH is represented in FLEXPART as a first-order chemical loss process along particle trajectories (Pisso et al., 2019). This loss is computed using temperature-dependent reaction rate coefficients and prescribed three-dimensional OH concentration fields from the GEOS-Chem model. The OH fields are provided as monthly mean distributions with global coverage and vertical structure, consistent with standard GEOS-Chem simulations of tropospheric oxidant chemistry."*

Line 183 – please state the significance of there being ICOS and non-ICOS observations – what is the difference between these networks? Are the measurements of different quality?

Response: We agree that the distinction between ICOS and non-ICOS observations should be clarified. In the revised manuscript, we explain that ICOS observations follow standardized protocols for calibration, quality control, and data processing, ensuring traceability and consistency across sites, whereas non-ICOS observations may follow different quality-assurance frameworks. A detailed comparison of measurement quality is beyond the scope of this study. The revised text now clarifies how both datasets are treated in the inversion (pages 7–8, lines 205–208).

125 *"...which provides a harmonized European network of atmospheric CH₄ observations widely used in inverse modelling. The dataset includes both ICOS observations, which follow standardized ICOS protocols for calibration, quality control, and data processing, and non-ICOS observations, which are collected outside the ICOS quality-controlled may follow different quality-assurance procedures."*

Lines 191-194 – Are the measurements at the sites in the same grid boxes comparable? Or, can authors provide some other justification for choosing arbitrarily? Are results sensitive to this choice?

130 **Response:** Station selection was partly constrained by computational limits, restricting the number of assimilated sites to 46. Where multiple stations fell within the same model grid cell or close proximity, one representative site was selected based on regional representativeness and network standardization. Preference was given to ICOS stations and low-altitude sites, as mountain stations are likely more influenced by complex transport. This choice was not based on a formal statistical performance assessment, as such an analysis is beyond the scope of this study.

135 *"..., only one was retained, with preference given to ICOS and low-altitude stations."*

The change is made on page 8, lines 217 – 218

Line 208 – again the ICOS definitions are unclear. Please define “ICOS levelling,” and the significance that data are ICOS labeled after a certain time.

Response: We thank the reviewer for pointing out the lack of clarity. We have revised the manuscript to define ICOS labelling and explain the significance of ICOS-labelled data. ICOS labelling refers to the application of standardized calibration, quality control, and data processing procedures. Data are considered ICOS-labelled from the point at which a station meets the ICOS

quality-assurance criteria, indicating compliance with harmonized standards and high data reliability. The revised manuscript has been updated to clarify that this represents the transition date (page 9, Figure 1 caption).

145 *"...Data points after the red marker are ICOS-labelled, indicating that measurements have been processed according to ICOS protocols from the indicated date onward..."*

Line 274-275 – How posterior error covariances are specific is unclear. Please give a brief description or equation.

Response: We thank the reviewer for this comment. We have clarified the estimation of the posterior error covariance in the revised manuscript. Posterior uncertainties are derived using a Monte Carlo ensemble within the 4D-Var framework, in which prior fluxes and observations are perturbed and independently optimized. The posterior error covariance is approximated by the sample covariance of the optimized ensemble, as described in the revised manuscript (pages 4–5, lines 115–131).
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In the revised manuscript, we revised the text as: "...All inversions were performed using a 4D-Var ensemble approach, in which 30 Monte Carlo realizations were generated by perturbing both prior fluxes and observational data. Posterior uncertainties were estimated from the ensemble by approximating the posterior error covariance discussed in Sect. 2.1, Eq. 4).

This clarification has been incorporated into the revised manuscript (page 12, lines 308–311).

155 *Lines 276-277 – what are the expected impact of only doing the sensitivity tests in the summer? Methane’s seasonality in both emissions and background concentrations in the northern midlatitudes is significant, affecting background and wetland sources, please comment on the significance of this. Even better would be to do winter sensitivity tests but this is likely computationally prohibitive.*

Response: We thank the reviewer for highlighting this important point. We agree that methane exhibits pronounced seasonal variability in the northern midlatitudes, affecting both emissions (e.g., wetlands) and background concentrations. The sensitivity experiments were therefore designed as a diagnostic test of the inversion framework and were limited to July 2021 for computational feasibility, rather than to characterize seasonal uncertainty. We now explicitly state this limitation in the revised manuscript on page 12, lines 313–314. To provide a more representative uncertainty assessment, posterior uncertainties are estimated over a full year and propagated across the study period. The results have been included in Fig. 5, and the main results are now reported with their associated uncertainties.
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Could authors please comment on the very large GEO decrease over Italy? The signal is a similar magnitude as wetland and anthropogenic emissions which is surprising.

Response: We agree that the magnitude of the inferred decrease in geological (GEO) emissions over Italy, comparable to wetland and anthropogenic sources, may initially appear surprising. This result is primarily driven by relatively large prior geological emissions over Italy, combined with the available observational constraints. Although footprint sensitivities over southern Europe are weaker than in western and central Europe, they remain non-zero. When combined with large prior GEO emissions, this results in sufficient emission sensitivity to permit substantial posterior adjustments. The pronounced reduction over Italy therefore reflects a correction of likely overestimated prior geological emissions rather than an anomalously strong observational constraint. Similar strong posterior reductions in geological emissions over Italy and Romania were also reported by Steiner et al. (2024), who attributed these decreases to inflated prior geological emission estimates in these regions. More
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broadly, recent studies suggest that global geological methane emissions may be overestimated and suggest the global total should be lower than 15 Tg yr⁻¹ (Saunois et al., 2025).

Lines 404-410 – authors may be interested in and should consider citing two recent papers regarding seasonal cycles of energy sector emissions Varon et al. 2025 ES&T (<https://doi.org/10.1021/acs.est.5c08745>) and Hu et al. 2025 ES&T (<https://doi.org/10.1021/acs.est.4c14090>)

Response: We thank the reviewer for highlighting these recent studies. We have now cited Varon et al. (2025) and Hu et al. (2025), which provide satellite- and observation-based evidence of pronounced seasonal variability in energy-sector methane emissions, with wintertime increases of up to 40% relative to summer. These findings support our interpretation of the winter peak in energy-sector emissions. The manuscript has been updated accordingly (page 21, lines 450 – 454).

"...Recent studies support this seasonal behaviour. For example, Hu et al. (2025) found that oil and gas methane emissions can be substantially higher in winter, with estimates indicating increases of up to 40% compared to summer. Similarly, Varon et al. (2025) reported pronounced seasonal variability in methane emissions from oil and gas production regions based on satellite inversions. These findings provide independent observational evidence supporting the wintertime enhancement of energy-sector methane emissions inferred in this study."

Lines 476 – Authors may also be interested in and should consider citing East et al. 2025 Nat Comm (<https://doi.org/10.1038/s41467-025-67122-8>) comparing inversion results to UNFCCC reports, and whether they see similar changes

Response: We thank the reviewer for this suggestion. We have now cited East et al. (2025), who compared inversion-derived methane emissions with UNFCCC national reports and identified comparable country-level discrepancies, including both positive and negative deviations across Europe. This comparison supports our interpretation of the differences between our posterior estimates and UNFCCC inventories and highlights the value of atmospheric inversions for evaluating national-scale emission reports. The manuscript has been updated accordingly (pages 22–23, lines 472–476).

"...These national-scale differences are consistent with the findings of East et al. (2025), who compared inversion-derived methane emissions with UNFCCC inventories and similarly reported both positive and negative deviations at the country level across Europe. Their results underline the value of atmospheric inversion approaches for identifying potential biases and uncertainties in national methane emission reporting."

What is the temporal resolution of the optimizations?

Response: The optimizations are performed at monthly temporal resolution, with fluxes estimated independently for each month (page 10, lines 257–258). In the revised manuscript, we now explicitly state the spatiotemporal resolution of the inversion. This clarification has also been added to the conclusion (page 27, lines 554–555).

"...The inversion is performed at monthly temporal resolution, optimizing fluxes independently for each month..."

"...enables the inversion of emissions at a monthly temporal resolution and a spatial resolution of 0.2° × 0.2°."

Lines 501-502 – could authors clarify what is meant by “fine-scale emissions patterns that are not accessible through conventional analytical inversion techniques”? I’m not sure this is a true statement.

Response: We thank the reviewer for this comment and agree that the original phrasing may have overstated the distinction. We have revised the text to clarify that our intent was not to suggest that fine-scale emission patterns are fundamentally inac-

cessible to analytical inversion techniques. Rather, the emphasis is on the flexibility of the variational framework to efficiently accommodate larger state vectors and high-resolution configurations. This clarification has been incorporated into the revised manuscript to better reflect the capabilities and limitations of the different inversion approaches. (page 27, lines 552–554).

215 *"...resolving emission patterns in high-resolution inversion settings, particularly where large state vectors make conventional analytical inversion approaches computationally challenging."*

Technical corrections

Abstract contains many undefined acronyms **Response:** We have revised the abstract to reduce the use of acronyms and improve readability for a broader audience.

Line 83 – parentheses around ICOS are missing

220 **Response:** We have corrected the text to include parentheses, and it now reads “Integrated Carbon Observation System (ICOS)” in the revised manuscript.

Figure 2 – I appreciate authors showing all of the data, but it is difficult to parse apart Fig 2 and make sense of it. Markers are quite small and cover one another, and the colors are difficult to distinguish and keep track of. Please consider if there is any way to make the plot more clear to aid readability.

225 **Response:** We thank the reviewer for this comment. Figure 2 has been revised to improve clarity by enhancing marker visibility and color contrast and reducing overlap, thereby improving readability.

Figure 9 – Y-axis is not labeled, what do the different bars with the same color represent?

230 **Response:** We thank the reviewer for pointing this out. The y-axis label has been added in Fig. 9 (ISO3 country codes), and the figure caption now clarifies that bars with the same color represent different years, stacked from bottom (2017) to top (2022), to improve interpretation.

Fig 7 – This figure is unclear. Where are the ranges coming from for the boxplots, from the posterior uncertainty? Or temporal variability? What is the yellow shaded area that is referred to in the description, could there be a typo?

235 **Response:** We thank the reviewer for pointing out this lack of clarity. The ranges shown in the boxplots represent temporal variability across the study period rather than posterior uncertainty, and this is now clearly stated in the revised figure caption. We also corrected a typo in the caption, replacing “yellow-shaded” with “light-blue-shaded” for consistency with the figure.

Authors' Response to Anonymous Referee #2 comments:

We thank the anonymous referee for the time spent reading our manuscript and for providing important comments and suggestions. They are enormously constructive and are used to improve the quality of the manuscript. Below, we address each comment in detail and indicate the corresponding changes made in the revised manuscript. Referee comments are shown in
240 blue, our responses in black, and additions to the manuscript in *italics*.

Overview

This study utilizes methane concentration observations from 46 ground-based stations across Europe to estimate European methane emissions from 2017 to 2022 using a constructed Community Inversion Framework. The manuscript reveals corrections to bottom-up emission fluxes across different sources and regions on a monthly scale and analyzes the sensitivity of the
245 results to various inversion parameter assumptions. The study addresses significant scientific questions and is well-constructed. The inversion methodology is described in detail and appears robust, with a comprehensive analysis of the results. I recommend publication after the authors address the following (mostly technical) comments.

Response: We thank the referee for the positive assessment of the manuscript and for recognizing the scientific relevance of the study as well as the robustness of the inversion framework. We are grateful for the referee's constructive and mostly
250 technical comments. All comments have been carefully addressed in the revised manuscript. The revisions include clarifications of methodological aspects and technical improvements to enhance clarity and transparency. We believe that these changes have strengthened the manuscript.

Specific Comments

Section 2.1: The ability of the inversion system to distinguish between different methane sources needs further justification.
255 The manuscript claims to constrain different sources (Line 85). While technically feasible within the inversion framework, atmospheric methane emitted from different sectors is physically indistinguishable without isotopic data or co-emitted tracers. Consequently, the source attribution in the posterior results likely relies heavily on the spatial patterns provided by the prior inventory. I suggest the authors avoid overstating this capability. A more critical discussion is needed regarding the extent to which the observational data actually contains sufficient independent information to disentangle different methane sources.

Response: We agree that CH₄ concentration data alone cannot distinguish source sectors without isotopic or co-emitted
260 tracer information. The original wording overstated this capability. We have revised the text to clarify that, although sectors are optimized separately, source separation is limited by the information content of the observations. The updated text now includes:

“... While sectors are optimized separately, atmospheric CH₄ observations alone cannot fully distinguish emission sources
265 without isotopic or co-emitted tracer information (Mikaloff Fletcher et al., 2004; Turner et al., 2019). Nonetheless, the inversion yields a first-order, observation-constrained estimate of sectoral emissions that remains useful for interpreting regional variability and supporting national reporting.”

This change is made on pages 3-4, lines 87 – 91.

270 **Section 2.2:** Please clarify how the OH sink is treated in the model. Is it accounted for within the last two terms of Equation 8? Providing a more explicit explanation of the OH representation would improve the clarity of the methodology.

Response: We thank the referee for requesting clarification on the treatment of the OH sink. The removal of CH₄ by reaction with OH is not represented explicitly in Eq. 8, but is implicitly included in the FLEXPART backward simulations through prescribed three-dimensional OH fields. During the computation of sensitivities, FLEXPART accounts for CH₄ loss by OH oxidation, so the resulting sensitivities already include the chemical sink. We have revised the text to clarify that OH is included via the observation operator rather than as a separate sink term in Eq. 8. The updated text reads:

275 *“...Sensitivities are computed by FLEXPART and include methane loss due to oxidation by OH.”*

This change is made on page 7, line 193–194.

Section 2.4: Inland water emissions (other than wetlands) appear to be excluded from the analysis. What is the rationale for this omission? Based on existing literature, what is the estimated magnitude of inland water emissions within the study domain? The authors should discuss how neglecting this source might bias or affect the inversion results.

Response: We thank the reviewer for highlighting this issue. We agree that the original text was unclear regarding the treatment of inland water emissions. In our inversion, inland waters are not treated as a separate source category but are partly included within wetland emissions from the JSBACH–HIMMELI model, which represents methane fluxes from peatlands, inundated soils, and other water-saturated or freshwater-influenced environments. Inland waters outside wetlands are estimated to contribute about 0.7 Tg yr⁻¹ in the study domain (Lauerwald et al., 2023). The revised text reads as:

285 *“...over Europe. The modeled CH₄ fluxes include contributions from peatlands, water-saturated and inundated soils, and net mineral soils, thereby representing the dominant diffuse terrestrial and freshwater-influenced methane sources.”*

This change is made on page 11, lines 280–282.

Section 2.5: Lines 213–223 provide a sound justification for the selection of the observation time windows, which is reasonable. However, could the authors provide information regarding the temporal standard deviation of the measurements during these periods? This would be helpful to understand the variability of the data used.

Response: We thank the reviewer for this helpful suggestion. To characterize temporal variability, we computed the standard deviation of CH₄ mole fractions within the selected observation windows at each site (14:00–16:00 local time for low-altitude stations and 02:00–04:00 local time for high-altitude stations). These values were compared to the variability computed over the full day. Across all stations, the mean standard deviation for the full day was 39.4 ppb (range: 18.9–125.7 ppb), while the mean standard deviation in the selected windows was slightly lower at 37.2 ppb (range: 18.8–103.8 ppb). This corresponds to a mean relative variability reduction of 4.0%, with values ranging from -4.2% to 23.6% across stations. Fig. 2 illustrates the relative reduction in variability for each station, expressed as a percentage decrease. Across most sites, the selected time windows show reduced temporal variability, reflecting periods of more stable atmospheric conditions. A few sites exhibit limited or even negative reductions, likely reflecting local influences such as nearby emissions or complex topography. This analysis confirms that the chosen observation windows are generally appropriate for inclusion in the inversion framework, minimizing the impact of short-term fluctuations on the estimates.

325 **Minor Comments**

Line 13: The phrase "the influence of horizontal correlation length" may be too technical (jargon) for the abstract. Please consider rephrasing it to be more accessible to a broader audience.

Response: We agree that the phrase "horizontal correlation length" may be overly technical for the abstract. To improve accessibility, we have rephrased the sentence to use more general language. The revised text now reads as "...*spatial spread of emissions...*", which conveys the same concept while being clearer for a broader audience. The abstract has been updated accordingly. This change has been made on page 1, line 12.

Structure: Some paragraphs are excessively long, such as the block from Line 280 to 326. I suggest breaking these into smaller paragraphs to improve readability.

Response: We thank the reviewer for this constructive comment. We agree that the paragraph between Lines 280–326 was overly long and reduced readability. We have therefore restructured this section into several shorter paragraphs, each focusing on a specific aspect of the analysis (station-level time series, aggregated statistics, and subregional performance). In addition, all reported statistical metrics (e.g. mean bias, RMSE, correlation coefficients) are now consistently rounded and reported to one decimal place throughout the text, figures, and tables. These revisions improve clarity and consistency and have been implemented in the revised manuscript. This change is made on pages 12–14, lines 317–365.

340 References

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