

Reviewer comment #1

This paper reports an inversion to calculate methane fluxes for Africa using a blended TROPOMI-GOSAT satellite XCH₄ product. Two different prior estimates are used, and the results are not very sensitive to the prior used. The inferred increase in emissions is attributed mostly to livestock, with other contributions noted. The authors describe changes between the prior and posterior emissions and give explanations for the changes. They suggest reasons for the errors in the wetland prior and how they can be improved.

The paper is very well written, the results are of significance, and I recommend publication after addressing the following minor comments.

We thank reviewer #1 for taking the time to review and help improve our manuscript.

General comments

The blended TROPOMI XCH₄ product is described at line 100, but elsewhere in the paper, including the abstract it is just referred to as TROPOMI observations. The Balasus et al (2023) that is the reference given for the product calls it a blended TROPOMI+GOSAT product. Would it be more appropriate to describe it as a blended TROPOMI+GOSAT product in the abstract at least, and possibly elsewhere? Or something similar to recognise that the data is TROPOMI bias corrected using GOSAT?

We follow the reviewer's suggestion and now call it the blended TROPOMI+GOSAT product in the Abstract. We also clarify in the text that "TROPOMI" refers to that product. The description in Section 2.1 now reads: "*We use the blended TROPOMI+GOSAT XCH₄ product (Balasus et al., 2023), accessed from <https://registry.opendata.aws/blended-tropomi-gosat-methane/> (last access: 22 October 2025) and refer to it as TROPOMI for brevity.*"

Many countries and a few regions in Africa are referred to in the paper, and not all readers will be familiar with them. I suggest labelling them on the maps when they are mentioned, to help the reader. Alternatively, a separate map or insets on existing maps could be used to show the countries/regions mentioned.

We struggle with that one. Papers focused on the US or Europe don't generally consider it necessary to label states or regions, and we prefer to follow suit here. We also prefer to keep the focus on broad spatial patterns (with the exception of Figure 8, where we quantify emissions from sub-continental regions and thus the regions are outlined specifically).

Are posterior uncertainties calculated by the inversion? Not much discussion is given on uncertainties in the estimates. The optimised fluxes are given as 71-72 Tg/a, where does this range come from, it is just from the two prior estimates? What else could contribute significantly to the uncertainty in the posterior fluxes? In the abstract the total flux is given just as 72 Tg/a, should that be 71-72 Tg/a? And is that a realistic estimate of the total uncertainty?

We now give 71–72 Tg a⁻¹ and other numbers as ranges in the abstract and explain that the ranges come from the choice of wetland prior estimate. Posterior uncertainties are calculated by the inversion but are known to be overly optimistic when aggregated to the continental-scale.

The Abstract now reads: “*Our best estimate of total surface fluxes from Africa over the 2019–2024 period is 71–72 Tg a⁻¹ depending on the choice of prior wetland emission estimate, including 28–32 Tg a⁻¹ from wetlands and 23–25 Tg a⁻¹ from livestock.*” To Section 3, we add the sentence: “***This range reflects only the uncertainty introduced by varying the prior estimate.***”

Specific comments

line 72 - it took me a while to understand what was meant by "with separation at the equator", perhaps there is a clearer way to explain this.

We have rephrased this sentence to read: “*and 6 boundary conditions (ppb) including for the northern and southern boundaries, and for the western and eastern boundaries separately north and south of the Equator.*”

Line 135-136 - This description of error could be improved: E.g., "are too low" - too low for what? perhaps better to say they don't reflect the total error. "we correct them" - better to say something like "we incorporate retrieval error using TCCON..."

We incorporate these edits such that the sentences now read: “*The uncertainties reported in the operational TROPOMI product for individual observations, propagated from noise in the spectral measurements, do not reflect the total observation error because they do not account for retrieval error (Sicisk-Paré et al., 2025). Following Schneising (2025), we incorporate retrieval error by using ground-based XCH₄ observations from TCCON stations worldwide (Wunch et al., 2011).*”

Line 229 - please give a short explanation of how the 10-fold cross validation is carried out (just half a sentence or so). There is not much detail given in Turner et al.

We have revised the sentence such that it now reads: “*We validate our results using 10-fold cross-validation as in Turner et al. (2020), in which observations are split into ten subsets and each subset is withheld from the inversion in turn for evaluation.*”

Reviewer comment #2

The authors use the latest satellite data to investigate changes in African methane emissions from 2018 to 2024. By extending the analysis up to 2024, they provide a timely examination of the causes of the recent surge in atmospheric methane concentrations. The study reveals that emissions in this region have continued to increase in recent years and clearly quantifies the contributions of multiple sectors. I recommend publication after addressing the following comments.

We thank reviewer #2 for taking the time to review and help improve our manuscript.

Specific comments

It is encouraging to see that the dependence of TROPOMI inversion results on prior information has been resolved in both seasonal and spatial patterns. The authors should discuss the general applicability of this method and its potential limitations.

We believe this result to be a function of our inclusion of prior error correlations and now elaborate on that point in the conclusions: ***“Prior error correlations dependent on spatial similarity in source sectors, as used here, further facilitates the separation of posterior emissions between sectors. This represents significant improvement over the standard practice of assuming uncorrelated prior errors.”***

The manuscript frequently emphasizes monthly emission quantification, but the analysis primarily focuses on the seasonal variability of wetland emissions. Since livestock migration can also lead to significant monthly changes in emissions, it would be helpful to include a discussion of this aspect.

We thank the reviewer for their comment. We acknowledge that livestock emissions can be seasonal due to migration as mentioned or due to hydrology as discussed in Lunt et al. (2019). However, our primary motivation for conducting monthly inversions is to avoid aggregation error through assumed seasonality in the bottom-up estimates. We now emphasize this in Section 2: ***“We run 77 months of sequential monthly analytical inversions (August 2018–December 2024). Unlike annual inversions, this approach avoids imposing seasonality from the bottom-up estimates.”***

For the scope of our manuscript, we prefer to limit the seasonality discussion to wetlands, where we believe, in contrast to livestock, we have quality activity data (from CYGNSS) and thus we can provide guidance on improving bottom-up estimates.

Line 179: The description of the CYGNSS satellites as “a constellation of satellites that can use GPS signals to map inland water extent” is not accurate. Its use of L-band Synthetic Aperture Radar (SAR) allows CYGNSS to detect surface water, even under dense wetland vegetation.

While CYGNSS does use the L-band, it passively uses GNSS signal and thus we do not believe the measurement technique to be accurately described as Synthetic Aperture Radar. We appreciate the reviewer pointing out that we have not highlighted some of the advantages of the L-band. We revise the sentence to now read: ***“Instead, we use observed inundation from CYGNSS, a***

*constellation of satellites that can use GPS signals to map inland water extent, **seeing through vegetation and clouds.***”

The results of McNicol et al. have considerable uncertainty, so a comparison may not be meaningful. The authors should clarify why this comparison is made—for example, whether it is intended to highlight the current uncertainty in emission intensity estimates.

We include the results of McNicol et al. to emphasize that applying their data-driven estimates of wetland emission intensity instead of CARDAMOM would not solve the issue. We revise the text to reflect this and the sentence in Section 5 now reads: “***As a potential alternative to CARDAMOM, also shown in Fig. 9 are methane emission intensities from McNicol et al. (2023), derived from the FLUXNET eddy-covariance flux tower network (Delwiche et al., 2021) by applying machine learning dependences on environmental variables.***”

The seasonal cycle from the CARDAMOM differs from both the TROPOMI inversion results and the CYGNSS inundation. It would be better to discuss possible reasons for this discrepancy.

We now highlight that the CARDAMOM seasonal cycle in Figure 10 broadly tracks that of temperature. The sentence in Section 5 now reads: “*Again, CYGNSS inundation alone does much better at reproducing the relative seasonal amplitude and interannual variability in our optimized emissions, but the seasonality is dampened by CARDAMOM which has opposite phase, **tracking temperature.***”

The recent article by Peng et al. (2026, DOI: 10.1126/science.adx8262) should be cited and discussed to support the conclusions regarding annual methane emissions from African wetlands and livestock.

We now cite Ciais et al. (2026, <https://doi.org/10.1126/science.adx8262>) in our introduction.