Reviewer 1

The authors explore the relative importance of the CH4 sources and sinks in recent changes in the methane growth rate, by performing several different inversions of GOSAT CH4. In particular, their results emphasize the necessity of including both the sources and sinks in such an inversion to get accurate results as well as the large impacts of increasing temperatures on the CH4 loss to OH. While this is an interesting paper, the authors sometimes fail to justify their assumptions, leaving questions about the accuracy of the results, as outlined below. Once these issues are addressed, the paper will be suitable for publication in ACP.

We are grateful for reviewer's insightful and positive comments. We fully agree that we must make assumptions about prior flux uncertainty and model errors, as we don't know them accurately. While there is a common issue for the community, we have made set of sensitivity tests to confirm robustness of our results.

We agree that key assumptions should be more clearly justified. We have expanded the manuscript to better explain our choices.

Line 88: Where does the value of 1.2% come from? I see you used the same value in your 2023 paper, but that also has no justification.

The value of 1.2% is not directly observed but represents an assumed estimate of atmospheric model error, reflecting the limited information available to constrain this quantity. However, this value is broadly consistent with the standard deviation of the differences between posterior model concentrations and independent in situ observations across the Northern Hemisphere. We have clarified this rationale in the revised manuscript.

Line 97: Sentence beginning with "Using" is poorly worded.

Agreed. We have re-written the sentence as:

Assimilating different subsets of observations, for example, including more data from Integrated Carbon Observation System (ICOS) sites over western Europe (ICOS RI et al., 2025), can affect regional flux estimates by up to several teragrams per year. However, these changes have only a small impact on the interannual variability of posterior global methane emission and sink estimates and remain within the reported uncertainty range.

Line 110: Need a citation for the MERRA2 dataset. E.g. Gelaro, R., McCarty, W., Suarez, M. J., Todling, R., Molod, A., Takacs, L., et al. (2017). The Modern-Era Retrospective Analysis for Research and Applications, Version 2 (MERRA-2). *J. Clim., Volume 30, (Iss 13), 5419-5454.*

Great suggestion. We have now added this reference.

Line 116: Are these annually varying OH fields or are they static for a given month? From later in the text, it seems the latter, but that should be explicitly stated here.

We now clarify that the OH monthly climatology is prescribed using month-specific fixed 3-D fields.

Line 125: Your reference is missing.

Thank you for identifying this issue. We have now added the reference.

Line 131: Where does the 40% number come from for your uncertainty? How sensitive are your results to this value?

We agree that the prior error covariance, including the assumed 40% uncertainty, cannot be quantified precisely. The value was selected based on previous inverse modelling studies (Feng et al., 2022) and evaluated through a series of sensitivity experiments. These tests indicate that our results are only weakly sensitive to the assumed prior uncertainty. For example, increasing the prior uncertainty from 40% to 48% (+20%) changes the annual global methane emission estimate by less than 1%. Similar experiments involving the assumed OH uncertainty yield comparably small impacts. To address this reviewer comment, we have clarified this point in the revised manuscript.

Sensitivity tests (not shown) demonstrate that our results are largely insensitive to the assumed prior error covariance. Adjusting the a priori uncertainties by ±20% changes the annual global emission estimate by less than 1%. Similarly, increasing the spatial correlation length from 300 km to 360 km modifies the global annual emission by less than 5 Tg yr^{-1}. The impact on the interannual variability of the posterior global methane emissions and sinks, which is the primary focus of this study, is also small, generally below 5 Tg yr^{-1}.

Line 134: How does this choice of adjusting the OH fields by latitude bands affect your results? Figure 3, for example, highlights the non-physical impacts of this methodology, which results in extreme discontinuities. If for example, you had adjusted the OH fields in 15 degree latitude bands, would you have gotten similar results? It is somewhat difficult to trust any quantitative conclusions without at least some discussion of the impacts of this scaling approach.

We acknowledge that the latitude-band scaling approach can introduce artificial spatial discontinuities in the optimized OH fields. Our original choice was motivated by the relatively weak observational constraints on OH, which do not support the robust estimation of scaling factors at substantially higher spatial resolutions. To assess the sensitivity of our results to our methodological choice, we have performed an additional experiment in which the OH field is adjusted using narrower 15° latitude bands. Preliminary results indicate that using a higher latitudinal resolution only slightly

affects the inferred global annual methane emissions: except for 2020, both the emissions and their interannual variability remain well within the uncertainty range. We find that increasing the number of latitude bands for which we solve for OH does noticeably smooth the latitudinal contrast in inferred methane-sink changes between 2024 and 2019 (see plots below).

Such sensitivity is one of the reasons why, in the revised manuscript, we use the finer 10-band configuration as our control run, together with other updates including the latest version (v10) of the proxy GOSAT retrievals and a revised initial field. A discussion of this sensitivity and its implications have been incorporated into the revised manuscript.

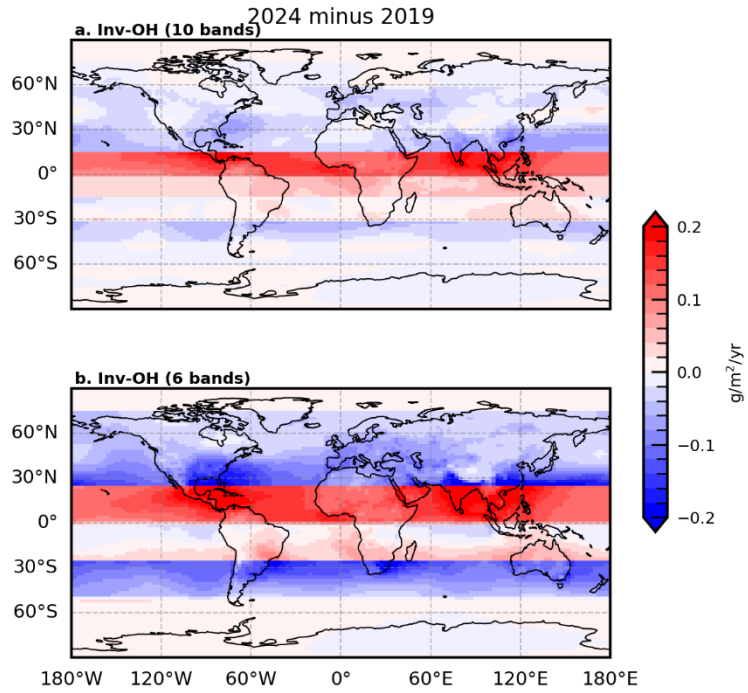


Figure 1: (a) Estimated change in the methane sink from 2024 to 2019 derived from an inversion that jointly estimates regional methane missions and monthly tropospheric OH concentrations (10 scaling factors across 15° latitude bands) using GOSAT XCH_4 retrievals and in situ surface observations. (b) Same as (a), but using six OH scaling factors across six 25° latitude bands.

Line 142: You say here that 2018 is your spin up year, but you include 2018 in many of your figures and analysis. Sometimes, however, you omit discussion of 2018 from the analysis. Since you consider this to be spin up, why are you including it in the paper?

This is a good point. The initial CH_4 concentrations at the start of 2018 are taken from our long-term inversion experiment (2010–2025) with fixed OH fields. We have now revised the initial fields, due to the use of GOSAT v10 data. While we consider the 2018 results to be helpful, we now clarify that 2018 should be treated with caution.

The initial methane concentration fields at the start of 2018 are taken from a long-term inversion (2010-2025) performed using fixed OH fields. Consequently, although the 2018 results are reasonably constrained, we regard 2018 as a transition (spin-up) year during which the system adjusts to the revised OH treatment introduced in this study.

For completeness, we retain 2018 in the figures but clarify in the revised manuscript that our analysis and interpretation focus primarily on the period from 2019 onwards. We also highlight the greater uncertainty associated with the 2018 estimates.

Line 160: This is where the inclusion of 2018 becomes confusing. According to your figure, OH in 2018 was even lower than in 2020. If you consider the results for 2018 to be meaningful, which including them in the figure would imply, then your statement here is incorrect.

We agree that including 2018 can make the interpretation of Figure 3 less clear. Although the optimised OH values in 2018 are indeed lower than those in 2020. Part of the reason is that the different bias correction scheme applied in the previous long-term inversions with fixed OH to generate the initial concentrations. Now with the consistent new GOSAT v10 dataset, such issue has been solved. However, we still caution against overinterpreting the 2018 results because they are influenced by model adjustment from the prescribed initial conditions and therefore form part of the spin-up period. We have revised the text to clarify that statements regarding temporal OH variability and extrema refer to the analysis period after spin-up (2019 onwards). We also explicitly note the status of 2018 in the figure discussion to avoid confusion.

Line 173: Your conclusions here seems entirely dependent on your choice of the size of the latitude bands for your OH scaling.

As noted above, following additional sensitivity experiments suggested by the reviewer, we now adopt the finer latitude-band configuration for OH scaling as the control inversion, replacing the previous six-band. The results show that the detailed OH adjustments depend on the chosen band width. For example, the spatial patterns of methane-sink changes between 2019 and 2024 become less pronounced across latitude bands as the band width is reduced. However, the inferred global methane emissions, methane sink, and their interannual variability are broadly consistent across the different configurations. We therefore conclude that the main findings of this study are robust to the choice of latitude-band resolution, and we have clarified this point in the revised manuscript.

Line 214: I think you need to include more details about the GOSAT data in Section 2, particularly as they relate to XCO_2. Are you using the same XCH_4:XCO_2 retrievals as in your earlier work? The discussion here would imply that, but there's no discussion of this in the methodology.

We thank the reviewer for highlighting this lack of clarity. In this study, we use the proxy XCH4 retrievals directly rather than the XCH4:XCO2 ratio retrievals employed in some of our previous studies. To clarify this point, we have revised Section 2 to make this

distinction explicit and to provide additional details on version 10 of the GOSAT data product. Accordingly, we also re-written the discussion to reflect the better agreement with the revised retrievals.

Line 218: Are the biases compared to TCCON invariant in time? In that case I could see how they wouldn't affect the interannual variability. If that's true, you should state that. Otherwise, what evidence do you have that they don't affect the interannual variations and how do you define "significantly"? That's a very imprecise modifier.

By using the new GOSAT v10 data, the bias against TCCON sites in the Northern Hemisphere has largely been reduced. The typical bias is now less than 5 ppb, except the site near Los Angeles and the two sites at northern high latitudes (>60°N). We believe that such biases, including their temporal variability, do not explain the interannual changes discussed in this study, as most of the features seen in the control run also appear in the sensitivity experiment with online bias correction. However, we agree that the term "significantly" was imprecise and have revised the text to state more explicitly that the observed bias is small compared with the inferred interannual methane variations.

Line 230: "whereas" is misspelled.

Corrected.

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