Reviewer #1

This manuscript presents an inverse modelling system for estimating global methane emissions. It is used to study the causes of the accelerated methane increase during 2020-2022. Compared with previous studies, a larger set of surface and aircraft measurements is used, extending the data coverage in south and east Asia, in addition to the use of alternative GOSAT retrieval datasets. The results highlight the role of increasing Asian emissions in the global growth rate enhancement during this period, attributed mostly to increases in agricultural emissions. The sensitivity of inversion-estimated emissions to the observational datasets is an important – although not unexpected – finding. This study points to a trade-off between African and Asian emission increases depending on the data that are used, which makes an important contribution the scientific understanding of the causes of the global emission increase.

The manuscript is well written. Provided that the few points raised below are sufficiently well addressed I see no reason to uphold publication.

We are grateful for your time to review our paper and for giving us fruitful comments and suggestions. Our replices to the comments and modifications are described below with current line numbers.

GENERAL COMMENTS

The method section describes how posterior uncertainties are quantified. However, besides posterior flux covariances and uncertainty reduction very little use is made of posterior uncertainties. How do posterior uncertainties compare with the differences that are found between the different inversions? How about the significance of the most important flux deviations from the prior that are used to explain the 2020-2022 growth rate anomaly? Some of the plots miss error bars.

We agree with your comments on the posterior errors. In order to show abosolute values of the posterior errors, we added the global totals of the errors as well as flux totals in Table 1. Furthermore, we also inserted bar plots presenting regional errors in Fig. 6. They could show how much uncertain each regional or sectoral emissions are compared to others. However, those abosolute values are small compared to the differences among the three inversions. In addition, they are also smaller than an inversion ensemble spread (e.g., Saunois et al. 2024). This indicates that these posterior errros cannot be considered as practical uncertainties of the inversion. Therefore, we did not put those posterior errors in the time series plots as error bars.

According to these modifications in Table and Figures, we added texts as below.

“The annual global totals and their integrated errors of the prior fluxes are presented in Table 1. ”

[Line 178]

"Despite such differences among the posterior fluxes, the three inversions showed the same tendency of sectoral emission changes with respect to the prior data, such as larger wetland and rice cultivation emissions, and smaller coal mining and oil/gas emissions (Table 1). The errors of those emissions were reduced with respect to the prior ones, indicating that those emission changes were constrained by observations. However, it should be noted that the posterior errors are generally smaller than the differences among the three inversions. In addition, they are also smaller than an inversion ensemble spread (e.g., Saunois et al. 2024). Therefore, those calculated posterior errors cannot be considered as practical uncertainties of the inversion." [Lines 362–368]

Increases in emissions over Africa and southeast Asia are discussed, which have been attributed increases in natural wetlands and agriculture. However, it is not clear to which extend these increases are in the a priori fluxes already. A priori emission estimates in zonal bands are presented that give some indication, but it is unclear whether those differences are representative for what is found for the regions that are used in the sectorial bar graphs.

We added the prior bars in Figs. 7 and 10. Although the prior flux data already have emission increases, they are smaller than those of the posteior ones and limited only for wetlands.

The following text was added in the main text:

"Only for wetlands, notable increases are estimated by the prior data, in which the VISIT data for 2020 were repeatedly used for 2021–2022." [Lines 515–516]

The sensitivity to observational datasets and their spatial coverage raises the question whether the size of regional observational constraints could drive the differences in the outcomes of different inversions. If Asian data are added, the importance of Asian emissions increases, if data over Tropical Africa are added (i.e. proxy-method GOSAT retrievals) the importance of African emissions increases. It could be coincidence but might also be a symptom of sampling bias. It would be useful to add a data thinning experiment to distinguish between the extra information on methane emissions that new measurements bring versus the impact of their added observational constraint.

We understand the raised question; it should be clarified before concluding which emissions should have impacted the global increase of atmospheric CH_4. However, we also think that a thining data experiment only cannot elucidate that, because the extra information the new measurements brought versus the impact of their added observational constraint are not independent of each other. Furthermore, balancing with observations in other areas would also affect the results. In addition, we should also consider the effect of the observation-model mismatch error covariance too. If we added new observations, but assigned large errors, they would not impact flux estimates so much.

Nevertheless, we designed the experiment so that observational constraints should not get too strong, which was achived by the observational weighting r_i introduced to the observation-model mismatch error covariance (Eq. 2). This works as data thining.

SPECIFIC COMMENTS

line 30: “increase” compared to what? It misses a notion of the extent to which this is expected or not given the a priori fluxes.

Thank you for pointing out that. We modified it as “increases from 2016–2019 to 2020–2022”. [Line 31]

line 35: Agreement was found between what?

We modified it as “Agreement was found in the sectoral estimates of the three inversions” [Line 36]

line 119: How do you mean 'derived'? From what?

We elaborated it by replacing “deribed but modified from” with “reduced by 8% with respect to”. [Line 126]

Equation 1: Parentheses are missing indicating the limits of the sum over i processes (that is only for a part of the equation, but it is unclear which part). Why are some processes corrected using delta-alpha and others using delta-f? This treatment makes an important but unexplained difference. Does delta-f cover grid boxes for which the corresponding f has zero emissions?

We added square brackets to make the range of the summation clearer. The reason why we mix delta-f and delta-alpha for the optimizing parameters is described in the next paragraph of Eq. (1) [lines 166–170]. To make it more noticeable, we add (delta-alpha) and (delta-f) after “the scaling factors” and “the flux deviations”, respectively. As pointed out, delta-f could change fluxes even where f has zero emissions.

Sect. 2.2: What spatial and temporal error covariances are assumed of the 1x1 degree a priori monthly and annual fluxes?

For the delta-alpha parameters, we assumed no spatiotemporal error correlation. Meanwhile, for the delta-f parameters, we made the error covariance matrix from an ensemble, which are derived from a long-term prior simulation data. In this method, not only variance but also covariances were calculated from the ensemble. However, they are localized in space by a Gaussian function to damp erroneous correlations in remote areas. Here, we also assumed no temporal correlation.

To elaborate how to construct the prior error covariance matrix, we modified the last paragraph of Section 2.2. [Lines 171–178]

line 163: How large are the wetlands, rice, soil uncertainties derived from VISIT?

We added those prior uncertainties derived from VISIT in Table 1.

line 172: How about the temporal coverage of data that have been used? Is the sampling network changing over time?

In fact, some in-situ/flask observations stopped during the analysis period. Furthermore, aircraft data are more sporadic. Such inhomogeneous data could affect flux estimates in inversion. Meanwhile, GOSAT observations are more constantly obtained, though the data availability differs by seasons. Therefore, comparing inversions that used those different types of observations independently like this study would make up for each other's deficiencies.

We added sentences as follows:

"., which could affect flux estimates in inversion." [Lines 208–209]

"Although the data availability differs by seasons, the GOSAT observations have been more constantly obtained from one year to another than the in-situ or flask observations." [Lines 250–252]

line 187: 'deemed to be comparable' within what accuracy?

We elaborated it as

"(the difference is about 0.5 ppb (Fujita et al., 2018))" [Line 202]

Equation 2: How does balancing of data constraints work out for the observational weights of surface, aircraft and GOSAT data? How do the corresponding terms in the cost function compare?

The balancing parameter of β was determined so that X^2 should be less than 1.

We modified the text about β accordingly. [Lines 236–237]

Line 307: How does the inversions performance evaluation in Figure 2 distinguish between data that are or are not used in the inversion?

Because the GOSAT inversions did not use any in-situ/flask observations, for the GOSAT inversion, the comparisons with surface and aircraft data can be considered as evaluation with independent observations. Conversely, the GOSAT data are independent for the SURF and SURF+AIR inversions.

Figure 6: uncertainty reductions are a % reduction?

Thank you for pointing out it. We added % in the caption of Figure 6.

Appendix A: Using the method that is presented reduces the likelihood of negative emissions but does not prevent that negative emissions might happen. To which extent is this still the case?

As you pointed out, this scheme cannot avoid negative values perfectly. However, unavoidably generated negative values were at most three orders of magnitude smaller than positive values in the experiments. We added this explanation at the end of Appendix A. [Lines 688–690]

Appendix B: line 644: It is mentioned that GOSAT retrievals are biased, but this need not be the case. There could also be an inconsistency between modelled surface and total column mixing ratios due to a transport model problem. I doubt that comparisons between GOSAT and TCCON show this bias. Past studies that used GOSAT struggled with this too, but concluded that the problem was probably more a model problem than a retrieval problem.

We consider that the biaseas are not caused by the model, because we see similar biases of the GOSAT inversion both at the surface and in the free troposphere (Figs. B1a and B1b). Those consistent biases suggest that a problem due to vertical transport or chemical loss in the model is less likely.

To elaborate the discussion, we modified the last part of Appendix B as

"Those differences consistently existing at the surface and in the free troposphere may not be contributed by vertical transport or chemical loss in the model. Given that in-situ and flask observations have much higher precision than satellite observations, this result indicates that the GOSAT observations have measurable biases in those latitudes. Meanwhile, the SURF and SURF+AIR inversions largely deviate from the GOSAT observations in the tropics and southern latitudes (Fig. B1c), which is consistent with the GOSAT inversion results." [Lines 703–707]

Nevertheless, further investigation is needed to clarify the cause of those differences, which is left for a future study.

Appendix C: The description of the method is clear, but it would be helpful add a map of what the resulting OH reduction looks like?

As expected from the method, the map is showing that OH reductions are concentrated in the mid-latitude, where fossil fuel emissions are largely existing. Because that OH reduction map was made based on a very simple assumption (completely correlated with CO_2 emissions reduction, only within boundary layer) and must be different from that of more probable OH reduction, we did not add the map to avoid giving a wrong impression.

TECHNICAL CORRECTIONS

line 599: "for several reasons" instead of "through several reasons"

Thank you for your correction. We modified it. [Line 655]

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