We thank the reviewer for constructive comments that help improve the manuscript. Our responses to the comments are in blue.

The paper by Liang et al. compares regional inverse estimates of methane (CH_4) surface fluxes in East-Asia for the year 2019. The driver data are GOSAT and TROPOMI satellite observations of the column-average mole fractions XCH_4. Liang et al. describe the methodology based on the GEOS-CHEM transport model and a regularized inversion. They compare inversions of GOSAT and TROPOMI XCH_4 and find good agreement for some, substantial discrepancies for other regions. Comparisons to independent data sets serve as guidance to explain the regional differences.

Scope: I am a bit puzzled of what the overall goal of the study is. Is it a budget report of East Asian methane emissions or is it an evaluation of GOSAT and TROPOMI biases? The former would require more complete error analyses, more than a year of data, and more extensive discussions of previous work. For the latter, I would argue that the manuscript lacks completeness in terms of discussing error sources (see comment below). I recommend making the overall goal of the study clearer and revising the paper in the view of that goal.

We now clarify our objective in the manuscript (abstract, introduction, and conclusion). We focus on understanding the uncertainty in posterior methane fluxes arising from using different satellite data, which is one of many uncertainty sources for an inverse analysis. We do not intend to provide a comprehensive budget report for East Asia. Our analysis is also more than just evaluation of GOSAT and TROPOMI retrieval biases, as the discrepancy of posterior methane fluxes is related to not only retrieval biases but also other factors such as data coverage. We now state in the text that "*the main objective is to assess the consistency of methane fluxes inferred from the two sets of satellite data that differ in their data coverage and regional accuracy, adding information to the uncertainty characterization of satellite-based methane emission accounting.*". We emphasize that "*the analyses are conducted with identically configured inversions to isolate the effects of observation data*".

Proxy-CH4: Generally, the main (and, I believe, conceptually limiting) error source of the proxy method (GOSAT) must be discussed more thoroughly. It is the errors of the CO_2 fields that are used to construct XCH_4 from the raw CH_4/CO_2 ratio. Any (e.g. regionally correlated) errors in the prescribed CO_2 fields (typically taken from models) will map into respective errors in XCH_4. In fact, others [Schepers et al., JGR, 2012, https://doi.org/10.1029/2012JD017549] have compared proxy and full physics methods in the early days of the GOSAT mission. They found that, in a case study for India, erroneous CarbonTracker CO_2 fields caused biases in proxy XCH_4 data [Fig. 9 and 10 and related discussion in Schepers et al.]. The paper must examine and discuss this source of error to balance the discussion of scattering induced errors of the full-physics method (TROPOMI). To the best of my knowledge, the current version of the

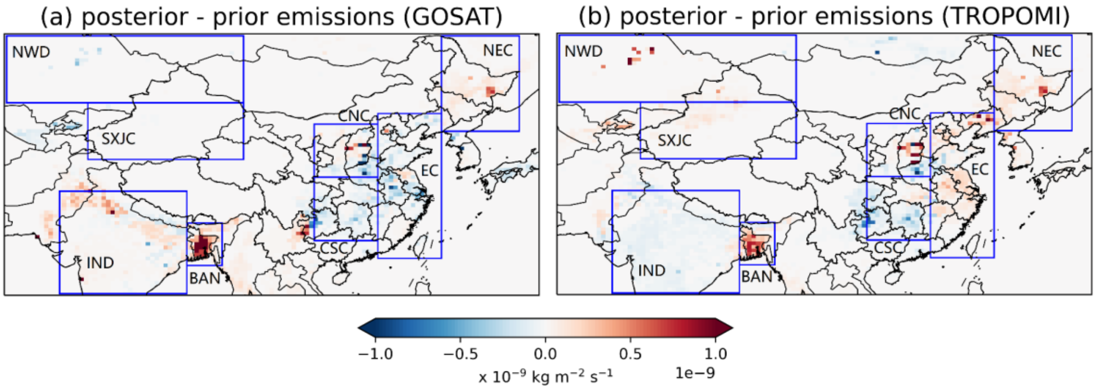
UoL proxy algorithm uses a model ensemble for CO_2-rescaling. One could try to estimate the error by looking at the spread of these (and potentially other) models in the investigated regions.

We add discussion on biases of proxy XCH_4 data in IND due to errors in the CO_2 field in Section 4.3.1. We cite Schepers et al. (2012) and Parker et al. (2015) in the discussion. We also mention broadly in the introduction section that the proxy method is subject to errors in specified CO_2 columns. We have not investigated the spread of CO_2 fields used in the UoL product as suggested, as our objective is not to assess error sources of a retrieval product. We aim to assess the impact of different retrievals on the inversed methane fluxes. Both retrieval biases and data coverage affect the inversion results. We show that retrieval biases between GOSAT and TROPOMI are relatively small over India, but they have large differences in data coverage and density.

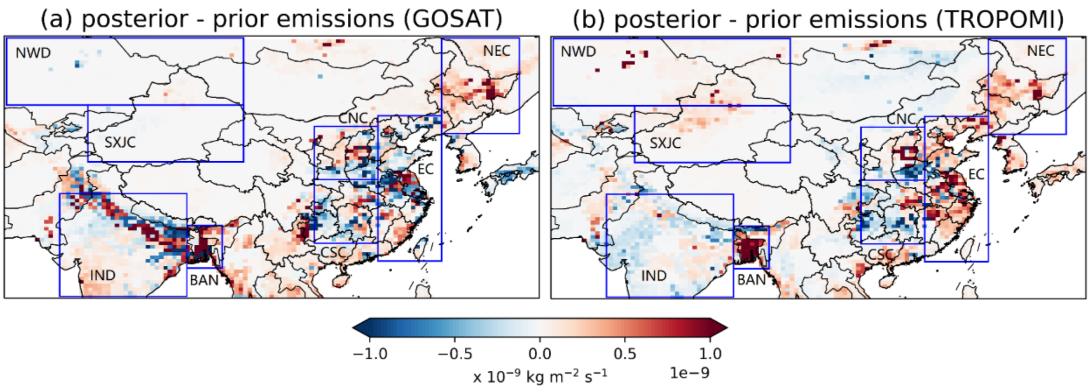
Setup of the inverse problem: I wonder about the setup of the inverse problem. If I get it right, the parameter vector contains 600 spatial elements which represent spatially distributed annual surface fluxes. I find this a mismatch of spatial and temporal scales. While the inversion is free to optimize a lot of spatial detail, any sub-annual temporal variability of fluxes is imposed. Given further, that the measurement vector contains daily XCH_4 data, I would argue that the temporal resolution of the inversion is at odds. The authors should discuss this aspect and provide sensitivity studies showing that their choice does not induce biases (e.g. by imposed seasonality).

Further, the authors have chosen to represent the prior covariance in relative terms (50%) with respect to the prior. This choice imposes that the spatial structure of posterior fluxes will be very similar to the one of the prior fluxes (simply because changing a small flux by 50% (or likewise) remains a small flux). This is clearly visible when comparing Fig. 2 and 4 (even though the log-scale in Fig. 2 needs some defiant eyeballing). The authors should clearly state the consequences of this assumption.

We perform additional sensitivity inversions to address the concerns raised by the reviewer. We first conduct a seasonal inversion, which shows similar correction pattern as the annual inversion (including the discrepancies between the GOSAT and TROPOMI inversions over East China and northern India, and the agreement over Northeast China, Central South China, and Bangladesh). The seasonal inversion in general infers smaller corrections than does the annual inversion (given the same error assumptions) because of less observations during a shorter period.



We then conduct an inversion with augmented prior errors of 100%, which again leads to similar correction patterns as the base inversion (agreement in NEC, CSC, BAN and disagreement in EC, IND, NWD, and SXJC). Results of this inversion tend to be noisier and generally have greater magnitudes because of less prior constraints, but the effect is overall small (by ~ 6 Tg a^{-1} over the whole domain).



These sensitivity inversions show that the agreements and discrepancies in posterior methane emissions between GOSAT and TROPOMI inversions are robust against perturbations of inversion setups. These results are now presented in Section 4.1 and supplementary information.

Inverse method: Equation 1 is the cost function of the inverse method. It is the classic regularization setup with a prior mismatch and a least squares measurement term where one term is scaled by a regularization parameter which the authors determine according to Figure S3. If I understand correctly, the condition on the selected regularization parameter is that the scaled least-squares term and the prior term impose equal cost. Why would one set such a condition when aiming at evaluating the information content of different data sets? In my understanding, this particular condition implies that whatever your measurement data are (be it dense or sparse, accurate or not), you force the inversion to deliver roughly the same degrees of freedom (for a given prior constraint). Figure 7 appears to confirm this conclusion: while GOSAT and TROPOMI have vastly different data density, the information content of the inversion is roughly the same. In consequence, the presented findings on degrees of freedom would not in any way represent the "natural" information content of the data but they are driven by design of the inverse method.

Generally, I would think that an L-curve method should work better for getting a regularization parameter that actually represents the information content of the data [see the cover (or chapter 4.6) of the book by Per Christian Hansen cited in the manuscript].

We now add more clarification in Section 3.2. The regularization γ is necessary here because error correlations are omitted in specified S_O (which is assumed to be diagonal for computational reasons). The extent of error correlations is different for different satellite data because of varied data density.

We have tried using L-curve method to determine a regularization parameter but found no apparent inflection points in the value range 0 ~ 1. We then determine γ following Lu et al. (2021) (section 2.4), which are also used by Qu et al. (2021) and Chen et al. (2022). The theoretical basis of the Lu et al. (2021) method is that if inversion results are consistent with specified errors ($\mathbf{S_A}$ and $\mathbf{S_O}$), $(\boldsymbol{x}-\boldsymbol{x_A})^T\mathbf{S_A^{-1}}(\boldsymbol{x}-\boldsymbol{x_A})$ should follow a chi-square distribution with n degrees of freedom and $(\boldsymbol{y}-\mathbf{F}(\boldsymbol{x}))\mathbf{S_O^{-1}}(\boldsymbol{y}-\mathbf{F}(\boldsymbol{x}))$ a chi-square distribution with m degrees of freedom.

Discussion: The posterior error bars of the satellite inversions (e.g. line 226f) are very small. I assume that they only represent the propagated measurement errors according to equation (4) (and line 185f) and that model transport errors, representativeness errors, more systematic measurement errors are neglected. When comparing the satellite-derived emissions to other studies (line 230ff), the reported error bars should be representative of the full error budget.

Our purpose here is not to compare with results from other studies or report a methane budget with comprehensively quantified uncertainties. We aim to assess the impact of different satellite observations on inversed methane fluxes. As we use identical inversion setups for the two inversions, systematic error sources mentioned by the reviewer would have similar effects on the two inversions, and therefore do not affect their comparisons. Errors derived from Eq. (4) is what we need to determine whether the difference in Fig. 3 is statistically significant, as it expresses random errors of the posterior estimates. We now clarify in the caption of Fig. 3.

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