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Comment on acp-2022-508

Anonymous Referee #2

Referee comment on "East Asian methane emissions inferred from high-resolution inversions of GOSAT and TROPOMI observations: a comparative and evaluative analysis" by Ruosi Liang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-508-RC2, 2022

Inverse methane emissions over East Asia are estimated and compared in this research for the year 2019 using data from two satellite observations, GOSAT and TROPOMI. Based on the GEOS-Chem transport model and analytical Bayesian inversion, Liang et al. developed the regional inversion framework. Comparisons at the regional level reveal consistency overall, but significant variation in some areas. The authors analyze the observations and independent measurements in further detail and argue that the variations can be explained by the data coverage and various retrieval techniques. Some arguments, however, need more analysis or are not convincing enough. Only after the authors address the following concerns can I recommend the paper to be published.

General comments

1, The overall goal of this paper is not very clear. Several key points are mixed up in this manuscript, but the authors do not break it down into individual points for this study. If the authors intend to assess the impact of the various satellite retrievals, I would suggest using one satellite but two retrieval products (for GOSAT: “proxy” v.s. “full-physics”; TROPOMI: official dataset v.s. WMFD). If the authors want to show the robustness of the inversion system they built, I suggest discussing error sources in detail and showing intermitted results.

2, In both the abstract and discussion, the authors mention the large discrepancy over certain areas in East Asia is caused by retrievals. The cost imposed by the least-squares term and the prior term, however, appears to be equal. Thus, the changes in posterior emissions are mainly driven by the inverse system but not observations.

3, The comparisons between simulations (with a priori and a posterior) and other independent measurements also indicate that increasing the emission intensity is ineffective to improve the result (see specific comment 6) in background areas. Does it imply that the model is unable to adequately capture the variations in these areas or that certain sources are missing from the same grid cell? The common problem in inverse modeling is the missing sources in a priori emission inventory. Again, if the authors aim at evaluating the emissions in China, please add more discussion on this aspect.

Specific comments

1, Line 50-55: Please add more information about other methods to derive methane emissions as well as other satellites that are currently in service for methane monitoring (Sentinel-2, GHG-sat, etc.). The introduction here can be more comprehensive.

2, Line 95-100: As far as I know, the new version of TROPOMI has already been reprocessed. And they provide the data over the ocean (glint-mode). If the authors downloaded the official operational product, I strongly suggest using the reprocessing data. The operational product comes in a variety of versions, each of which contains various errors and biases that might cause inconsistency in error analysis. Please check/specify if the data in 2019 comes from the same version.

Additionally, retrieving data over the ocean (typically retrieved under sun-glint conditions) differs from retrieving data over land. It might cause discontinuity from land to ocean. Do authors check if there are any corresponding biases in GOSAT data?

3, The diverging colormap of Figure 2 causes confusion. It is better to use a monotonically increasing colormap.

4, Line 160. The anthropogenic emission from EDGAR v4.3.2 is relatively out of date, and which has also been found that the emissions have been overestimated in many areas. Why do authors not use the later version (latest: EDGAR v6)? At least, the authors should mention/estimate known biases in EDGAR v4.3.2.

5, About Figure 4, either in GOSAT or TROPOMI inversion, the spatial differences show a strong spatial correlation between a priori and a posterior (a v.s. c and b v.s. d). Is it caused by the assumption of a priori covariance?

6, About Table 1, there are no improvements in the values of R^2. The low R^2 may imply the model lack repetitiveness in some places (considering they are background stations). Additionally, after being constrained by satellite measurements, the negative biases with the a priori inventory simply turn to positive biases, demonstrating that adjusting the emission intensity does not improve the outcomes of simulations.

7, Line 310, section 4.3.1. Figure 3(a) show higher emission corrections than (b) while Figure 6(a) displays a small variation in concentration in IND. However, the variation of XCH_4 in EC demonstrates the consistency of Figures 3 and 6. Any explanations for this?

8, section 4.3.1. How do the sampling biases in different seasons and regions affect the comparisons between GOSAT and TROPOMI?

9, Line 390, Section 4.3.3. The authors argue that the lack of observations over the ocean leads to unrealistic enhancement of XCH_4. However, there are no sources over the ocean, the strong enhancement at the southeast corner is more likely caused by the model's erroneous processes of the transport. For example, the boundary condition of the regional model is updated by the output of the global model, which may contain bugs/errors.