

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-309

Anonymous Referee #2

Referee comment on "Global distribution of methane emissions: a comparative inverse analysis of observations from the TROPOMI and GOSAT satellite instruments" by Zhen Qu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-309-RC2>, 2021

The authors performed inversion analyses of atmospheric CH₄ using column average data from TROPOMI and GOSAT. Since the TROPOMI product is at the initial stage, the retrieved CH₄ data may have substantial uncertainties. Considering those uncertainties, this study elucidated characteristics of the TROPOMI data specifically for inverse results by comparing with the GOSAT data, which are at a mature stage. The inversions and related statistical calculations they performed revealed significant discrepancies between TROPOMI and GOSAT in terms of error characteristics and information content, and much better quality of the GOSAT data at this moment. Detail information of the TROPOMI CH₄ data obtained in this study is surely useful for improving the data quality of the TROPOMI data in a near future. The manuscript is well structured and most descriptions are clear to me. I think this paper can be published for Atmospheric Chemistry and Physics after minor revisions suggested below.

L43: "Climate change action" is better than "Climate action", isn't it?

L45: Here, "GOSAT" first appears in the main text. Therefore, it should be written as "Greenhouse Gases Observing Satellite (GOSAT)"

L53-54: Like "TROPOMI (GOSAT) observes light intensity at 2305-2385 (1630-1700) nm wavelength"

L64: "inversion of a chemical transport model" => "inversion with a chemical transport model"

L89-90: "one year of data" => "one year data"

L105: TCCON first appears here. It should be "Total Carbon Column Observing Network (TCCON)". Furthermore, it is better to make a brief explanation of TCCON so that people who are not familiar in this field can understand the purpose of the use of TCCON.

L141: Why was the 4x5 degree used only here (Fig. 3)? The other analyses seem to be done with the 2x2.5 degree.

L201-202: I think this kind of pulse calculations assumes that the model is linear. Does the GEOS-Chem satisfies the model linearity?

L203: Only one-year-long inversion fluxes would have some errors attributed from the initial mole fraction field, which was optimized just by the globally uniform factor, especially for the earlier period. I think some discussion about that error is needed.

L256: How is the "35" derived?

L266: The term "averaging kernel matrix" is familiar in the satellite retrieval field, but not the case for flux inversions. Although, it is theoretically correct, it might be better to put some note to avoid confusion.

L292: The ratios of the posterior/prior non-wetland emissions in Fig. 5 show values close-to-zero or over 2 in many places (e.g., over Europe and Africa in the joint case). Does this mean that the inversion zeroed/doubled non-wetland emissions? If that is the case, are the resulted emissions reasonable?

L314: It is hard to see the effect of TROPOMI. It would be better to show sensitivity differences from those of the GOSAT inversion case in the right bottom panel of Fig. 5.

L324: Is this the same for GOSAT?

L347: Why does the GOSAT inversion show negative biases against the GOSAT observations almost everywhere? Is that contributed by the strong prior constraint to wetland emissions?

L371-372: Can you estimate magnitude of the potential errors due to the strong constraint on wetland emissions?

L386: Specify the version of EDGAR.

L392: Not clear what is "inconsistent" with the TROPOMI inversion.

L402: What is the "base" inversion?

L424: The number of 34 Tg a⁻¹ seems not consistent with Fig. 7, which looks like around 40 Tg a⁻¹. Furthermore, I am not sure why the joint inversion increased the emissions estimate, as each inversion showed downward estimates from the prior estimate.