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Comment on acp-2022-155

Anonymous Referee #2

Referee comment on "Satellite quantification of oil and natural gas methane emissions in the US and Canada including contributions from individual basins" by Lu Shen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-155-RC2, 2022

This work by Shen et al. uses TROPOMI XCH4 retrievals to quantify oil/gas emissions at high GEOS-Chem resolution of 0.25 by 0.3125 degree. It follows the optimal estimation framework with GEOS-Chem as forward model developed in the TROPOMI-Permian study by Zhang et al. (2018), GOSAT/in-situ-global study by Lu et al. (2021), GOSAT/in-situ-North America study by Lu et al. (2022), and TROPOMI-Mexico study by Shen et al. (2021). This work also presents significantly improved spatial resolution of emission estimates and detailed basin-level emission comparison with previous quantification by airborne measurements. It is recommended for publication in ACP after addressing the following issues.

General comments:

- Some languages are still reminiscence from previous version of the manuscript. For example, “see Methods for more details”, whereas there is no “Methods” section. The first three paragraphs of section 3 is largely redundant.
- It is suggested to clarify and maybe expand section 2.6 on the ensemble uncertainty analysis, which seems to be an advance from previous GC analytical inversion studies from the group. How does this “posterior uncertainty” compare with the classic posterior uncertainty calculated from equation 4? Where does the number 2400 come from at line 178? Posterior emission at each grid cell drawn from Gaussian PDF 100 times and then multiplied by 24?
- Section 5 gives a novel way of quantifying TROPOMI’s capacity to constrain basin-scale emissions. However, the posterior uncertainty threshold of 30% seems arbitrary and presumably closely related to the prior uncertainty assumed (50% here). Equation 8 may be misleading as it shows that the posterior relative uncertainty is driven by basin-total emissions and satellite coverage only. Consider adding prior uncertainty as a predictor, or using the relative reduction from prior error to posterior error.

Specific comments:

Lines 112-115: is this the "specification of boundary conditions" mentioned in lines 91-92? It is not very clear what the "vertical fields" are, and how GC CH4 fields are corrected exactly. Was GC CH4 scaled every day, so that the mean of the boundary grids of the North America domain matches the mean of TROPOMI pixels within a buffer zone?

Line 122: it implies that the time variation of CH4 emission at each grid cell is not considered. Is constant emission assumed throughout the study period? Please clarify.

Line 135: this equation implies that the model is linear so the jacobian (K) can be calculated only once. A linear forward model is essential for this framework given the cost of generating K. It is suggested to add a reference or calculations to justify that GC CH4 simulation is linear.

Line 136: it is suggested to provide more information on how K and S_o are constructed and how the computational challenges are solved, given the very large number of observations (7e6).

Line 154: suggest to comment on the rationale of choosing this over the L-curve approach in some previous works.

Line 169: it is "sigma_i,nation" that refers to the error standard deviations.

Line 182: suggest emphasize that this is "topography-corrected" XCH4 in the text.

Line 220: is 20% the sum of the first 9 largest O/G basins' uncertainty, or the mean of them?

Line 269 and Figure 4: is R2 from the fitted ordinary least squares line between this work and inventory/field work? Different fitting lines may give different R2. Pearson correlation coefficient might be more proper here.

Line 279: update the "Methods" section.

Line 280-281: please clarify/confirm what is “error variances weighted by the corresponding error covariances”.