Geoscientific Model Development Discussions

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Comment on gmd-2021-238

Anonymous Referee #1

Referee comment on "How well can inverse analyses of high-resolution satellite data resolve heterogeneous methane fluxes? Observing system simulation experiments with the GEOS-Chem adjoint model (v35)" by Xueying Yu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-238-RC1, 2021

Yu et al present a series of Observation System Simulation Experiments (OSSEs) to test how well methane emissions from North America can be resolved using TROPOMI satellite data. They consider different inversion approaches in the presence of errors in the prior spatial distribution, and model transport error. Overall, the manuscript is well-written and presented although I have a few comments and questions that might be addressed.

General comments:

The authors analyse biases between posterior and truth at two scales, grid-scale and whole domain. In general, whole-domain performance exceeds grid-scale performance. This is not surprising given the sensitivity of observations to any single grid cell vs the domain as a whole and errors in the spatial distribution. Analyses are presented for individual source sectors but I am curious as to how the spatial overlap between sectors affects this. Could the authors comment on spatial scales at which posterior biases of the source total approach that of the domain-wide bias? i.e. through successive coarsening of the posterior grid scale solutions. Some discussion of certain regions is provided (e.g. Bakken, Permian), but it might be useful to provide a more direct analysis over these key emission areas.

One key feature of North America is the concentration of CH_4 sources (and prior errors) in the East, but the majority of TROPOMI retrievals are in the West. Is there any value in analysing the skill of each inversion in those areas which are well-observed by the data separately to those areas that are not?

The discussion on posterior error reduction and correlation with the true error seems specific to the approximation of the posterior error used in the 4D var approach. This should perhaps be made clearer in the abstract and other points in the text.

Specific comments

P8, Section 2.4: Does the approach also optimize boundary conditions? For an OSSE this obviously isn't really the focus, but the transport error simulations change the treatment of boundary conditions which presumably would affect the results if boundary conditions are not optimized.

P9, L204: What is the impact of the imposed 200 km prior error correlation length-scale on derived emissions at grid-scale? To what extent would this affect your results, assuming the true error correlation is different?

P9, L207: Do the authors account for off-diagonal observation error covariances? The lack of description suggests not. That may be an issue for the high-resolution TROPOMI data. Furthermore, do the model-transport error simulations exhibit spatial error correlation structures? Could the inversion performance be improved by accounting for these in the off-diagonals of the observation error covariance matrix?

P9. L211: Even quality controlled TROPOMI XCH4 data seem to be impacted by systematic errors due to albedo and other considerations (Lorente et al, 2021). So perhaps the uncertainties here are an optimistic representation?

P13, Fig 7: Could you define what ρ_{est} and ρ_{true} are in the figure caption?

P14, L 265: "This reflects a tendency…" Doesn't this also reflect that the satellite data are not sensitive to small sources?

P18, Section 4.3: Is the prior actually independent of the data in the V-obsguess case? In effect aren't the data being used twice to optimize the emissions? Once to create the prior and secondly in the assimilation.

P19, L398: …"it is strongly influenced by meteorological errors" Is it any more influenced by errors than other methods, and if so why?

P20, L421: "all inversions except V_flat yield modest improvements". Improvements relative to what?

P22, L503: “This arises because…the true prior emissions disparities are not randomly distributed with zero mean” If I understand correctly, this is a direct result of the way the error matrix is calculated in Eq. 2. Does this mean that this caveat or mismatch is only applicable to the 4D-var approach and the posterior error approximation? Would one expect a different outcome using an exact Bayesian rather than approximate approach to error estimation?

P23, L508-513: Presumably if bottom-up estimates were improved sufficiently we wouldn’t need top-down approaches. Just a comment, but correcting errors in bottom-up distributions seems like a basic requirement of top-down approaches.