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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "2010–2015 North American methane emissions, sectoral contributions, and trends: a high-resolution inversion of GOSAT satellite observations of atmospheric methane" *by* Joannes D. Maasakkers et al.

Anonymous Referee #2

Received and published: 28 October 2020

This paper presents a GEOS-Chem 2010-2015 inversion of CH4 sources over North America. Sectoral emissions and their trends are optimized using a state defined by a so-called Gaussian Mixture Model (GMM), published earlier. Emissions are constrained by GOSAT observations. From an ensemble of inversions, it is found that emissions from the oil and gas sector are higher than in bottom-up reporting. Also, a slight positive trend of 0.4% per year in US anthropogenic methane emissions is derived.

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Discussion paper

The paper is well written, referencing is adequate, and the results are compared to previous studies. In that respect, the paper is a valuable contribution and deserves publication. However, I also find that the paper cleverly hides some of the bottlenecks in the set-up. I mention two major issues that require further attention/discussion.

First, the inversion is driven by 156110 GOSAT observations in the 2010-2015 time-frame. On page 9, line 29, the authors write that the mean squared difference with GOSAT is reduced by only 3.5% by optimizing the emissions. This relatively small improvement is ascribed to the already good fit using prior emissions, due to the optimized boundary conditions (global Geos-Chem inversion in which emission are optimized by GOSAT observations). This implies that the GOSAT observations are used twice: (1) in the global observations to set the boundary conditions, and (2) in the regional inversion using these boundary conditions. Although I think this is not a major issue, some mention of this drawback is needed (other approaches have been developed to circumvent this issue, e.g. Rödenbeck, C., Gerbig, C., Trusilova, K., and Heimann, M.: A two-step scheme for high-resolution regional atmospheric trace gas inversions based on independent models, Atmos. Chem. Phys., 9, 5331–5342, https://doi.org/10.5194/acp-9-5331-2009, 2009.) Next to this, I was surprised by the large increase in correlation with independent surface observations (r-squared increases from 0.58 to 0.81). Presumably, emissions (and their associated seasonal cycles) are adjusted such that temporal correlations increase. However, the authors provide remarkably little information. In fact, we do not get any information (other than the numbers above) about the ability of the posterior model to simulate GOSAT and surface observations. I also wonder why both data-sources are not assimilated together. Likely there is an unmentioned bias. To remedy this, I suggest that the authors present metrics/figures concerning prior/posterior mismatches with assimilated and unassimilated data.

Second, the authors mention that they developed an analytical optimization, based on 1200 model simulations. This makes it easy to explore sensitivities once the simu-

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lations have been performed. In an analytic inversion framework, the calculation of the posterior co-variance matrix is possible. However, on page 11, the authors only present a metric that indicates how well the observations constrain the emissions of particular emissions (actually, I would move this part to the method section). One aspect is missing in this analysis: It would be interesting to know what is the co-variation of total wetland emissions and anthropogenic emissions, because natural emissions (are reduced from 15.7 to 11.8 Tg per year) can only to some extend be separated from anthropogenic emissions (which increase from 28.7 to 30.6 Tg per year). The co-variance matrix would inform on this co-variance (how well can you separate these emissions?), as well as on the uncertainty reduction associated with individual emissions. Although the "sensitivity" inversions provide useful information, I think their range remains always somewhat subjective, depending on the choices made. For instance, is the range in gamma values (0.1, 0.5, 1.0) logical? Why is there no sensitivity for different/perturbed boundary conditions? In that sense, the posterior co-variance of a particular inversion is a useful additional metric that should be reported, when its calculation is feasible (which I guess is the case, given the fact that the averaging kernel matrix is explored).

Finally, I include an annotated pdf which contains minor comments and suggestions.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-915/acp-2020-915-RC2-supplement.pdf

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