Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-77-AC1, 2016

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Inverse modeling of GOSAT-retrieved ratios of total column CH_4 and CO_2 for 2009 and 2010" *by* S. Pandey et al.

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Received and published: 4 April 2016

We thank the referee for his/her useful comments. We have included the referee's comments (*italics*) and comments specific replies (AC) below. The corresponding changes made in the manuscript are written in blue below the ACs.

Anonymous Referee 1

This paper presents an inversion of CH4 and CO2 using GOSAT column retrievals and surface observations. The central theme of the paper is a comparison of flux inversions derived from an assimilation of the XCH4/XCO2 ratio (also constrained by surface observations) to "proxy" or "surface only" retrievals. The paper is suitable for publication in ACP, provided that some of the comments below are addressed.

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Discussion paper

General Comments:

- Use of bias correction. As the authors themselves note (P18, line 29), there appears to be a double counting of the surface observations in the satellite inversions, because a "bias correction" has been applied, based on previous satellite-only and surface-only inversions. In addition to this double-counting, I suggest that there are two problems with this approach:

a) any discrepancy between these two inversions is likely to be indicator of systematic model errors, which are likely to result in a relatively complex "offset" between two such inversions (indeed, this appears to be indicated in Figure 12). Therefore, the use of a linear "correction" would leave out some potentially important features un-accounted for;

AC: Previous analyses have shown that systematic errors are likely caused by a combination of errors in the transport model and the GOSAT retrievals (Monteil et al., 2013). It is better to take them out to avoid that these errors impact the fluxes. We agree with the reviewer that our approach of using a linear bias correction may leave some important features unaccounted. At the same time, using a higher order bias correction can also remove some of the information content of the GOSAT measurements. Linear bias correction is not an unsuitable choice keeping in mind the trade-off.

b) any uncertainty associated with this correction is not propagated through the inversion. Instead of imposing this correction as a hard constraint, why not include it in the inversions? Point a) above could also be addressed by disaggregating this potential error into more than two components (i.e. an intercept and a gradient).

AC: Optimizing the corrections further in the inversion will lead to a compromise between fluxes and bias adjustments. We do not trust this component of the flux adjustment, as it might be different for the proxy and ratio inversions that we want to compare. It would add further complexity to our analysis.

- Uncertainty quantification. Given the rather extensive discussion of potential biases and uncertainties associated with the retrievals and the model, the uncertainties derived in Figures 3, 4, 5 and 7 seem very optimistic to me. I think the paper would benefit from a much broader discussion about any limitations in the uncertainty quantification methods employed in this paper (preferably with reference to other methods that have been used in the literature). This should also include a discussion of the choices made about the a priori uncertainties. For example, it appears that a choice of 50% uncertainty for each grid cell for the CH4 prior was used with a temporal correlation of 3 months and length scale of 500 km. Why were these figures chosen? What might be the influence on the inversion of choices of this nature? When aggregated together, it appears that this uncertainty leads to a prior uncertainty on continental scales of about 5-10% (Figure 3). Does this seem realistic? It seems small to me, especially since it is apparently inconsistent with the outcome of the inversion for several regions. There are several systematic factors that could also strongly influence the outcome of the inversion (e.g. convection, OH uncertainty), which should be discussed.

AC: It is hard to guess the true uncertainty of the prior fluxes. We take 50% as 1σ in each grid. If the min/max values are at 95% confidence (i.e. 2σ), our fluxes range between 0 and twice the mean. The correlation lengths help to transfer the uncertainties to larger scales. Earlier studies like Fraser et al., (2013) have also used similar (5-10%) prior uncertainty on continental scales.

The balance between adjustments made to CH4 and CO2 fluxes in the RATIO inversion is also an important factor in our setup. With our present values, the Xratio measurements are twice sensitive to 1σ changes in CH4 than CO2. As the inversion adjusts the fluxes with respect to the square of model–observation mismatches in the Bayesian framework, CH4 fluxes are adjusted $2^2 = 4$ times more than CO2 fluxes (please see our reply to the 1st comment of 2nd reviewer). If we increase the CH4 prior uncertainty, this number will become even higher making the CO2 fluxes too restricted.

We have made the following update to our manuscript (Discussion Section) to address the optimistic estimates of posterior uncertainties:

"It is noteworthy that the inversions are run assuming uncorrelated measurements and a perfect transport. Also, as we are not optimizing the atmospheric sink of CH4, all the information from its budget is used to constrain the surface fluxes. Hence, the estimates of posterior uncertainties tend to be optimistic in this study. The χ^2 statistic indicates whether the assumed measurement and prior errors are statistically consistent (Meirink et al., 2008). We find χ^2/n_s = 0.93 for RATIO, 0.96 for PR-CT, 0.93 for PR-LM and 1.14 for SURF in the CH4 inversions (n_s is the number of observations assimilated in the inversion). This shows that we are not drastically underestimating the prior uncertainties in our CH4 inversions."

Minor Comment:

- P2 L5: "still consistent results are obtained". Consistent with respect to what?

"still consistent results are obtained with respect to other CH4 inversions."

- P2 L10: "original information"? I'm not sure what this means.

"Atmospheric measurements of GHGs can provide information about their atmospheric budget. Inverse modeling methods, also known as top-down approaches, have been developed to make use of this information to obtain improved estimates of surface fluxes"

- P4, Section 2: This could do with a brief overview of the chemical transport model setup (fluxes, OH fields, dynamics, etc).

AC: we have added the following to our manuscript (in method section):

"We use the TM5-4DVAR inversion modeling system. It is comprised of the Tracer Transport Model version 5 (TM5, Krol et al., 2005) coupled to a variational data assimilation system (4DVAR, Meirink et al., 2008). TM5 simulates the spatiotemporal distribution of a tracer in the atmosphere for a given set of fluxes. In this study, TM5 is run at a 6×4 degree horizontal resolution and 25 vertical hybrid sigma pressure levels from the surface to the top of the atmosphere. The meteorological fields for this offline model are taken from the European Centre for Medium-Range Weather Forecasts (ECMWF) ERA-interim reanalysis [Dee et al., 2011]."

- P5 L4:7. Are these choices largely subjective? Reasons should be given, and a discussion of the implications. See general comment above.

AC: Please refer to our reply to the general comment above.

- P5 L14: Missing full stop before The RemoTeC

- P5 L18: referred to as

- P5 L25: Use of "additional" here. . . additional to what (I presume it means in addition to the TCCON bias correction, which is described in the section below).

- P8 L1: Use of uncorrelated observation/model errors. Is it realistic to assume that the observation and model representation errors are uncorrelated in space and time? What might be the implications of this choice?

AC: A Bayesian inverse model should in principle address the correlation of observations and weight them properly. The observations are assimilated in the inversion by comparing them with model-simulated mixing ratios. Therefore, errors in the model can also create a correlation. In practice, correlations are often ignored, both because they are difficult to quantify, and properly taking them into account slows down the inversion systems by a large extent. A prominent illustration of this is given by the numerical weather prediction systems since most of them assume uncorrelated observation errors (but correlated prior errors). Inverses modeling studies of CH4 don't take them into account directly (for example, Alexe et al., 2014, Houweling et al., 2014, Monteil et al., 2013). Studies assimilating SCIAMACHY measurements implemented the binning method to reduce the impact of clustered measurements of SCIAMACHY (Houweling et al., 2014, Monteil et al., 2013). However, this is not so critical for GOSAT, as the number of soundings is lesser in amount. Also, the main goal of this study is to compare the results of different inversion methods in a consistent setup. All the inversions are done assuming uncorrelated observation/model errors, so this should not affect our results drastically.

- P8 L18: Re-word "relatively less errors" is not grammatically correct.

- P11 L9: "do not show an important seasonal dependence". This needs re-wording. I don't understand what an "important seasonal dependence" means.

- P11 L18: "information that is used" (remove comma).

- P17 L18: "poorer" rather than "lesser"

- P21 L4: Isn't it fairer to say that they "largely cancel out"? The cancellation is not 100%.

- P23 L8: Isn't 5x5 degrees quite a large area? Can this analysis be carried out over smaller scales?

AC: 5x5 degrees area provides us with a sufficient number of GOSAT retrieval around the TCCON sites.

AC: All other minor comments are addressed in the revised manuscript.

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