Atmos. Chem. Phys. Discuss., 15, C4659–C4665, 2015
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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

Interactive comment on "On the use of satellite derived CH_4 / CO_2 columns in CH_4 flux inversions" *by* S. Pandey et al.

S. Pandey et al.

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Received and published: 10 July 2015

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

1 Summary of review:

The authors outline a new method to interpret space-borne atmospheric observations of XCH4/XCO2 to infer surface fluxes of CH4 and CO2. They concurrently assimilate surface observations of these gases to help separate the information embedded in the ratio. The paper is generally good but weak in describing the method in places. Unfortunately, the newness of the method is greatly exaggerated with the technique outlined and demonstrated in a recent paper in this journal. Nevertheless, once this and other comments have been addressed I don't see why it can't be accepted for publication.

2 Specific comments

The authors advertise the newness of the method but this is deceitful. The broad methodology has been reported in Fraser et al, 2014. I'm sure details of the authors' new methodology are indeed new but they cannot claim the method is new. Their one mention of Fraser et al as being noteworthy is disingenuous at best. On a more positive note, it is encouraging that this method works well using a different transport model and inversion method (4D-Var vs MAP for Fraser et al, 2014). At the very least, these authors should discuss the similarities in their method and results with those previously reported by Fraser et al, 2014.

AC: We agree with the referee that Fraser et al. (2014) already used a joint inversion approach. However, we are the first to apply the method to the variational inverse modeling approach. We have clarified this as follows:

"We present a method for assimilating total column CH4:CO2 measurements from satellites for inverse modeling of CH4 and CO2 fluxes using the variational approach."

We have now given appropriate references of Fraser et al. (2014) by adding:

"Fraser et al. (2014) developed a method for assimilating Xratio in the MAP inversion setup coupled to the GEOS-Chem global 3-D atmospheric chemistry transport model. Similar to our findings, their OSSEs show that the assimilations of Xratio along with

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surface measurements of CH_4 and CO_2 can reproduce the true fluxes. However, there are some important differences with our study:

1. We focus on a comparison between the proxy and ratio approach and also perform a CO_2 inversion using surface measurements for calculating the model derived CO_2 fields used in the proxy approach. This way the propagation of errors from modeled CO_2 fields into proxy CH_4 measurements is also simulated. Instead, Fraser et al. (2014) add a constant or random bias to the Xratio measurements.

2. Fraser et al. (2014) report posterior uncertainties of CH_4 and CO_2 fluxes derived from their Xratio inversions. Although posterior flux uncertainties can in principle be derived from our method also, they are not reported here for computational reasons.

3. The ratio inversion system is weakly non-linear. The Fraser et al. (2014) ratio inversions assume linearity. We do a non-linear inversion using a suitable optimizer."

Section 2.1: Do the authors assume that R and B are diagonal?

AC: R is assumed diagonal and B is not. The correlation lengths used for calculating B is given in table 1 of the manuscript. We have added the following to clarify:

"We assume no prior correlation between flux categories of CO_2 biosphere, CO_2 oceanic and CH_4 total. The spatiotemporal covariance components for each categories were included in B."

"The diagonal terms of R are the squared sum of measurement uncertainty and model representation error. We assume no correlation between the measurements. Therefore, all the non-diagonal terms of R are set to zero."

Section 2.1: Not reporting a posteriori uncertainties is a major weakness of the method. How do they know that a posteriori fluxes are indeed significantly better than the a priori fluxes? I appreciate that small uncertainties is not a perfect metric but it is useful.

AC: In figure 7, we see that the mean annual posterior fluxes of ratio are closer to the

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truth than the prior. We can assume the posterior uncertainties for RATIO and PROXY are of similar order given the facts that: (1) same amount of information is assimilated in both inversions; (2) we do not introduce any prior correlation between CO2 and CH4 fluxes; (3) PROXY has measurement information coming form SURFCO2. Therefore, it is likely that the posterior fluxes from using the RATIO method have smaller uncertainties than the prior, and hence significantly closer to truth.

Section 2.2:âĂÍ Typo: assumned.

AC: The typo is corrected in the revised manuscript.

Section 2.2:âĂÍ The authors mention nothing about temporal and spatial correlations (see above comment about R and B).

AC: We have added the necessary information about temporal and spatial correlations (See our response to earlier comment about R and B).

Section 2.3: Did the authors sample the RemoTecv1.9 data for cloud-free scenes determined by small AODs and cloud fractions?

AC: We do not sample the RemoTecv1.9 data for cloud-free scenes. We have added the following to clarify:

"We do not sample GOSAT data for cloud free conditions, and therefore assimilate a rather optimistic number of GOSAT measurements. However, satellites such as Sentinel-5 will provide a comparable amount of data".

Section 2.3: Some brief details about the representation error would be useful to report in this paper rather than a simple reference to Basu et al, 2013.

AC: We have added a brief description of the model representation error calculation in the revised manuscript.

"The model representation error is the error made by our finite resolution model in simulating a sample at a specific location. Its size scales with the sub grid concentration

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variability, and is calculated using the local concentration gradients simulated by the model (Basu et al., 2013)"

Section 2.3: I'm not sure I completely follow the logic associated with the decision about not perturbing pseudo observations. It depends if they want to characterize their inversion system ability to infer fluxes.

AC: The aim of our study is: 1. To understand, in a Gaussian framework, the adverse effects of the biases introduced by a model-derived CO2 field on the posterior CH4 fluxes of a proxy inversion. 2. To understand whether the ratio inversion method can help us get better knowledge of the CH4 fluxes in regions where the proxy method doesn't perform well.

As we explained already in the manuscript, our choice of not adding noise to the proxy and ratio measurements does not affect the comparison between the two methods. If we perturb the pseudo measurements with noise according to the data covariance matrix R, we will have to do several inversions with different noise realizations to catch the mean behavior. This multi-inversion mean would correspond to the results of a single inversion without noise. For this reason we do not perturb the data.

Section 2.4: The authors do not clearly explain in the abstract or elsewhere why their RATIO methodology uses the surface data. They do not explain why they are using these data.

AC: We have explained this in section 4 (paragraph 1):

"The method requires assimilation of surface measurements of CH4 and CO2 as an additional constraint, since a ratio alone is not sufficient to independently constrain the CH4 and CO2 fluxes."

Section 2.4: There is no mention anywhere that the ratio data have a smaller systematic bias relative to the full-physics products.

AC: Full physics methane retrievals are outside the scope of our experiments. However,

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it is true that Xratio has less bias than the full physics XCO2 and XCH4 retrievals, as the scattering-related biases tend to cancel out. We followed the suggestion by the reviewer and added the following line to the revised manuscript.

"Also, Xratio is less biased and has a larger number of measurements than XCH4 and XCO2 full-physics retrievals (Fraser et al., 2014)"

Based on the remainder of the paper it is not clear why the paper title, abstract etc is focused on inferring CH4 fluxes even though the method clearly has a capability to infer CO2 fluxes (see section 3.4).

AC: We agree with the referee and we changed the title of the paper to:

"On the use of satellite-derived CH4:CO2 columns in a joint inversion of CH4 and CO2 fluxes"

Discussion: There is a paragraph apologizing for not reporting uncertainties, which is clearly not good enough. Maybe they could compare/contrast the reporting of uncertainties from other methods.

AC: As outlined in the manuscript, this lack of posterior uncertainties in our variational approach is caused by the non-linearity introduced by the ratio method. However, the lack of posterior uncertainties in our synthetic experiment is partly compensated by the fact that we know the true fluxes. Furthermore we make the not unreasonable assumption that the posterior uncertainties of the RATIO and PROXY methods are of similar magnitude. The reason is that they make use of the same observational and a priori constraints. However, we agree with the referee that further discussion is needed. Therefore we have added a paragraph to the discussion section (please refer to our reply of the first specific comment).

Reference: Fraser, A., Palmer, P. I., Feng, L., Bösch, H., Parker, R., Dlugokencky, E. J., Krummel, P. B., and Langenfelds, R. L.: Estimating regional fluxes of CO2 and CH4 using space-borne observations of XCH4: XCO2, Atmos. Chem. Phys., 14, 12883-

12895, doi:10.5194/acp-14-12883-2014, 2014.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8801, 2015.

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