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Atmospheric Chemistry and Physics
Discussions

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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We thank the referee for his/her 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:

This paper presents an implementation of a method for assimilating the ratio between satellite observed total column methane and carbon dioxide, which is in some ways

more robust than the standard proxy method, which is plagued by the uncertainty of the model-derived XCO2, while maintaining the larger number of measurements associated than are left with a full physics retrieval. Overall the paper is well written and the results are well-presented, and the method seems to hold some promise. It would be a more interesting study had they chosen to assess real measurements in this study rather than simply testing the mathematical framework using pseudo-data, especially as the approach is not entirely new (see Fraser et al., 2014, who did use real measurements), and the fact that there is now a long record of GOSAT measurements available. I'm sure this was a decision guided by publication strategy rather than scientific merit. but it does detract from the potential impact of the study. The experimental design seems to overstate the capabilities of the satellite measurements due to a variety of choices (not perturbing the pseudo-measurements, using "true" fluxes derived using the same transport model, and possibly using a truth derived from satellite measurements, although this last point is not clear). These need to be addressed and potentially rectified. Despite these misgivings, the study is appropriate for publication in ACP once the following points have been addressed.

2 Substantive points:

As mentioned by a previous reviewer, it seems that overall the newness of the method is overstated, given that Fraser et al. have very recently published a similar approach in the same journal. Given the similarities, the relative newness of the present study should be better framed in context to this already published work, and, if possible, the approaches and results should be compared. Of course this would be easier if this study had used actual measurements in addition to testing the concept with pseudo-measurements.

AC: Paul Palmer has also raised this issue in his review. We have included a more elaborate comparison between our method and the Fraser et al. (2014) approach in the revised manuscript. Please refer to our replies to his first specific comments.

The performance of the inversion under these conditions is almost certainly overly optimistic. Adding a purely Gaussian noise to the "true" fluxes which were derived by the same model is almost too easy a problem: The truth is clearly statistically compatible with the prior assumptions, and the difference is very well-behaved, with no systematic differences.

AC: We define our pseudo truth fluxes on a 4 x 6 (latitude x longitude) grid and then add a Gaussian noise, which is correlated in space and time with parameters defined in Table 1 of the manuscript. This method allows a significant deviation in the CO2 prior fluxes from the truth in regions with large uncertainties. Using a purely Gaussian case has the advantage that we know how the inversion is supposed to behave, which helped to verify the implementation of the method. Also, we don't make ourselves dependent on a particular choice of biased priors or measurements, as there is no choice that could be considered general. To address the valid point raised by the reviewer, we added the following statement to the paper:
"The performance of inversions assimilating satellite data in this study is optimistic compared to inversions using real observations as we have not introduced any systematic biases in our measurements."

I'm not entirely convinced by the argument that the pseudo-measurements do not need to be perturbed. Yes, if this perturbation is entirely Gaussian then many realizations would result in a convergence to the true result, but isn't the experiment meant to show what information can be gleaned from the measurements in only one year (i.e. not for many repeated years with identical fluxes but varying random mea- surement noise)? This does not seem valid, and also overstates the information content of the satellite measurements over those of the surface network, the latter having comparatively few measurements, but notably better measurement precision (and ac- curacy). Or have I misunderstood the purpose of the experiment? Either this explanation needs to be

fully justified, or the experiment needs to be repeated with properly perturbed pseudo-measurements.

AC: We weigh satellite and surface measurements according to their uncertainty, and therefore the difference in precision does influence their performance. Again, if you would repeat the experiment many times you would arrive at the unperturbed result. The experiments are not meant to demonstrate the performance of satellite inversions, but to compare the performance of the proxy and ratio methods.

Another question related to the "truth" scenarios: the references of Basu et al., 2013 and Houweling et al., 2014 are given, but the specific inversion from each of these studies is not given. I assume that you are using the GOSAT+flask inversion from Basu et al. and one of the SCIAMACHY+flask inversions from Houweling et al., but I can't really tell. This is relevant, as the Basu study in particular (as well as several recent studies, including a just-published GOSAT inversion intercomparison in JGR by Houweling et al.) point to the fundamental inconsistency of the CO2 fluxes derived from GOSAT and those derived from surface-based measurements. Given this knowledge, if the "truth" is a perturbed version of what is seen by GOSAT, it's hardly surprising that the satellite measurements are better able to reproduce the fluxes than are the surface measurements. This should be further discussed, laying out explicitly which inversions were the basis for the "truth" scenario. Furthermore, the choice of "true" fluxes derived from the same transport model will likely minimize the true problem of transport errors.

AC: The pseudo true fluxes used in our study are not the posterior results of TM5-4DVAR inversions done by are Basu et al, 2013 and Houweling et al, 2014. We use the prior fluxes that were also used in those studies (taken from CarbonTraker, EDGAR, GFED, etc.) as our pseudo true fluxes. Transport model uncertainties affect the performance of both inversion methods, whereas our experiments are meant to isolate their differences. We acknowledge that some of the choices we made (e.g. use the same model to generate pseudo data) ignore transport biases, but like to repeat that the main aim of this study is to evaluate the performance of the ratio method and to compare its performance to the proxy method. The use of true GOSAT data and need for bias correcting these will be the subject of a future paper.

Granted, the lack of posterior uncertainty estimates makes it difficult to compare, but assuming that the error bars are of a similar magnitude to those of the PROXY method (which may well be an overestimation, although the PROXY method explicitly does not take into account the uncertainty on the modelled XCO2), I'm not sure about how much can be read into the differences in Figs. 7 and 8. Isn't it likely that these PROXY and RATIO (and for that matter SURFGHG) perform equally well within uncertainty in most cases?
AC: Statistically the posterior of PROXY, RATIO and SURFGHG fall within the uncertainty range of each other for most regions. This has strong connections to our choice of prior fluxes. The truth is well within uncertainty range of the prior.
"Statistically the posteriors of PROXY, RATIO and SURFGHG fall within the uncertainty range of each other for most regions in figure 7."

In section 3.4 it's argued that the surface network performs significantly more poorly over Temperate North America because of the high model representation error in this region. On what is this based? Why is it higher here than anywhere else? The data records seem to be longer and the sampling better than most regions, and because it's a pseudodata experiment there shouldn't be representation problems related to boundary layer height, or other issues that would affect the surface-based inversion but not the satellite inversion. Please explain.
AC: In reality, Temperate North America is well constrained with surface measurements compared to other regions. The high model representation error in this region is the result of our concentration variability dependent model representation error, which makes sense for a model that has a too coarse resolution to represent the CO2 variability over continental USA. We have added the following to our revised manuscript:
"In Temperate North America, due to coarse resolution of the model in combination with large emission gradients, large representation errors are assigned to the simu-

lated measurements. Also, we do not take the full advantage of surface measurement coverage of this region as we use only fully processed NOAA/ESRL flask measurements."

Further to the discussion in 3.4: Is the problem with RATIO in Northern Africa its inability to distinguish the biomass burning fluxes? This was a point in Fraser et al. (2014), it might be good to include in the discussion.
AC: We are not optimizing the biomass burning fluxes in our inversions, so that should not be the reason for poor performance of RATIO in Northern Africa.

It might also be relevant to discuss the sparsity of not only surface but also satellite measurements in the tropical land regions.
AC: The reviewer is right that in applications with true GOSAT data the number of data in the tropics will be lesser than used in our experiment. To clarify this point, we added the following to our manuscript:
"As we do not filter-out measurements taken in cloudy scenes and we use medium gain measurements in our inversion, we are optimistic about the satellite coverage in the tropics compared to real-life inversions. However, it is also true that satellite measurements are an important additional source of information about GHGs concentrations in these regions."

Clarification: p8809, lines 15-19: I think I understand what is meant here with the treatment of the prior, but isn't there still a smoothing error that needs to be taken into account due to the different vertical grids of the model and the prior? (See Rodgers and Connor, JGR, 2003, if this isn't clear.) An equation here might help clarify.
AC: The same prior profile is used for generation of pseudo satellite column data and for converting the model profiles to model columns. There will be an interpolation error, but it will be same for the pseudo measurements and the model that is trying to fit the data. Therefore this error does not play a role in our experiments. However, in an application with real data, interpolation errors would play a role, but the ratio and proxy method would be affected similarly.

3 Very minor points/typos:

p8803, line 8: about methane -> about the methane
p8803, lines 15-20: rework this, the text is awkward and misleading. CSIRO is not a network, nor is NOAA/ESRL, they're organizations that operate networks.

p8803, line 27: onboard Greenhouse -> onboard the Greenhouse (although it might be better to just say GOSAT, and include the full name in the parentheses if you feel it's necessary).
p8803, line 29: constrains -> constraints
p8805, line 2: RemoteC -> RemoTeC
p8806, line 10: setup -> set up (written together it is only a noun, not a verb)
p8807, line 11: method operator -> method the operator
p8807, line 19: assumned -> assumed
p8811, line 3: form -> from
p8811, line 4 line 7: land TransCom -> TransCom land
p8811, line 12: regions -> region
p8811 line 15: postrior -> posterior
p8812, line 16: in-comparioson -> in comparison
p8812, line 22: worse -> worst
p8814, line 15: is -> are
p8814, line 17: the Fig. -> Fig.
p8814, line 19: the Sect. -> Sect.
p8814, line 22: satellites -> satellite
p8816, line 24: regions -> region
p8817, line 7: BEr -> Boreal Eurasia (either use short forms throughout, or spell it out fully)
p8817, line 23: constrain -> constraint

p8818, line 5: ratio -> the ratio
p8819, line 8: side of problem -> side of the problem
p8819, line 19: remove comma
p8820, line 1: factor 2 -> factor of 2
p8820, line 27-28: in the applications -> in applications
Figure 5 caption: fluxes deviation from the true fluxes at land Transcom regions -> flux departures from the true fluxes for the land TransCom regions
Figures 2 and 10: please change the units on the axis labels to "months" instead of C2076 "m" to avoid confusion In general.
When did XCO_2 and XCH_4 become X_{CO_2} and X_{CH_4} ? I feel like the latter is more widely used.
Also, I agree with a previous reviewer that the current title underplays the discussion of the CO2 fluxes, which play quite a large role in the discussion.
I assume that the figures relate to only the biogenic (i.e. not fossil fuel, and perhaps not fire) fluxes, but it would be good to clarify this.

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

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