Atmos. Chem. Phys. Discuss., 15, C2072–C2077, 2015
www.atmos-chem-phys-discuss.net/15/C2072/2015/

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.

Anonymous Referee #3

Received and published: 29 April 2015

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 XCO_2, 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.

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.

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 amost 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. 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 measurement 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

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comparatively few measurements, but notably better measurement precision (and accuracy). 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.

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 CO_2 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.

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 XCO_2), 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?

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

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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.

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. It might also be relevant to dicuss the sparsity of not only surface but also satellite measurements in the tropical land 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.

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

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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

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"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 CO_2 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.

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