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Interactive Comment

Interactive comment on "Inverse modeling of CH_4 emissions for 2010–2011 using different satellite retrieval products from GOSAT and SCIAMACHY" *by* M. Alexe et al.

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Interactive comment on "Inverse modeling of CH4 emissions for 2010–2011 using different satellite retrieval products from GOSAT and SCIAMACHY" by M. Alexe et al. P. Rayner (Referee) prayner@unimelb.edu.au

The authors thank Peter Rayner for his comments. The author comments (AC) can be found in blue below.

General Comments

This paper assesses the robustness of inversions of methane sources and sinks to

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the choice of satellite-derived concentration product. It uses four satellite products and tests a range of bias correction schemes on subsets of these.

The results seem to be good news. The fluxes inferred from different retrievals seem quite similar and, on average, they improve the fit to independent data compared to an inversion using only surface data.

Both results, however, lack a little context for their evaluation. “Similarity”, for example, is a relative measure. The usual yardstick is the posterior uncertainty on the fluxes themselves (e.g. Gurney et al., 2002) but Hungershoefer et al. (2010) suggested a metric of the detection limit required for policy verification. I understand that the cost of uncertainty calculation in these variational systems is very high, especially as the non-Gaussian statistics used by the authors preclude some of the more efficient methods. I suggest, therefore, that they make use of some previous uncertainty calculation to place the flux differences in context.

The case of the independent data fit is more difficult and the question sits at the edge of current research. I will flag it here mainly to stimulate discussion rather than a request for the authors to react. The fit to independent data should take account of the uncertainty in the posterior fluxes. If, for example, the posterior flux most strongly linked with an observation that was not used in the inversion is highly uncertain then it is likely that the fit to this data point will be poor. As data is added the posterior PDF for the fluxes will be refined and so, consequently, will the PDF of simulated concentrations.

So, it is certainly good news that the fit to the independent data improved with the addition of satellite data. We should, however, demand a little more. I think the right question is whether the fit improved as much as our confidence in the fit. The next question is how to calculate this confidence. If the posterior uncertainty in fluxes is calculated using the Monte Carlo techniques used by Chevallier et al. (2007) and subsequent papers the data is available from the simulated concentrations for the flux realizations. If uncertainties are calculated from an approximation to the Hessian this is

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more difficult, especially since the iterative calculation usually produces a lower bound on uncertainty.

If the authors have Monte Carlo realizations of the posterior uncertainty for surface data and satellite-data inversions I recommend (rather than request) they look at the related realizations of simulated concentrations.

AC: The authors are currently investigating several uncertainty quantification approaches for the methane fluxes and 3D mixing ratios, among which a Monte-Carlo ensemble method similar to that discussed in Chevallier et al. (2007). As remarked by the reviewer, an additional complication is introduced by the use of semi-exponential (non-Gaussian) PDF for the prior fluxes. The work is in progress, and further investigations are needed before the results can be published. We will mention this in the conclusions section of the revised manuscript.

I have one other concern with the paper. The fluxes themselves are discussed very little in the paper. There is, for example, no comment in the abstract on the fluxes themselves, only their sensitivity. I hope this means there is another paper coming on the physical interpretation of these results. If not, I request a little more discussion of what we have learned about methane fluxes from the study.

AC: The revised version of the manuscript will include an extended discussion of the inverted regional methane fluxes, particularly over North America, which suggest larger CH4 emissions related to fossil fuels (natural gas) compared to bottom-up inventories (EDGAR). This result is consistent with a recent comprehensive review by Brandt et al. (Science, 2014), which points to a systematic underestimation of CH4 emissions from North American Natural Gas Systems in bottom-up inventories.

Furthermore, we will expand the discussion of the derived emissions over tropical Africa and include a comparison with other wetland inventories (e.g., Melton et al. 2013).

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Finally we will also include some discussion of the regions where derived emissions show differences (e.g., South America and India).

Specific Comments
The paper is well written. My only editorial note is:
P11498L6 Consecutive “of”.

AC: Minor editorial comments will be addressed in the revised version of the manuscript.

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