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Atmospheric Chemistry and Physics Discussions

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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General Comments

this paper assesses the robustness of inversions of methane sources and sinks to 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 datapoint 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 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.

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.

Specific Comments

The paper is well written. My only editorial note is

P11498L6 Consecutive "of".

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