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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating global and North American methane emissions with high spatial resolution using GOSAT satellite data" *by* A. J. Turner et al.

Anonymous Referee #1

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Review of "Estimating global and North American methane emissions with high spatial resolution using GOSAT satellite data"

Overview: Turner et al.'s manuscript uses GOSAT methane observations in combination with a model framework to invert for optimized methane fluxes. They perform a global optimization, and then perform a higher resolution inversion over North America. Overall this paper is appropriately placed in ACP, well written, and contributes to our understanding of global and North American methane emissions. I do have some concerns I outline below. I think the author's should be able to address many of the concerns rather easily, and at that point I would welcome publication in ACP.

Important Issues: Attribution. As currently written, the manuscript implies stronger attribution than actually is possible in the observing-model framework presented. In the abstract and conclusion it is very strongly stated that "We attribute ....". In fact, the model-inversion framework does the attribution. Importantly, this relies on the prior distribution of source types, and the assigned uncertainty. As the author's state later in the manuscriptâĂŤdepending on how one constructs the uncertainty either oil/gas or livestock are 'attributed' as the largest discrepancy in the US. The author's should clearly state in the abstract and conclusion that attribution is purely a model product, and relies on accurate spatial distribution of prior sources (or at least distribution of dominant sources) and is dependent on uncertainty assigned by the author's with expert judgment.

Representation error. GOSAT has a footprint with a diameter of $\sim$10.5 km. The GEOS-Chem model is either being run at 4x5 or $\frac{1}{2}$ by 2/3 degree resolution ($\sim$50 x50 km). The author's never discuss how they address the mismatch between the simulated column enhancements and the observed. Importantly, in regions of strong sources (and topography) and therefore strong XCH4 gradients, observations at 10.5km can and will see signals averaged out at 50x50km (even worse at 4x5 degrees). How are the authors dealing with this? What are the implications of this? In particular given the non-smooth nature of methane emissions from anthropogenic sources, the resolution of these model runs may not be sufficient to constrain fluxes at the uncertainties reported.

Transport error. When linking atmospheric observations to fluxes, atmospheric transport is the key integral ingredient. Many regional, continental, and global studies go to great lengths to attempt to quantify the potential role of transport error and its impact on the inverted fluxes. This has not been addressed by the author's in this manuscript adequately for inverting fluxes of greenhouse gases. GEOS winds are not perfect, and the representation at 4x5 degrees and 50x50km might greatly impact the analysis and interpretation. What confidence can we have in the transport accuracy? Can we quan-

tify the uncertainty in that term and include it in the inversion? Can we test if a bias error is possible? Many of the cited regional studies over California or North America make efforts to compare different transport as a proxy to understand transport error.

Minor Issues: Abstract: Line 1-2. The GOSAT observations do not constrain the inversions, but are used in a inversion framework to constrain fluxesâĂŤplease correct wording here.

Line 3: It is confusing to see degrees and then kilometers for resolution. Since the model is run at 1/2 x2/3 at higher resolution, that should be stated here (and $\sim$50km can be in parentheses, but the $\sim$ is needed since it is only approximate).

Line 9-10: This makes it appear there is some circularity in the analysisâĂŤthe aircraft/surface data is used to address a bias and is then again used as an independent check. I don't think you can still call this an independent check at this point.

Line 12: As you cite later, this result has been found and reported previously, so should not be stated in the abstract as if a new, novel findingâĂŤit is consistent with other studies.

Line 21: The model framework does the attribution (with greater uncertainty that reported)

Line 25: "most important anthropogenic greenhouse gas" is more of a subjective statementâĂŤbetter to explicitly refer to its climate-relevant role as done in the IPCC report being cited.

Page 4499, lines 6-14. I am confused a bit here. The large spatial overlap in source types is not a problem you circumventâĂŤin fact this also limits you. Furthermore, it is misleading to imply satellite work has only focused on global scalesâĂŤyou later cite work where satellite methane observations are analyzed are much smaller scales.

4502, line 25. This seemingly implicates poor representation of stratospheric CH4 (and/or tropopause height) rather than a GOSAT artifactâĂŤcould you comment more

on this? And what are the implications in regions with significant topography, where there is variance in the tropospheric column contribution to the total column?

4506, line 5. It could be misleading to state the errors are fully characterized. There are a large number of assumptions inputâĂŤincluding uncertainty levels, lack of systematic errors, and lack of covariances, which are very important in the total error assessment and are not included in the uncertainty range presented.

4507 line 21-22. I'm not quite sure if state-scale is well-definedâĂŤconstraining California does not imply any other states could have their flux quantified independentlyâĂŤCA is very large and is on the ocean so it doesn't have much upwind sources. I would suspect a more accurate statement could be made here about the ability of GOSAT to constrain a specific spatial extent defined by the number of 50x50km boxes that can be constrained together.

4508, line16-17. This might not be quite equivalent to assuming the prior distribution is correctâĂŤbut it is close and is highly dependent on the prior distribution.

4509, section 5. It may be useful to discuss the different time frame these studies focused their observation-inversions on. The different studies have been conducted focused on different years, and one could speculate that could contribute to the differences (though I find it more likely different observing network/transport/inverse strategy has a bigger role). Miller et al., 2012 and Wecht et al., 2014 had different years of focus.

4509 17-18. This is a bit misleading as stated. The Miller study had little to no sensitivity to many regions with stronger wetland sources (such as Florida), and so pre-subtracting wetland emissions is not really an important component of the analysis in that respect (wetland emissions from Florida could be increased by multiple Tg and it would not affect the Miller inversionâĂŤthis pre-subtraction matters, but it is not a simple Tg subtraction from the net).

4510, line 15-16. This is a really important statement that needs to be made clear in

the abstract and conclusion as well.

4511 line 24. Suggest change ‘We attribute’ to something along the lines of : “The model framework attributes (with potentially larger uncertainty)”

Table 2: What does the increase in all the mean biased post-inversion mean?

Figure 7: There appears to be a plotting problem with the error bars.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4495, 2015.