

Review of “*What can we learn about tropospheric OH from satellite observations of methane?*” by Penn et al.

My sincere apologies for the delay in my review.

Penn et al. present global inversions of methane and OH using GOSAT, AIRS, and GOSAT+AIRS from 2013-2015. They find that both AIRS-only and GOSAT-only inversions have similar performance. They argue that GOSAT+AIRS-inversion is not substantially different than that GOSAT-only inversion and later argue that AIRS does not add much value to the inversion. The final argument is that GOSAT can independently constrain some aspects of methane and OH. Spatial patterns of OH do not seem well constrained. The paper is generally well written and the figures are quite clear. I have some comments below, but most of them are rather minor.

In this reviewer's opinion, the main issue that needs to be addressed is the language regarding the performance of the different satellites. Previous work (including papers from some of the same authors) have argued that the combination of SWIR and TIR observations are quite valuable. This paper seems to find the opposite. This reviewer feels that the authors should clarify some of that language as the value of SWIR+TIR is being called into question. This can be easily addressed by adding just a bit of text to explicitly state where this SWIR+TIR is, or is not, valuable. This reviewer recommends minor revisions.

## Comments

### 1.) Previous work using SWIR and TIR

The authors mention studies that developed combined products using SWIR and TIR in the past (Worden, Schneider, Kuze, and Suto). These seem to be most relevant, yet the findings from those papers are only briefly mentioned around Line 95. This is in contrast to other work that is discussed in detail in the preceding paragraphs. Do these other papers using similar data and methods reach similar conclusions?

### 2.) Choice of state vector

How do the authors decide on the state vector? They separate the state vector into wetland emissions, non-wetland emissions, and OH. Is there sufficient sectoral information to attribute fluxes to wetland and non-wetland? It does not seem like they use any observations that would allow for that sort of separation.

### 3.) Time period

Why do the authors limit the study to 2013-2015? AIRS and GOSAT both have data extending much longer. Their Jacobian is quite small once constructed and the construction can be done in parallel. 3 years is very short given the interannual variability in some of their sources and sinks.

### 4.) Prior error covariance for OH

How do the authors decide on the off-diagonal elements in their error covariance matrix for OH? It seems like the 0.1 corresponds to the 10% systematic error. But that

systematic error would not be the off-diagonal term in the error covariance matrix. That off-diagonal term should signify how OH will co-vary across years. This reviewer would expect that OH in 2015 would co-vary more with 2014 than 2013, but that is not what their matrix indicates.

### **5.) Off-diagonal elements in the averaging kernel**

Why don't the authors include the AIRS-only inversion in Figure 7? It would be interesting to see how AIRS alone performs. The authors argue that AIRS cannot resolve methane. Showing the low performance for AIRS would give the readers confidence that the other Averaging Kernels are meaningful.

Same for Figures 8 and 9.

My recollection was that Worden et al (2015) showed that there was value in having the combination of GOSAT and TES. Why does AIRS not provide any value in this study?

My main question at the end of the manuscript was about the relative value of each set of satellite data. The authors show that AIRS-only inversion performs similarly to the GOSAT-only inversion (Figs 4-6). But the authors later argue that AIRS does not add anything beyond GOSAT. I think this needs to be more clearly explained. The only real discussion I saw of this was on Page 16. I think this discussion needs to be expanded and laid out more clearly.

## **Specific Comments**

**Ordering of references:** it seems the authors have ordered the citations alphabetically. This seems atypical. I would usually expect the first paper to show something to be the first paper cited. As an example, Lines 38-39 when discussing OH. This is pervasive through the manuscript.

**Lines 52-53:** I think this should say "lifetime of CH<sub>4</sub>". I don't think the authors mean to say the lifetime of OH is 10 years.

**Lines 159-160:** How do the authors rule out a bias in GEOS-Chem's free tropospheric methane?

**Line 263:** Missing a period before "We determine...".

**Figure 3:** Figure 3 is great.

**Figure 4:** Add AIRS-only to the left panel and GOSAT-only to the right panel. It would show how an AIRS-inversion performs against GOSAT and vice versa.

**Figure 5:** Emissions in panel d are less than b and c. Using both GOSAT and AIRS results in a decrease from the prior. Why?

**Lines 386-387:** Two sentences in a row start with "Remarkably".