Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "An increase in methane emissions from tropical Africa between 2010 and 2016 inferred from satellite data" *by* Mark F. Lunt et al.

Anonymous Referee #1

Received and published: 24 July 2019

1 Overview:

Review of "*An increase in methane emissions from tropical Africa between 2010 and 2016 inferred from satellite data*" by Lunt *et al.*

Lunt *et al.* present an analysis using 7 years of GOSAT methane measurements over Africa. They use these measurements in a hierarchical Bayesian inference framework to estimate monthly methane emissions from Africa. The authors then employ a number of correlative measurements (e.g., land surface temperature and water levels in Lake Victoria) to deduce the underlying drivers of these methane emissions. Overall, I

think the work is excellent. The text is clear and reasonably concise, figures are generally high quality, and my comments are all seemingly minor. I suggest the paper be accepted pending minor revisions.

2 Minor Comments:

2.1 Bias correction term for the model or GOSAT?

The only true shortcoming I found in the paper was the lack of a bias correction term. Most papers using GOSAT data have investigated the possibility of a latitudinally dependent bias. Fraser et al. (2014), Alexe et al. (2015), and Turner et al. (2015) all included some sort of polynomial or quadratic bias correction term (although some attributed it to the model stratosphere). Others used a bias term that was dependent on air mass factor (Cressot et al., 2014), that would also lead to latitudinal differences. The authors study domain covers $\sim 50°$ of latitude, so it seems like this could be an important factor. Including this in the hierarchical inversion framework would be quite exciting.

2.2 CO_2 fields for the proxy method

It would be illustrative to also show a comparison between PR1, PR2, and a third case where the proxy retrievals are constructed using the full-physics XCO_2 retrieval:

$$XCH_{4\,proxy} = \frac{XCH_{4\,no-scatter}}{XCO_{2\,no-scatter}} \times XCO_{2\,full-physics} \qquad (1)$$

This could be included in Figure 2, I don't think it's necessary for the authors to perform an additional inversion with this retrieval. It would, however, be nice to show a retrieval

that is independent of modeled CO_2 fields.

2.3 Number of MCMC samples

Page 7, Line 10 mentions that the *a posteriori* distributions each have 2,000 samples. However, aren't there 3,697 basis functions? Does that mean there are less than one sample per basis function? Do these posterior distributions really sample the full space?

2.4 Mention 2010 LST anomaly discussion in the abstract

I think it's important that the authors mention that 2010 may be an anomalously low year for wetland emissions in the Sudd (based on their LST analysis). This paper will likely get quite a bit of attention because a number of groups are looking for trends in wetland emissions. The authors do an excellent job of discussing the nuances of their trends in the main text and conclusions, but I think there should be a short (less than one sentence) mention of this possible anomaly in 2010.

2.5 Wolfe et al.

The authors should mention Wolfe et al., *PNAS* (2018): "Mapping hydroxyl variability throughout the global remote troposphere via synthesis of airborne and satellite formaldehyde observations" and consider using this data to help constrain their OH fields in the future (would be beyond the scope here).

3 Specific comments:

Page 2, Line 23: Suggest replacing *mathematically* with *mechanistically*.

Page 3, Line 11: Suggest replacing *strong signatures* with a synonymous phrase. I typically associate "signatures" with isotopic source signatures, which are also discussed in the manuscript.

Page 5, Lines 8–12: This paragraph seems out of place, I feel like it should go before the discussion of CO_2 fields. Right now it goes $-\!\!>$ GOSAT details $-\!\!>$ proxy retrievals $-\!\!>$ CO_2 fields $-\!\!>$ impact of CO_2 fields $-\!\!>$ back to GOSAT details.

Page 5, Line 31: I think you should move the Feng et al. (2017) citation to the beginning of Line 30. It initially sounded like *you* were building some Kriged global concentration fields from the NOAA data because of this text "...coarser global run that was **fitted** to *in situ* data..."

Page 6, Line 4: Suggest replacing "prior" with "*a priori*" for consistency with the rest of the text.