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Interactive comment on "Accelerating methane growth rate from 2010 to 2017: leading contributions from the tropics and East Asia" *by* Yi Yin et al.

Anonymous Referee #1

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The authors use a range of surface and satellite observations of methane to estimate methane emissions from 2010 to 2017. They also use a combined methane-carbon monoxide-formaldehyde inversion that also uses satellite observations of formaldehyde and carbon monoxide. The study describes a range of calculations that sometimes appear to be cobbled together without any particular logical flow, almost as if two groups have written this without any proper integration. Some of the calculations are also presented in a way that makes it difficult to gain any meaningful insights. The paper would greatly benefit from a robust revision, not least to ensure the authors' key messages are easier to understand. Below I outline my substantive and minor comments.

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Discussion paper

Substantive comments

Line 54: here (or in methods) it would be useful to outline the caveats associated with the CH_4-HCHO-CO method. The method assumes correct knowledge of the underlying chemistry, e.g. the fate of the methyl and higher peroxy radicals.

Line 57: here (or in methods) is an opportunity to tell the readers about any differences in the vertical sensitivity of GOSAT, OMI and MOPITT and how they might impact the combined inversion results. Even if this is addressed in an earlier paper, an acknowledgement would be useful.

Line 63: this reader did not find anywhere in the paper any mention of the ability of this combined system to independently estimate CH_4, HCHO and CO.

Section 2.1: by using XCH_4 from the proxy retrievals the authors are assuming XCO_2. Irrespective of what XCO_2 they use, this approach will introduce an error in the posterior emission estimates, which should be acknowledged. The resulting XCH_4 data might very well agree within X% of TCCON data but this study is making statements about low and high latitude regions where there is barely any coverage from TCCON.

Line 124: does the optimisation of CH_4, CO and HCHO lead to a chemically consistent atmosphere? It would also be useful if the authors reported the methyl chloroform e-folding lifetime as a way of assessing the prior and posterior OH.

Line 139: I was baffled by the diversity of uncertainties attributed to chemical production of HCHO production and OH. Please tell the reader where these values come from. Particularly for the low OH uncertainty, given that later in the study (line 145) the authors explain the large differences between OH fields.

Section 2.2.3: From what this reader understands, the focus of the work is on the 4DVar method. To address the difficulties associated with the ease with which the posterior solution can be characterised using this method, the authors have decided to include additional inversions. This somewhat muddies the water unless the authors can convincingly show both methods produce consistent emission estimates - not just zonal mean totals. For example, is Figure 6 consistent with the 4DVar system?

Line 209: this is a bold and unsubstantiated statement that appears with no prior warning, e.g. discussion in methods. I am sure the authors could come up with competing reasons for small inter-annual variations.

Line 216: this is a critical point. Later discussions about OH do not appear to address this point.

Line 222: this diversity in results is not addressed very well in the paper and does not bode well for using the alternative set of inversions (section 2.2.3) to help characterise the 4DVar solution. This reader is less concerned about the results using the surface data than the range of results inferred from the satellite data. These satellite inversions are consistent only by virtue of their large uncertainties.

Section 4.1: what I find a bit odd is the authors' use of a four-year period (2010-2013) that includes a La Nina and a subsequent four-year period (2014-2017) that includes a large El Nino. Subtracting these two periods could potentially exaggerate the growth over the eight-year period, particularly over the tropics. Figure S12 shows the temporal changes in global methane emissions (at least I assume it shows the global values). An equivalent figure to accompany Figure 7 would be useful.

Line 288: do Gatti et al and Liu et al use consistent methods to calculate fire emissions? Otherwise, I am unclear how this statement is necessarily valid.

Lines 294-298: this statement does not make sense as written. Are the authors suggesting that variations of XCH4 and wetland extent are consistent but land models that incorporate CH4 emissions are let down by imperfect representations of various hydrological processes? And that is why models do not capture XCH4 variations?

Line 298: the authors' qualitative statement is noted. They noticed a relationship between one study and another. I am certain they can do better than that.

Line 301: tropical African emissions of methane originate mainly from the Congo Basin? That is inconsistent with previous studies. The attribution cannot be "supported" by a statement that large peatlands exist in this region. This reviewer understands from Dargie et al that most of the central part of the basin is permanently flooded in which case why would methane emissions be increasing?

Line 315: another study has estimated Chinese trends in methane are *likely* due to coal mining but is there any evidence in the multi-tracer inversion that this is true? Are the spatial distributions over China consistent with that conclusion? The authors take more time to interpret the Russian signal using spatial distributions. I encourage the authors to do something similar for tropical Africa and China.

Line 342: Hand waving.

Minor comments

Line 41-42: the statements after the first dash makes little sense to this reader.

Line 48: there have been a few studies to investigate the recent acceleration. I urge the authors to use primary references rather than Nisbet et al 2019 reference, which glosses over some of the underlying issues.

Figure caption 1: remove parentheses around Thoning et al.

Line 68: 'We' should be 'we'.

Formaldehyde is referred to as CH2O and HCHO. Please be consistent.

Line 162: posterior or a posteriori. Please be consistent.

Equations 1 to 3: the convention is to use lower case bold for vectors and upper case bold for matrices.

Line 171: reference is a bit mangled.

Line 176: ...generally capture well... This reader fails to understand the meaning of this statement.

Line 183: anchor points rather than anchoring points?

Line 185: this statement assumes that variations in the column overhead can be related to changes in the underlying surface emissions.

Line 290: typo. Anthropogenic.

Line 294: increase exponentially with temperature?

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