1 Overview:

Review of "*Methane emissions responsible for record-breaking atmospheric methane growth rates in 2020 and 2021*" by Feng *et al.*

Feng *et al.* present a brief analysis of a set of methane inversions for 2019-2022 using an EnKF and GOSAT observations. They find spatial changes in methane emissions and look at correlative data such as GRACE. They conduct two sensitivity studies using different prescribed OH levels. The authors conclude that methane emissions are responsible for the fast methane growth observed in 2020 and 2021. A crucial aspect of this work that is missing is the evaluation. There seems to be no evaluation of the results using independent observations or techniques like k-fold cross validation. The work addresses an important and timely topic but, in this reviewer's opinion, the main claims in the manuscript (that the growth is driven by emissions, not chemistry) do not seem supported by their numerical experiments. This reviewer would recommend major revisions to evaluate their results and either reframe what is actually being concluded or provide evidence supporting their conclusions.

2 Review Criteria:

1. Does the paper address relevant scientific questions within the scope of ACP? **Yes.**
2. Does the paper present novel concepts, ideas, tools, or data? **Yes.**
3. Are substantial conclusions reached? **Yes.**
4. Are the scientific methods and assumptions valid and clearly outlined? **Mostly.**
5. Are the results sufficient to support the interpretations and conclusions? **No.**
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? **No.**
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? **No. Some important references are missing.**
8. Does the title clearly reflect the contents of the paper? **Yes.**
9. Does the abstract provide a concise and complete summary? **Yes.**
10. Is the overall presentation well structured and clear? **Yes.**
11. Is the language fluent and precise? **Yes.**
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? **Yes.**
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? **No.**
14. Are the number and quality of references appropriate? **No. Missing some important references.**
15. Is the amount and quality of supplementary material appropriate? **N/A. No supplement. This reviewer feels that some of the appendices should be brought into the main text.**

3 Major Comments:

3.1 Robustness of attribution to emissions or chemistry

The major concern this reviewer has with the manuscript is that the title and central claims don't seem supported by their data. The main scientific claim (and their final conclusion) is that the record-breaking methane growth rates in 2020 and 2021 were driven by emissions, not chemistry. This claim certainly seems plausible (if not likely), but their experiments do not seem sufficient to justify that claim.

In this reviewer's opinion, the ideal way to conclude as to the relative importance of emissions and chemistry would be to include both emissions and OH in the state vector for their EnKF. That would result provide a straight forward assessment of the relative role of each process. The argument presented in this manuscript, as this reviewer interpreted it, is as follows:

- the authors conducted a global inversion at $2^\circ \times 2.5^\circ$ resolution with an EnKF from 2019-2022. This inversion assumes constant OH fields for the 3-year window. The authors find changes in the magnitude and spatial patterns of methane emissions.
- the authors compared these emission changes to rainfall, GRACE groundwater, and temperature. The largest correlations were 0.5-0.6 (representing 25–35% of the variability).
- the authors conducted a second global inversion with the same setup but reduced OH by 5% where the largest COVID changes occurred.

The authors show the difference in emissions resulting from these cases but it is not clear to this reviewer which result is better. The differences seem to be central to their conclusions as indicated in the last two lines of their abstract (*"Based on a sensitivity study for which we assume a conservative 5% decrease in hydroxyl concentrations in 2020...we find that the global increase in our a posteriori emissions in 2020 is ∼22% lower than our control calculation. We conclude therefore that most of the observed increase in atmospheric methane during 2020 and 2021 is due to increased emissions."*) but I could not discern how they concluded why one was better than the other. Specifically, it is unclear why the control calculation is the correct answer here.

3.1.1 Evaluation and/or overfitting?

Two common methods for evaluating the performance of optimization schemes are to: 1) evaluate against independent observations or 2) perform k-fold cross validation. Neither of these were included here. This is something that should be included for all their cases with an inversion analysis to ensure that one is not overfitting for a particular inversion.

3.1.2 OH is inconsistent with other work

The authors chose a 5% reduction in OH based on Laughner et al. (2021). However, Laughner et al. (2021) was a review/synthesis paper that took global mean OH changes from Miyazaki et al. (2021; doi:10.1126/sciadv.abf7460) and used them in a box model.

This reviewer wonders how large the *global* mean OH changes are in this manuscript from Feng et al.? My suspicion is that they are quite a bit smaller than what was reported in Miyazaki et al. and Laughner et al.

Additionally, the OH chemistry is highly non-linear and Miyazaki et al. discuss how OH and ozone actually increase in some regions despite the NO_x reductions. Using OH fields from Miyazaki et al. would be a much better way of testing if the OH simulated in that work impacted the methane burden.

Essentially, this reviewer does not think the OH sensitivity run designed here accurately portrays the OH changes that others have found. Data supporting the choice of OH runs used here would help assuage these concerns.

3.2 GOSAT proxy observations

The authors use GOSAT proxy observations. This means that the methane concentrations will be dependent on the CO_2 concentrations. However, it seems like the authors use CO_2 simulations with monthly emissions through 2019. Therefore the CO_2 could lead to a bias in their methane concentrations during COVID due to the reduction in CO_2 emissions. This would be most pronounced in urban areas.

This reviewer was also very confused by the description of the data used in places. For example, when describing a sensitivity study the authors mention using proxy GOSAT $X\mathrm{CH}_4$ data (Line 90) but the main inversions also seem to use proxy GOSAT data.

4 Minor Comments:

4.1 Oversight of previous work

The authors seem to have overlooked important recent literature on this topic including, for example, McNorton et al. (2022; doi:10.5194/acp-22-5961-2022) who used TROPOMI data to constrain methane emissions during COVID.

4.2 Uncertainties

The authors don't seem to have reported uncertainties. It's clear what changes are actually substantial or within the noise. For example, the abstract lists changes of -3 Tg and -5 Tg as "substantial" in the abstract (Line 18). These don't seem particularly large. The text later claims that their work is within the uncertainty of another paper (Line 119), so it would be good to see uncertainties reported throughout.

4.3 Error correlations

Where do the temporal and spatial prior error correlations come from? It seems that the authors use spatial correlation lengths of 300 km and 1 month. Are these important in the spatial patterns found here?

4.4 Introduction

This reviewer is a bit confused by the list of citations in the intro. Specifically, Lines 34-35. The authors claim there is an intense debate on the role of fast growth in 2020 and 2021. They then claim that work has shown the importance of regional anomalies in the tropics. But many of these studies are from earlier than the time period being discussed.

"The underlying reasons for these anomalous growth rates in 2020 and 2021 are currently subject to intense debate with some studies attributing most of the growth in 2020 to a reduction in the hydroxyl radical (OH) sink of methane due to global-scale reductions in nitrogen oxides due to pandemic-related industry shutdowns (Laughner et al. 2021). On the face of it, this appears to be a reasonable explanation, but recent studies have used satellite observations of atmospheric methane to reveal regional hotspots over the tropics that are responding to changes in climate and have global significance (Pandey et al. 2021; Lunt et al. 2019; 2021; Pandey et al. 2017; Feng et al. 2022; Palmer et al. 2021; Wilson et al. 2020)."

4.5 Correlative data

The authors show plots of the changes in correlative data, but don't show spatial correlations. In this reviewer's opinion, it would be helpful to show a map with the correlation between the emission anomalies and the correlative data. The manuscript currently requires the reader to make the connection themself.