

Chua et al use the GFDL CCM to understand the modeled trend in tropospheric OH over 1980 -2014 and, through using various sensitivity runs, to tease out the relative importance of the different OH drivers on these trends. Ultimately, they find that the increasing trend in NO_x emissions over the period, along with the increase in CH₄ abundance, have the largest impact on global OH trends. CO and meteorology, through it's impact on water vapor abundance, primarily impact interannual variability, although they can affect trends regionally. They also compare modeled trends in CO and NO₂ to observed trends from MOPITT and OMI, respectively, finding that the model replicates NO₂ trends well but fails to capture CO accurately. They attribute errors in the CO trends to errors in the emissions. Overall, this is a well-written paper that incrementally advances our understanding of OH variability. It is suitable for publication in ACP once the minor revisions below are addressed.

We thank the reviewer for their overall positive feedback as well as specific comments. We address each of the specific comments below:

Line 125: Since lightning NO_x is so important for OH production, a few more details about how lightning NO_x is calculated in the model should be included.

We have included more details of how lightning NO_x is calculated in the model in the updated manuscript. We now have included the following text: 'As described in Horowitz et al. (2020), lightning NO_x emissions are calculated interactively as a function of subgrid convection, as diagnosed by the double plume convection scheme described by Zhao et al. (2018b). The lightning NO_x source is calculated as a function of convective cloud-top height, following the parameterization of Price et al. (1997), and is injected with the vertical distribution of Pickering et al. (1998).'

Line 127: Are the surface concentrations of CH₄ and the other species set by latitude? What dataset do you use to constrain the values?

We use Meinshausen et al. (2017), and use a global mean value for the surface as opposed to setting by latitude.

Line 130: Should be "A summary of historical emissions ... is shown in Fig 1."

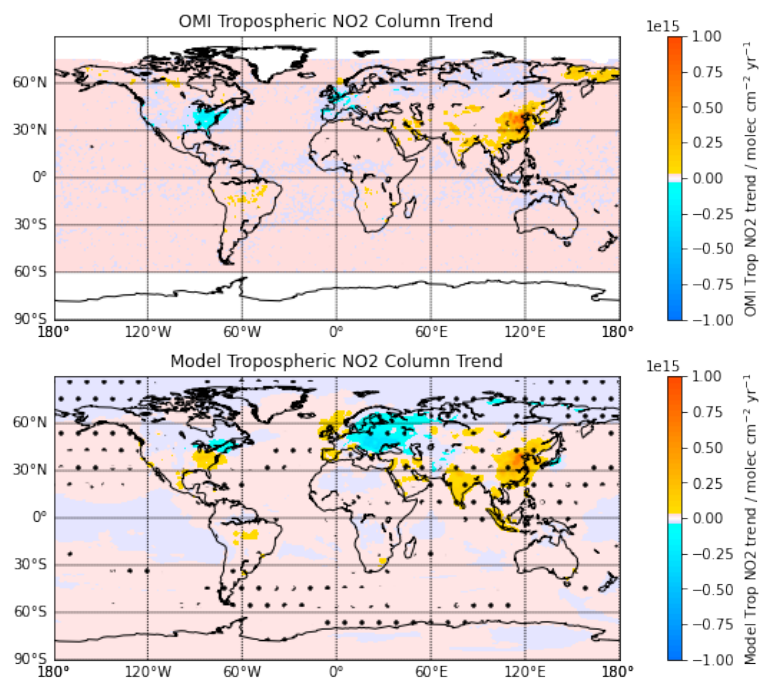
Thank you for the suggestion. We have made the change.

Line 166: You say that you don't need to evaluate the CH₄ since surface values are set as a boundary condition, but this does not necessarily translate to CH₄ being correct aloft or even at the surface, since I'm assuming you're using latitude bands to set the surface concentration. Since CH₄ plays such an important role in your results, seeming to be second only in importance to anthropogenic NO_x on a global scale, some discussion of how errors in CH₄ could affect your results is warranted.

We have added a paragraph in the ‘Implications’ section to say ‘We acknowledge that CH₄ concentrations are prescribed on the surface in the current model set-up, so this can lead to an underestimation of the surface chemical feedbacks. Including the surface feedbacks would likely amplify the modelled effects of CH₄ on [OH]. This should be further investigated in an emissions-driven run.’

Line 198: Why aren’t you using the most recent OMI NO₂ retrieval (v4.0) (Lamsal et al, 2021)? Changes in the air mass factors for the new retrieval have led to some large changes in the retrievals, particularly over highly polluted regions (see Fig. 10 in Lamsal, for example). Are these changes irrelevant for the trends you are studying?

We have revised the figure to use OMI NO₂ retrieval (v4.0) that has been processed by Goldberg et al. (2021).



Line 220: Is He et al (2020) using the same simulation you discuss here, or one similar enough in configuration that the OH trends can be compared? Also, in the citations, you list the version of He et al (2020) from ACPD. That should be updated to the finalized version.

He et al. (2020) use surface methane emissions that have been optimized so as to reproduce the observed surface methane concentrations. Their OH trends are comparable to ours (for example, in their figure 6). We have also updated the citation.

Line 300: The dip in 1992 is also evident in the met run, indicating that, for this case, CO isn't necessarily the main/driving factor. Assuming your simulation includes the effects of the Pinatubo eruption on the stratosphere, isn't this a more likely explanation for that particular dip, at least in part? There's no need to get into a discussion about this but maybe just removing the reference to 1992 would simplify things.

We thank the reviewer for the suggestion, and have implemented it.

Figures 4 and 6: For all panels in Figure 4 and for panels c and d of Figure 6, most of the text is illegible. Please increase the font size.

We thank the reviewer for the suggestion, and have implemented it.

Line 340: Should be "increases" not "increses".

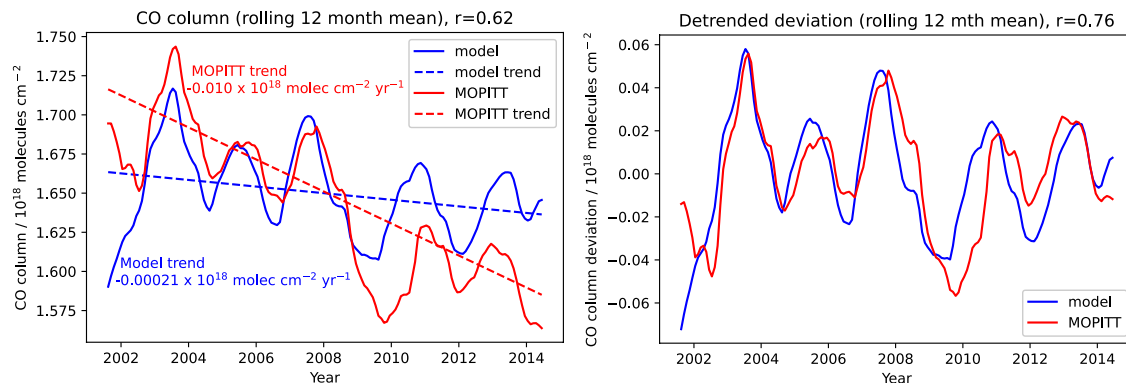
We have made the suggested changes.

Line 385 – 386: Should say "significant positive trends". Also, I think land is supposed to be India?

We thank the reviewer for pointing out the mistake and we have made the suggested changes.

Section 4.1: Since your model results suggest that CO affects global OH more through IAV than through trends, I think it also warrants some discussion on how well the model captures the CO IAV as compared to MOPITT. Otherwise, I think the MOPITT evaluation section is sufficient in highlighting the potential limitations of the impact of the modeled CO on this analysis.

We have included a comparison of model and MOPITT area-weighted rolling 12 month mean CO column from 60S to 60N. We found that the Pearson correlation is high ($r=0.62$, and $r=0.76$ from the detrended series) which suggests that the SST-driven run captures the IAV well. Also, this supports the earlier findings in the paper that the model underestimates the CO column trends compared to MOPITT. The trend (calculated using the Theil-Sen method) for MOPITT observations is -0.010×10^{18} molecules $\text{cm}^{-2} \text{yr}^{-1}$, which is about 5 times that for MOPITT which is -0.021×10^{18} molecules $\text{cm}^{-2} \text{yr}^{-1}$. We have updated Fig. 11 with these additional figures and have added a discussion on the IAV comparison.

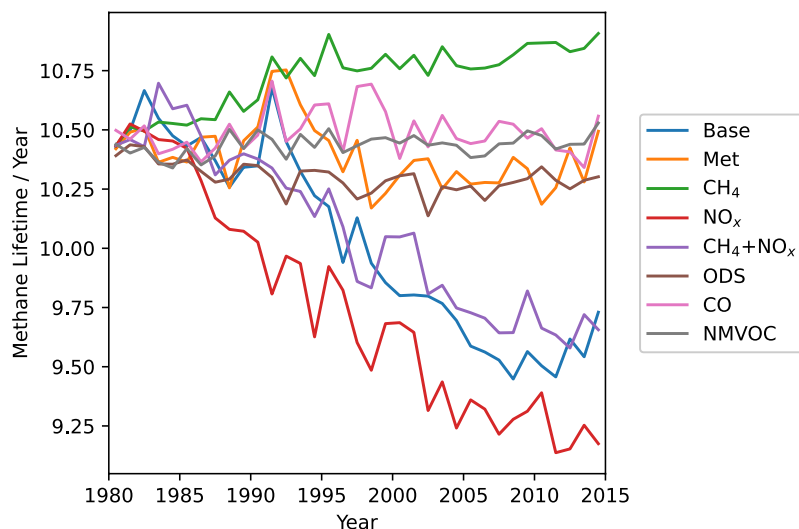


Line 444: Should be "The increasing CO trends ... lead to higher CO levels."

We have made the change.

Figure 13: Something seems off about the methane lifetime for the “Met” run. If I’m understanding correctly, for that simulation, all anthropogenic emissions were held to 1980 values, so while it’s understandable that there would be large differences by the end of the simulation, it seems unrealistic that, in 1981, the CH₄ lifetime would differ by more than 1.5 years from the baseline simulation.

We thank the reviewer for pointing the error out; there was an error in the calculation on our end, and it has been corrected. This is the updated Fig. 13.



References

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