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Comment on acp-2023-9

Anonymous Referee #2

Referee comment on "Exploring the drivers of tropospheric hydroxyl radical trends in the Geophysical Fluid Dynamics Laboratory AM4.1 atmospheric chemistry–climate model" by Glen Chua et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2023-9-RC2, 2023

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 CH_4 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 NO_2 to observed trends from MOPITT and OMI, respectively, finding that the model replicates NO_2 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.

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.

Line 127: Are the surface concentrations of CH_4 and the other species set by latitude? What dataset do you use to constrain the values?

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

Line 166: You say that you don't need to evaluate the CH_4 since surface values are set as a boundary condition, but this does not necessarily translate to CH_4 being correct aloft or even at the surface, since I'm assuming you're using latitude bands to set the surface concentration. Since CH_4 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 CH_4 could affect your results is warranted.

Line 198: Why aren't you using the most recent OMI NO_2 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?

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.

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.

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.

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

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

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.

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

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 CH_4 lifetime would differ by more than 1.5 years from the baseline simulation.

Sources:

Lamsal, L. N., Krotkov, N. A., Vasilkov, A., Marchenko, S., Qin, W., Yang, E. S., Fasnacht, Z., Joiner, J., Choi, S., Haffner, D., Swartz, W. H., Fisher, B., and Bucsela, E.: Ozone Monitoring Instrument (OMI) Aura nitrogen dioxide standard product version 4.0 with improved surface and cloud treatments, Atmos. Meas. Tech., 14, 455-479, 10.5194/amt-14-455-2021, 2021.