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Interactive comment on "Trends in global tropospheric hydroxyl radical and methane lifetime since 1850 from AerChemMIP" *by* David S. Stevenson et al.

Anonymous Referee #2

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1 Overview:

Review of "*Trends in global tropospheric hydroxyl radical and methane lifetime since 1850 from AerChemMIP*" by Stevenson *et al.*

I apologize for the delay in my review. Stevenson *et al.* present an analysis of changes in OH abundance and methane lifetime from 1850 to present using simulations from a model intercomparison (CMIP6/AerChemMIP). Specifically, they use output from 3 models: GFDL-ESM4, CESM2-WACCM, and UKESM1. The three models simulate stable OH concentrations prior to 1980 and an increase post 1980. The work then uses

a set of sensitivity simulations to diagnose the processes that control the time evolution of OH. Overall, I think the work is both useful and interesting. My main comments relate to the presentation of the interpretation. Specifically, the discussion regarding conflicts with observational MCF constraints and the brevity of the final discussion (there's only half a page of discussion after laying a solid groundwork in the methods). I feel like this could be expanded to make the work more useful to others. I would suggest minor revisions for the work.

2 Comments:

2.1 Discussion of MCF constraints

The authors seem to be arguing that these model-derived forward simulations of OH are more reliable than reconstructions. I'd be wary of framing it this way as this paper has ZERO observational constraints. On their face, the results differ from observationally constrained OH estimates and this is the interpretation from the authors (Line 3 in the abstract); however, I'm not convinced they really differ. If the authors were to include the uncertainty estimates from the Rigby et al. (2017) paper, for example, they would likely find that it bounds their results (the uncertainties are included in the supplemental data from the Rigby paper). So I think some of the "disagreement" they see is within the uncertainties. Additionally, the OH changes here do seem to agree quite well with the results from Turner et al. (2017) up until 2005. One could argue there is a divergence post-2005, but the authors don't really seem to discuss this at all. The authors seem to argue that the entire post-1980 rise differs from the MCF-derived estimates. This is curious to me.

I feel that line 3 of the abstract (*"The model-derived OH trend since 1980 differs from trends found in several studies that infer OH from inversions of methyl chloroform mea-*

surements; however, these inversions are poorly constrained and contain large uncertainties that do not rule out the possibility of recent positive OH trends.") and some of the main text discussion of the MCF reconstructions needs to be rephrased.

The authors seem to have missed two important papers from Joe McNorton as well: McNorton et al. (2016; https://doi.org/10.5194/acp-16-7943-2016) and McNorton et al. (2018; https://doi.org/10.5194/acp-18-18149-2018).

There are two other recent papers that should also be referenced and briefly discussed: He et al. (2020; https://doi.org/10.5194/acp-20-805-2020) and Nguyen et al. (2020; https://doi.org/10.1029/2019GL085706). He et al. (2020) also used the GFDL model to simulate methane from 1980 to present and find a similar time evolution of OH. Nguyen et al. (2020) look at the impact of chemical cycling on methane and OH.

2.2 Processes controlling the OH changes

It would be nice if the authors had one additional schematic type figure that summarizes their findings. There are quite a few acronyms and competing effects that make it confusing at times. Naik et al. (2013) paper had some nice bar charts showing the relative contribution of different factors to the PI-PD OH changes. This really helped follow the argument and understand what the different scenarios are doing. It seems like this would be particularly helpful to the casual reader.

As it stands, Figures 5 and 6 are the ones that diagnose the processes controlling the long-term OH changes in the model. But I can imagine many readers having a difficult time figuring out what they are supposed to take away from those figures. As it stands, they are an acronym soup.

Personally, I feel that the manuscript would greatly benefit from a final synthesis figure that summarizes the changes described in the abstract and a few additional paragraphs in the discussion section describing this.

3 Specific comments:

Lines 180–185 and 280–283: I'm confused here, I thought the Rigby et al. (2017) and Turner et al. (2017) paper showed that the problem was under-constrained. If I recall, the Turner paper showed they could fit the data without changing OH and that there were a number of valid solutions. It's not clear what the Naus et al. (2019) paper added?

Lines 198–200 and 277–280: This is the discussion that I would disagree with. The model results don't seem that different from the model results (especially if you include error bars from Rigby). You might be able to argue differences post-2005, but 1980-2005 seem be in pretty good agreement. The He et al. (2020) paper also look at this.

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