

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

Anonymous Referee #1

Received and published: 7 February 2020

This paper analyses the OH trend and methane budget in the period 1850-2014. An important conclusion is that global OH was stable in 1850-1980, after which all three models show an increase of roughly 10%. The analysis convincingly shows that emission changes in Near-Term Climate Forcers (NO_x & CO) are responsible for this behaviour.

The manuscript is relatively well prepared, but some improvements are needed, e.g. to the figures, referencing, and discussion.

Throughout the manuscript authors use “concentration”, while I think in practical calculations, tables, and plots mole fractions are shown. Better to replace concentration by

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mole fraction.

Concerning the sensitivity simulations: they are sometimes difficult to understand, but I like the calculated impact on the methane mixing ratios.

All in all, the paper is concise and to the point, and clearly demonstrates that from a modelling point of view, OH should be increasing. I miss, however, a thorough discussion on the role of climate change on OH (temperature, natural emissions, lightning NO_x.) This is certainly something that needs some more attention, also in light of earlier studies.

What also clearly misses is some validation of the model results. I understand that the individual models are (or will be) published, but to gain some confidence in the results, it would be nice to see how e.g. trends in CO are reproduced.

Minor comments:

R4: wrong. H₂O instead of HO₂.

Line 62: a sink → a dominant sink

Line 67: Wrongly suggests that ozone reacts directly with H₂O

Line 94: GFDL-ESM4 is later called GFDL-AM4, please be consistent.

Line 187: Referencing: I miss references to some recent satellite assimilation work which is relevant, e.g. <https://www.atmos-chem-phys.net/15/8315/2015/acp-15-8315-2015.pdf>

Figure 1: It would be nice to show also the modelled natural NMVOC emissions and how they changed due to climate change and variability in the different models.

Figure 2: inset: why is the GFDL-ESM4 simulation not included?

Figure 3: The use of the vertical coordinate “model level” is not acceptable.

Line 214: It would be nice to compare and discuss these new estimates to existing

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estimates. Methane is forced to observations, so the lifetime may be biased due to model biases.

Line 251: I do not see why the values of f are unreliable due to changes in halocarbon mole fractions.

Line 300: Read papers of Miyazaki et al.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1219>, 2020.

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