

Interactive comment on “Future methane, hydroxyl, and their uncertainties: key climate and emission parameters for future predictions” by C. D. Holmes et al.

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We appreciate the comments from M. Krol, S. Montzka, and J. Lelieveld. They emphasize that measurements of methyl chloroform (MCF) by both the NOAA and AGAGE networks provide excellent constraints on global mean OH and methane lifetime when averaged over several years. Furthermore, the interannual variability (IAV) in the MCF decay rate provides an upper bound of 2.3% on the global OH IAV, as described by Montzka et al (2011). One sentence in our initial manuscript apparently suggested that we disagree on these issues. This was a mistake, we do not. Indeed, we think these data provide a unique and valuable measure of the variability in global OH. We agree

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with Krol et al. and will revise our manuscript to make this clear.

The poorly phrased sentence in our discussion paper is: "Given that differences in observed MCF decay rates between the two networks are as large as their difference from CTM $\tau_{CH_4 \times OH}$ anomalies, we conclude that better understanding of the systematic differences between the observation networks is required before using them as a constraint on $\tau_{CH_4 \times OH}$ and OH interannual variability." What we meant here was that 1% $\tau_{CH_4 \times OH}$ variations seen in CTM hindcasts do not match the NOAA and AGAGE networks. The two networks also differ at the 1% level. The observations do, of course, provide an upper limit on the anomaly magnitude. With MCF concentrations about 30 ppt in 2003 and decaying at a rate of 6 ppt/yr, quantifying OH anomalies of 1% requires measurement accuracy of 0.06 ppt or better for the monthly mean concentration. This is a difficult task for the current networks and getting more difficult as the abundances dropped below 10 ppt in 2009. This is basically what Krol et al. said in their comment and we agree.

We will incorporate their minor suggestions in our revised manuscript as well.

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