

Comment on acp-2021-1021

Anonymous Referee #2

Referee comment on "OH and HO₂ radical chemistry at a suburban site during the EXPLORE-YRD campaign in 2018" by Xuefei Ma et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1021-RC2>, 2022

Ma et al. presented the OH and HO₂ radical measurement conducted at a rural site in Yangtze River Delta, China. A box model based on RACM2-LIM1 was used to simulate the radicals concentrations, which was underestimated the OH at low NO (<1 ppb) conditions. The influence of monoterpane oxidation and heterogenous loss of HO₂ on aerosol surface were tested. The authors also summarized the HOx measurement in (sub)urban environments all over the world, which gave a very nice overview of the HOx studies. Overall, the manuscript is well-written, and the investigation is scientifically sound. I strongly recommend the publication with some minor modification.

Specific comment:

Line 189, Information on back trajectory analysis of air masses is better added in the SI for the demonstration.

Figure 2: Oxidation product of HCHO and Gly diurnal pattern show peaks in morning, which is not the common sense of their formation pathway in summertime. Even for the urban site in YRD, these two species also show comparable abundances but higher levels around noontime (Guo et al., 2021). Could the authors explain more about the anthropogenic emission-related origin?

The investigation of possible influence of monoterpane on radical chemistry and ozone production is interesting and of importance. I agree with Reviewer#1 that additional discussion on the monoterpane oxidation (most probably using alpha-pinene as a representative) would benefit the manuscript. However, lacking the simultaneous measurement of RO₂ concentration and OH reactivity, one could not really draw solid

conclusion from this comparison. It's unlikely to test the chemical mechanism with the current dataset. Therefore, I suggest to minimize the discussion on monoterpane and only show the potential impact. The same applies to the closure of OH reactivity.

I would like to suggest the authors can review other literatures reporting the precursors related species, e.g. HONO, HCHO etc to the ROx production in YRD areas to strengthen the discussion, and destruction also.

In addition, comparison with wintertime OH and HOx in YRD may benefit the discussion on measurements and simulation throughout the manuscript, which can help to give the full view of the year for YRD region, e.g. Zhang et al., 2022.

Technical corrections:

Line 42: 'a' slow radical propagation

Line 47: dramatic

Line 92: needs

Line 104: measurements

Line 113: emissions

Line 115: highways

Line 121: cancel for

Line 145: were

Line 158: the time resolutionà time resolution

Line 177: wereàwas

Line 215: time series or time-series

Line 217: radicals

Line 273: sources, conditions

Line 297: reacts

Line 307: compositions

Line 309: focuses

Line 335: other processes

Line 336: conditions

Line 366: a strong

Line 371: a minor

Line 373: influences

Line 417: ranging in fivefoldàdifferent by a factor of 5

Line 448: the U.S.

Line 451: because of

Line 471: precursor

Line 478: field

Line 506: to decrease

Ref:

Guo et al. Atmospheric formaldehyde, glyoxal and their relations to ozone pollution under low- and high-NO_x regimes in summertime Shanghai, China. *Atmospheric Research*, 258, 105635, 2021.

Zhang et al. Observation and simulation of HO_x radicals in an urban area in Shanghai, China. *Science of The Total Environment*, 810, 152275, 2022.