Comments on “Uptake on HO_2 radicals onto Arizona Test Dust aerosols” by Matthews et al. (2014)

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Mattews et al. (2014) reported the first measurement of the uptake coefficient of HO_2 radicals onto airborne mineral dust particles (ATD particles), which can be of significant importance in the troposphere. The experimental work has been nicely carried out, and its atmospheric implication has been properly discussed. I have a few comments for the authors to consider:

1) Surface area of dust particles:

If I understand it correctly, the average surface area per particle, A_d, is calculated from the average diameter, *i.e.* Eq. (5) (P4236, line 20-21). The particles used in this study are poly-dispersed and on per particle base, and larger particles contribute more to the overall surface. Therefore, using Eq. (5) to calculate the average surface area per particle would underestimate A_d and therefore overestimate $\gamma(HO_2)$.

The average surface area per particle can be calculated by dividing the total surface area concentration of the aerosol by its total number concentration (*e.g.*, Tang et al., 2014), both of which can be derived from its size distribution (*e.g.*, Figure 2). The true average surface area could be 50% larger than that calculated by Eq. (5).

2) Time-dependent uptake

I find that what is present in Figure 8 is somehow contradiction with Figure 6. Figure 6 (together with Figure 5) confirms that within the experimental uncertainties the decay of HO_2 appears to be of pseudo first order; however, Figure 8 suggests that for the same time regime as in Figure 6, the uptake coefficient is time-dependent, *i.e.* the first-order decay rate is not constant over time. This may need to be further discussed and clarified.

3) Intercept in Figure 5

Due to the time required to fully mix the injector flow with the main flow in the flow tube, the y-axis intercept of Figure 5 is usually negative. However, it appears to be positive in this work, and this is attributed to a faster uptake at the initial stage (P4238, line 1-5). Could this be due to the self-reaction of HO_2 radicals? The HO_2 concentration is very low in the flow tube so that its self-reaction is not important after the flow is well mixed, but this reaction can be important before complete mixing because the HO_2 concentration in the injector flow is higher.

4) Discussion on surface saturation

The uptake coefficient was found to depend on $[HO_2]$, and two explanations are provided (Page 4239). I think the degree to which the surface is saturated (or surface coverage by HO_2 radicals) can be calculated, assuming a certain area of each HO_2 uptaken by the surface will occupy, given the measured HO_2 concentrations in the AFT and $\gamma(HO_2)$.

5) Other minor comments:

5.1 Page 4232, line 10: Though the flow tube was detailed in a previous study, it may be helpful to provide its basis information here (e.g., ID, length, linear flow velocity, and Reynolds number etc.).

5.2 Page 4236, line 4-9: I believe the methodology to operate an aerosol flow tube with changing aerosol concentrations, including data analysis, was first described by Wagner et al. (2008).

5.3: Reference: I assume (Tang et al., 2013) and (Tang et al., 2014) are actually the same paper. The authors may want to go through the citations and the reference list in the revised version.

References:

Tang, M. J., Schuster, G., and Crowley, J. N.: Heterogeneous reaction of N2O5 with illite and Arizona test dust particles, Atmos. Chem. Phys., 14, 245-254, 10.5194/acp-14-245-2014, 2014.