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Interactive comment on "Uptake of HO_2 radicals onto Arizona Test Dust aerosols" *by* P. S. J. Matthews et al.

Anonymous Referee #1

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This paper presents a set of laboratory experiments measuring the reactive uptake of HO2 to Arizona Test Dust aerosol particles using an entrained aerosol flow reactor and conversion of HO2 to OH followed by LIF detection of the OH. The methodology has been used and described previously by the authors to study HO2 uptake to other types of aerosol particles. The paper is well written, the data are compelling, and the conclusions are careful and well supported by the data. The topic continues to be important given the typical lack of agreement between models of atmospheric photochemistry and measurements of HO2. There is a growing body of literature that concludes heterogeneous chemistry of HO2 is a significant component of HOx loss, but these conclusions are often based on indirect comparisons of measurements and photochemical models and are thus in need of fundamental parameters measured and reported in this paper. I therefore suggest publication after a few minor comments/suggestions are addressed.

1. Is there a reason Saharan dust could not be used in the dust generator? The choice of ATD instead of Saharan or Gobi, etc should be discussed in a bit more detail.

2. For the RH dependence, was the humidification timescale for the ATD particles varied to see if there were a time dependence in the humidification?

3. Has the effect of aerosol particles on the LIF detection efficiency been discussed in previous papers on the apparatus? It does not appear that particles are removed prior to the excitation volume. The critical orifice likely is an effective virtual impactor, but some particles may be transmitted, no? Have the authors ruled out that the anti-correlation between ATD number concentration and HO2 is due to reactive uptake and not detection efficiency changes? I'm guessing so, but the authors should mention the paper where this issue is discussed.

4. I assume rs is the surface area weighted mean radius, based on the s subscript, but the authors should state that explicitly. Also, both the number weighted and surface weighted distribution should be presented since the surface weighted distribution is more relevant to the uptake process and calculating the gas-phase diffusion limitations.

5. In the interpretation of Figure 5, there is no discussion of the time dependence of the wall loss. Is the wall loss found to be pseudo-first order? In Figure 5, presumably the effect of wall loss has been subtracted, but was that subtraction done assuming first order behavior, and if so, is that justified? Also, it seems utilizing a smaller diameter tube might have allowed the shorter interaction times to be probed to see at what point the non linearity in time became apparent.

6. It seems from the exponential fit in Figure 8 giving non-zero limiting values that R7 as written can't be an explanation and that there would need to be a regeneration of the reactant at the very least - i.e. a catalytic loss but where the catalyst steady state

concentration is somewhat determined by the concentration of HO2.

7. In the model description, products of the HO2 uptake are ignored - i.e. a complete loss of HOx is assumed for HO2 uptake to dust. How sensitive was the model to that assumption? That is, if HO2 uptake produced H2O2 that was released to the gas-phase would there have been a noticeable change in the steady state HOx abundance due to H2O2 photolysis under these conditions?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4229, 2014.

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