

## ***Interactive comment on “Kinetics and mechanism of the uptake of N<sub>2</sub>O<sub>5</sub> on mineral dust at 298 K” by S. Seisel et al.***

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In advance to our response to the referees I would like to comment on the discussion concerning the mass dependence of the uptake coefficient.

At first some details on the mineral dust used and the calculation of the "formal grain layers": The Saharan dust samples used have a broad size distribution with a mean diameter of 15  $\mu\text{m}$ . The bulk density has been determined to be 0.93 g cm<sup>-3</sup>, the true density is 2.9 g cm<sup>-3</sup>. The BET surface area of the samples has been measured to be 49 m<sup>2</sup> g<sup>-1</sup>. The number of formal grain layers has been calculated from the bulk density, the mean particle diameter, the sample mass used (140 - 460 mg) and the diameter of the sample holder (49 mm). For the lowest mass used we end up with

5.3 formal grain layers. However, if the sample holder will be completely covered by mineral dust, one should use the true density and this results in 1.7 layers for 140 mg of dust. The samples have been prepared by spreading a suspension of the mineral dust onto the sample holder. From the above estimation it is clear, that with less than approximately 100 mg of dust it is very difficult to cover our sample holder completely. The use of an atomizer we rejected since this will change the size distribution as well as the chemical composition of the sample compared to the origin mineral dust sample.

The second point I would like to mention is, that in my opinion two problems are mixed up in the discussion: The first is the diffusion of  $\text{N}_2\text{O}_5$  into the bulk of the mineral dust sample. If this occurs, we all agree, that the determined uptake coefficient has to be corrected for the internal surface area accessible for  $\text{N}_2\text{O}_5$ . If diffusion occurs or not can be easily checked by measuring the uptake coefficient as a function of sample mass. In our case we did not find any dependence of the initial (!) uptake coefficient on the sample mass and concluded that diffusion will not occur. However, we admit our arguments may not be straightforward enough and we will revise this discussion in the manuscript. The second problem which has been addressed by the referees is the question which surface area (BET or geometric) should be used. In my opinion this problem is completely independent from the diffusion problem. The BET surface area is determined from the amount of an inert gas (usually nitrogen) adsorbed under equilibrium conditions and gives the upper limit for the surface area. In principle even a BET surface area for the uppermost layer of the sample can be calculated.

On the other hand, the geometric surface area is determined from the particle diameter. If only one layer is considered, this surface area will give the geometric surface of the sample holder. In case where diffusion into the bulk occurs, the available (geometric) surface area will be the sum of the areas of the individual particles accessible (as done for the case of salts with no internal surface). These values will always represent the lower limit for the reactive surface area. However, one of these values will only equal the actual reactive surface area by chance. Therefore, one has to decide if the lower or

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the upper limit should be used. Since in atmospheric models in general the available particle surface is calculated on the basis of the particle diameter, that is the geometric surface area, we prefer to report our values on the basis of the geometric surface area.

Nevertheless, we agree with both referees that these values are indeed upper limits and will consider this in the revised version of the manuscript.

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