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Atmospheric Chemistry and Physics Discussions

Interactive comment on “A comprehensive evaluation of water uptake on atmospherically relevant mineral surfaces: DRIFT spectroscopy, thermogravimetric analysis and aerosol growth measurements” *by* R. J. Gustafsson et al.

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Comment 1

The method of conditioning the samples is appropriate for production of material that should be characteristic of aerosol particles under very low humidity atmospheric conditions. However, it may be misleading to suggest that conditioning at 120 degrees under helium for one hour removes any species adsorbed on the surface. It is unlikely that surface bound OH would be entirely removed in this way (see for example Al-Abadleh and Grassian, Langmuir, 2003, 19, 341). Although one might argue that surface bound OH is chemically bound, not adsorbed, the extent of coverage by surface bound OH is likely to alter the adsorption properties of the aerosol.

Response to comment 1

We agree that there is likely to be residual chemically bound OH remaining after conditioning, and we shall point this out in the revised paper. As the reviewer states, our surfaces should represent a reasonable approximation to aerosol particles under very low RH. **Manuscript to be amended.**

Comment 2

The FTIR spectrum between 3000 and 3700 wavenumbers certainly does provide a useful tool for monitoring adsorbed water. However, the shape of this broad absorption is a result of multiple vibrational modes attributable to water clusters (e.g. Cantrell and Ewing, J. Phys. Chem. B, 2001, 105, 5434 and Estrin et al., J. Phys. Chem., 1996, 100, 8701, as well as the reference to Al-Abadleh and Grassian above). The absorption band in Figure 1 is similar to those seen for adsorption of water on mica and alumina, for example, and in both cases the role of clusters has been discussed in relation to the shape of both the absorption spectrum and the BET isotherm. In the case of mica in particular, it has been argued that the surface still has vacant adsorption sites at quite high relative humidities in spite of adsorption of a sufficient quantity of water to amount to multilayer adsorption if the water were to adsorb as individual molecules rather than as clusters. This clustering is discussed to some extent on the last two or three pages of the manuscript but the authors still refer to monolayer adsorption. I wonder how meaningful the concept of a monolayer is under clustering conditions. The effect of clustering becomes particularly problematic in the argument at the very end of the results section where the molecular diameter of water is used.

Response to comment 2

We appreciate the concern of the referee that discussing coverage in terms of ‘monolayers' may be misleading and requires clarification. Using formal monolayers as a measure of total water uptake is conceptually convenient, but elaboration is required. We recognise that clustering will introduce uncertainty into the interpretation of the DMA data, and this is already addressed as a possible limitation on pg 7201, line 25. However, we shall modify the text to clarify the assumptions made and their limitations. **Manuscript to be amended.**

General comment

The minor technical corrections identified by referee 2 are appreciated and the text will be amended accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7191, 2005.