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Interactive comment on "Efficiency of the deposition mode ice nucleation on mineral dust particles" *by* O. Möhler et al.

O. Möhler et al.

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We acknowledge the comments and questions from all three referees of our paper. The comments are indeed helpful to improve the manuscript. Some important points have been addressed by all reviewers. Nevertheless, we think that all referee comments deserve separate answers. This is our reply to the comments from referee #2.

Answers to specific comments:

a) As mentioned in our reply to referee #1, a definition of the relevant nucleation modes will be added to the introduction of the manuscript. We also discussed in the reply to referee #1 the criteria to judge that deposition mode ice nucleation is the predominant process in our experiments. This was concluded from the facts that (1) ice nucleation

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occurred at relative humidities with respect to water between 50 and 70 %, (2) in situ scattering measurements indicated no significant swelling prior to ice nucleation and growth, and (3) Arizona Test Dust showed almost no hygroscopic growth in laboratory studies at room temperature. From the experimental data it was not possible to directly infer the mode of ice nucleation.

b) In the final manuscript we will extend the discussion of the mineral dust origin and nature. We already mentioned that Arizona Test Dust (ATD) should be considered as a reference sample which may not be representative for atmospheric dust. In general, the two desert dust samples AD1 and SD2 show a similar ice nucleation efficiency as ATD at lower temperature but appear somewhat less active at the warmer temperature. As recommended by the reviewer we will add some more warnings that our experiments should be considered as first case studies to demonstrate and discuss basic characteristics of heterogeneous ice nucleation on mineral particles at simulated cloud conditions. More investigations, including comparison of the experimental results to process models, are certainly needed to get a better idea of the applicability of the suggested parameterisation in numerical models.

c) The calculation of the growth time is based on the fact that during growth to detectable sizes the pristine ice germs pass the transition regime between kinetically controlled growth and diffusion growth. The latter is in fact calculated according to Maxwell's treatment, as presumed by the referee. For the transition regime we applied a formulation suggested by Dahneke (1983, Simple Kinetic Theory of Brownian Diffusion in Vapors and Aerosols, in Theory of Dispersed Multiphase Flow, edited by R. E. Meyer, Academic Press, New York, pp. 97-133). For the kinetic growth rate we used a molecular accommodation coefficient of 0.5. An overview of the respective formulations can also be found in the book by Seinfeld and Pandis (Atmospheric Chemistry and Physics, Wiley, 1998). We did not include latent heat terms because they have only a minor impact on the growth rate of very small ice crystals under the conditions of our experiments. The above discussion and the reference to Dahneke's work will be

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added to page 1549 of the manuscript.

d) We appreciate any of these very interesting questions. We agree that the present paper is only a first case study and attempt to formulate deposition ice nucleation on the surface of mineral particles with only a limited number of parameters as function of the temperature and the ice saturation ratio. The remaining open questions may be addressed in future investigations. We think that among the most important points is the dependence of the activation spectrum on physical parameters like the particle size, type of mineral, surface morphology and surface modification by chemical reactions, trace gas adsorption and coating.

Technical corrections:

All technical corrections suggested by the referee will be included in the revised version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1539, 2006.