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

Anonymous Referee #2

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General Comments:

The purpose of this paper is to present new experimental data on the ice nucleating characteristics of dust aerosols under conditions appropriate to cirrus clouds in the upper troposphere. The approach is scientifically sound, and the data have been carefully gathered and scrutinized. The paper describes the experimental system and methods in a very thorough and organized manner. Of particular importance, the limitations of the methods and the uncertainties in the results were addressed openly and honestly. The paper, once minor changes have been made, will be appropriate for publication in ACP.

Specific Comments:

a. The focus of this paper is “deposition nucleation”, yet the term is neither clearly defined nor distinguished from other modes of heterogeneous nucleation. As not all readers will be familiar with the jargon used by cloud physicists, I suggest providing a brief overview in the Introduction. The different nucleation modes could there be mentioned and distinguished conceptually, perhaps within an historical context (if appropriate). Then, possibly in Section 2.6, an operational definition would be helpful. What criteria were used to judge that the ice particles formed in the chamber indeed arose via deposition nucleation, rather than via one of the other possible modes? Simply noting that the maximum saturation ratio was well below liquid-water saturation may suffice to exclude other possibilities.

b. Great attention has been placed in this paper on the Arizona Test Dust (ATD). Natural dust is a complicated matrix to begin with, but once it has been so artificially processed as the ATD has been, one has to question the relevance of the data to atmospheric aerosols. As a reference standard and as a reproducible substance for developing methods, ATD is a good choice. The danger is that numerical modelers may see the parameterization developed here and use it blindly to represent ice nucleation in the atmosphere. Perhaps some caveats or warnings could be mentioned.

c. The mass growth model referred to on page 1549 and used to develop the activation spectra needs to be explained better. As a minimum, a reference should be provided and some indication given of the assumptions and parameters used in the model. Is the model based simply on Maxwell’s treatment, or has the surface resistance been accounted for? The time for particles to grow to detectable sizes, especially at the initially low supersaturations, could be appreciable at the low experimental temperatures. Please add a sentence or two on page 1549 about the model used. Also, please state (on page 1553) the magnitude of the characteristic time by which the time series were shifted to account for the growth to detection by the OPC-PCS instrument.

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d. With all due respect to the great efforts made to perform the experiments and to write up the results so carefully, it is nevertheless natural to ask what these experiments reveal about the nucleation process itself. It is certainly worthwhile showing how an empirical parameterization can be generated from the data, but can any more physical insights be gained? The hypothesis that particle size plays a role in determining the nucleation rate, as logical as it is, is not apparently consistent with the findings. If the saturation ratio were to have continued climbing beyond Smax, would all of the particles have inevitably activated? If so, how general would such a result be for atmospheric aerosols? What physically determines the activation spectrum of an aerosol in the first place? Could we refine our understanding the molecular-scale physics by employing a simpler, model aerosol, rather than a natural one? How robustly can one establish a critical saturation ratio (S0) from uncertain measurements? The authors make a couple of good attempts to extract what they can from the data, and I do not necessarily want to see the manuscript changed just because I raise questions. Experimental data are all the more valuable when they stimulate new lines of inquiry. These experiments are successful in this regard.

Technical Corrections:

1. Numerous acronyms have been used throughout the text. Some are needed and conventional (e.g., FTIR), but some are undefined and/or unnecessary (e.g., INCA, SUCCESS). Please review the usage of all acronyms and remove any that are not clearly needed.

2. Comma usage should also be reviewed throughout the manuscript. Here, I indicate a few examples where new commas would help the reading of the text: Page 1542, Line 2, after “droplets”; P. 1544, L. 6, after “volume”; P. 1544, L. 20, after “chamber”; P. 1545, L. 1 and 2, after “sample”; etc., etc.

3. A few grammatical changes should be considered: P. 1542, L. 5: “microphysically”, not “microphysical”; P. 1542, L. 17: “experimentally”; P. 1544, L. 14: Replace “Starting

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at almost" to "With initially"; P. 1548, L. 2: Change "are" to "were" (tense); P. 1549, L. 11: "PCS", rather than "PSC" (typo?); P. 1550, L. 4 and 7: "aspherical" is not hyphenated; P. 1552, L. 25: Change "is" to "are" (the word "data" is plural).

4. P. 1546, L. 11 and 12: Make data here consistent with those in Table 1.

5. P. 1547, L. 17: Replace: "Hz" with "s" (to maintain consistency with "time".)

6. P. 1551, L. 2: Replace "one-to-one" with "data set". (A one-to-one line always indicates good agreement.)

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