

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

Anonymous Referee #1

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General Comments

This is a well written paper which presents new data on the heterogeneous nucleation of ice on various mineral dust aerosols studied at temperatures typically encountered for cirrus cloud formation. Ice nucleation was investigated using a large aerosol chamber equipped with a variety of instrumental detection techniques to determine gas phase water, aerosol distribution, and ice formation. In addition, the elemental composition of the various dust particles has been studied using X-ray fluorescence analysis. The fraction of ice nucleating dust particles and the corresponding ice saturation ratio have been determined. These data are important to improve our current understanding of heterogeneous ice formation in the atmosphere. From these data a parametrization

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for the heterogeneous nucleation of ice on mineral dust is suggested. The experimental approach is sound. This paper is within the scope of ACP and should be published. There are a few minor points which should be addressed. One general comment: The paper addresses ice nucleation via deposition mode nucleation. Is there the possibility to show unambiguously, using experimental data, that ice formed by deposition mode nucleation or can this be inferred indirectly from the experimental conditions and data?

Specific Comments

Introduction: Could you briefly outline the mechanism of ice nucleation by deposition mode and immersion mode with appropriate references?

Page 1541, line 10: A reference should be given which describes the influence of heterogeneous ice nucleation on the microphysical and optical properties of cirrus clouds.

Page 1545, 2nd paragraph: "The elemental composition..." It would be beneficial to compare the obtained elemental composition of the dusts to the ones other studies found. Does the ATD analysis agree with the one given by the manufacturer? Does this Saharan dust sample possess a similar composition as other Saharan samples studied by XRD? This could give insight in the variety or similarity of dust particles. Is it possible to derive the mineral type from the elemental composition?

Page 1549, line 7: "...only a minor fraction..." may be changed to "...detects a smaller amount of the..."

Page 1549, line 11-14: An appropriate reference for the negligible uptake of water by mineral dust is missing.

Page 1549, line 20: A reference for the mass growth equation should be given.

Page 1550, line 19: "A major advantage...". From this sentence the benefit of using the OPC-Welas is not clear to me. Maybe elaborate very briefly what is beneficial using Welas-OPC. Does this change the experimental uncertainties, etc.?

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Page 1554, line 5: Here, I have a question concerning the microphysics within the chamber: The fraction f_i is measured during the increase of S_i or the decrease of T_g . f_i reaches a constant value approx. when $S_i = S_{max}$. Do the initially formed ice particles influence the subsequent formation of ice particles due to mass transfer processes, i.e. by scavenging of water vapor due to differences in water vapor pressures between ice particles and dust aerosols? In other words, do dust particles situated nearby ice particles experience lower S_i ? I guess, mass transfer processes would result in a less steep slope of $f_i(S_i)$ in Fig. 7 and 8? How does this relate/compare to atmospheric conditions?

Page 1554, last paragraph: Experiment IN02 results in about 60 % dust particles nucleated ice whereas in experiment IN03 at similar S_i 80 % are activated, however, lower particle temperatures have been applied. Can you give an explanation for this difference? How does this relate to an IN activation dependency solely on saturation ratio as suggested by the parametrization (however, you state below, that it may also depend on temperature and particle properties)?

Page 1556, first paragraph: It is not clear to me how to interpret the uncertainty of S_i shown in Fig. 7 and 8. Why is the relative change in S_i during the experiment more precise? What is the accuracy and precision of the measurement? I think for the interpretation of the data (see below, exponential fit) this information may be important.

Page 1556, line 5: Does the shift in S_i not also depend on the temperature? At lower temperature the shift might be larger?

Page 1556, equation 3: It would be beneficial to discuss the error and/or the limitations of this suggested parametrization. Indicate the temperature and S range for which this parametrization can be applied. In addition, the presentation of a 95 % confidence interval of the fit in Fig. 7 and 8 could show how good the data are described by this model. The validity of the suggested model could be strengthened by statistical analysis of fit and data. You remark that variable “a” may depend on temperature and

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aerosol, (which may explain the difference of 60 and 80 % activation mentioned above). I guess, with much more ice nucleation data for various mineral dusts at different temperatures the quality of the exponential fit increases, mainly due to a better averaged value of “a” representative of the complexity of mineral dust. I agree with the conclusion of this paper that more experiments like those presented here are necessary to provide a simpler parametrization. You may state in the abstract somehow that the experimental results indicate that heterogeneous ice nucleation on dust aerosols may be represented by an exponential function of solely S_i .

Page 1559, line 14: Results for J_S values. I suggest to report J_S including error due to possible uncertainties, e.g. the uncertainty of S_{ae} as given in table 1.

Page 1560, line 25: The values in table 1 do not show that ATD was slightly smaller than the other dust types when taking the error into account.

Page 1562, line 2: Please add a reference for these typical updraft velocities.

Technical Corrections

Page 1551, line 20: after “210 K” a reference to the respective experiments is missing.

Page 1553, line 3: It would be easier for the reader to understand Fig. 4, if you add the abbreviation of the chilled mirror hygrometer, MBW, in the text.

Page 1555, line 6-9: I suggest to remind the reader here again on the relatively long growth times for ice at low temperatures. This may help to understand more readily your point which you want to make.

Page 1557, line 4: “... and 210 K.” After “210 K”, “respectively” is missing.

Page 1557, line 6: “For the experiment... .” I suggest the following change: “...of 1.7 and 3.0 for experiments IN04.44 and IN03.11 with SD2, respectively.”

Page 1558, line 23: change to “... 10^5 and 10^7 cm⁻²s⁻¹.”

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Page 1559, line 14: units of first J value and of first temperature value could be omitted.

Page 1561, line 23: similar to previous point, give units only for the last value.

Page 1562, line 2: similar to previous point.

Figures:

Figure 1: The symbol α is missing at FISH.

Figure 5 and 6: Panel 2: label should be moved to other place. Panel 3: On print you can see only one solid and one dashed line. The text speaks also of a dotted line. This would contradict the labelling, too - please check.

Figure 7 and 8: Change f_{ice} to f_i for consistency with text.

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

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