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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleation of ammonia gas exposed montmorillonite mineral dust particles" *by* A. Salam et al.

Anonymous Referee #2

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This paper investigates the effect of ammonia exposure on the heterogeneous ice nucleation by montmorillonite particles. The dust samples were exposed to either pure ammonia gas or a volume fraction of 30 ppmv in nitrogen at variable time periods. The ice nucleation induced by particles of diameters as large as 5μm, which were dispersed from the montmorillonite samples by a vibrating membrane technique, was measured with a new continuous flow diffusion chamber (CFDC) instrument at temperatures between -5 and -35°C and two distinct relative humidities of 90 % and 100 % with respect to liquid water. Despite the fact that, as carefully mentioned in the manuscript, atmospheric dust particles are typically exposed to much lower ammonia concentration compared to the laboratory procedures, the work presented in the manuscript by Salam et al. is an important and relevant piece of information of how

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the ice nucleation of mineral particles depends on their surface characteristics. In addition to the different concentrations and exposure times in the atmosphere and in the laboratory approaches, it should be noted here that in the atmosphere the majority of ammonia molecules interact with acids like sulphuric acid and nitric acid. Therefore, the effect of surface coatings with ammonium sulphate or ammonium nitrate or mixtures of those with organic substances should be more important in the atmosphere than pure ammonia exposure.

On the other hand, I am somewhat concerned about the quality of the results at least in their presented form (see my list of comments and questions below). This is mainly related to the poor and incomplete description of the experimental methods and to the fact, that actually only a lower fraction of the nucleated ice particles are detected (those larger than 5μm in diamter), but in the text and also the Figures the results are presented as 'active ice fraction' of the aerosol. Of course a lower limit of ice activity is still a conservative estimate and important result, but some of the conclusions concerning the dependence on temperature and the relative humidity may be affected by the fact that the fraction of undetected ice particles may vary with these parameters. After an extension of the experimental part and revisions outlined below the paper may be accepted for publication in ACP.

Major Questions and comments:

Does Figure 2 show the size distribution measured before or after the impactor?

At the end of section 2.4 you state that 'only montmorillonite particles smaller than 5 μm enter the CFDC'. However, an impactor normally is characterised with an S-shaped cut-off curve of a certain width. What was the cut-off diameter of the impactor. Did you measure the characteristics of the impactor? Did really no particle larger than 5 μm enter the CFDC?

If the OPC measures particles larger than 0.3 μm as mentioned in section 2.6, the instrument should also detect all the dust particles shown in the size distribution of

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Figure 2. I note that the MetOne OPC had only 6 size bins and therefore provide only a poor size resolution, but at least the total number counts of the APS and the OPC should compare to each other. Why was the number counts in the larger (ice) size bins not just related to the counts in the smaller size bins?

What are the bin edge sizes of the OPC? If particles are counted in only 6 size bins, could a slight shift of the bin sizes, e.g. by slight changes of the refractive index due to variable amounts of water attached to the particle surface, have affected the number counts in the smallest size bin interpreted as only ice bins. How many size bins have been used for the ice detection and how have the number counts been distributed in these bins?

Can you estimate the fraction of ice crystals smaller than 5 μm that could not be detected in the experimental setup? What about the aerosol particles with aerodynamic diameters smaller than about 0.5 μm. Have they been disregarded in the calculation of the activated fraction? The fact that the results give a lower limit of the activated fraction should be more clearly stated in the manuscript (e.g. replace 'Active ice nuclei' in Figures 3 and 4 by something like 'Detected fraction of ice nuclei') should more clearly be stated in the manuscript and the potential systematic errors like missing ice crystals or incomplete size distributions (if relevant) should be estimated, if possible.

What do you mean with 'The noise level was determined from particle artifacts measured during the blank and dry experiments'?

The quality of the Figures should be improved. In Figure 1, the lines are rather thick and the font size is small. Thinner lines and larger font would certainly improve the readability of the figure. Is there any reason for the uneven numbers at the x-axis of Figures 2 and 3? Tick marks are missing in Figure 3. To show the dependency of the ice nucleation on exposure time, a respective time scale (linear or log) should be used instead of the equally distant plot format on the x-axis. Otherwise use a bar chart format.

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How did you define the noise level in Figure 2?

The description of the aerosol measurements with the APS and the optical particle counter (OPC) should be extended in the experimental section of the manuscript.

Minor comments and typos:

p.385, l.27: . . . the interaction of . . . was investigated . . .

p.391, l.9: . . . which is absent in Fig. 3a: If you look closely the band at 3220 cm^{-1} also weakly appears in Fig. 3a. Any explanation for that?

p.391, l.13: There is another small IR signature . . . Which one do you mean here?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 383, 2007.