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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of particle size on the ice nucleating ability of mineral dusts" *by* A. Welti et al.

Anonymous Referee #1

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Review of "Influence of particle size on the ice nucleating ability of mineral dusts". Author(s): A Welti et al.

The authors have carried out some very interesting and important measurements. Ice nucleation on mineral dust particles plays an important role in ice cloud formation in the atmosphere and may be important for climate predictions. Also, understanding the dependence of particle size on ice nucleation is important for extrapolating laboratory results to the atmosphere and for understanding the fundamentals of ice nucleation.

Due to the importance of the topic and the questions addressed, the manuscript is well suited for ACP. The paper is also reasonably well written and the experiments appear to be carried out carefully.

The paper, however, should include a proper error analysis and the experimental results should be placed in better context with previous measurements. Other points the authors should address are included below. I recommend this paper for publication, if the authors are able to adequately address these points.

1. Page 6932, line 4-7. Are the authors referring to Table 5 in Archuleta et al. 2005. If so, the data agree within the uncertainty of the measurements. The data do not suggest that the maximum nucleation efficiency occurs at -50C, I don't think.

2. Page 6934, line 5-8. The authors state that results for 100 and 200 nm particles can not be used for quantitative analysis. However, the authors fit the 100 and 200 nm data to a sigmosiodal function for predicting the activation spectrum. Does the statement on Page 6934, line 5-8 imply that the fit for the 100 and 200 nm data should not be used in atmospheric models since the results are not quantitative. Please explain. Also when discussing the 100 and 200 nm data in the Results and Discussion Sections, please discuss that the limitations of the data due to the multiple charged particles.

3. Throughout the manuscript there is no discussion of uncertainties associated with the data. This makes it difficult to compare the results for the different minerals or even to compare different particle sizes. Figure 5 and 10 should have error bars and the uncertainties (i.e. error bars) for the other plots should be discussed.

4. Page 6936, line 16-17. "The corresponding temperature shifts from -50C for 100 nm particles to -35 from 800 nm particles." Looking at the data, it does not appear that this is the case for every mineral. Also, is this true once you take into account the uncertainties in the data?

5. Related to the above comment, it appears that in some cases the RHi required for 1% activation increases as the temperature decreases below approximately -35C. What is the explanation for this observation. Is this due to uncertainty in the measurement, or slow growth rates of ice particles at low temperatures?

6. Page 6937, line 1-2. “at -35C a steep increase in the activation fraction for illite and kaolinite particles has been observed as water saturation was reached”. The steep increase in the plots is not obvious to me. Exactly what panel are the authors referring to, and at what RH does the steep increase occur?

7. Page 6938, line 26-27. “the activation spectra for 200, 400, and 800 nm particles overlap and become uniform.” This is too strong of a statement. For example the -35C and -40C data do not overlap.

8. Page 6939, line 1-2. “the activated fraction is dominated by the surface area of the selected particles.” Here they are implying the data for 200, 400 and 800 nm particles overlap better when the data is normalized to surface area. However, it is hard to tell how much better the data overlap, without comparing directly the same data set plotted both way. For example I would like to see the data in Figure 8 plotted unnormalized. Maybe in some cases the improvement is minor. Some of the unnormalized data is shown in Figure 6, but not all the data. I would like to see all the data plotted both ways, and compared directly. Also, the authors should be more quantitative when comparing data. For example, do the normalized data agree within the uncertainty of the measurements? If not, then surface area cannot explain everything.

9. Only the surface normalized data for illite is shown. Is it possible to include the surface normalized data for the other minerals in an appendix or supplemental data. This is very important data and should be included. Also, I would like to see a direct comparison between the normalized and unnormalized data for these other minerals. Is the overlap significantly better when the data is normalized to the surface area? Again, please be quantative in the comparison. Do the normalized data agree within the uncertainty of the measurements?

10. The authors show very nice new data, but they do not put their data in the context of previous measurements. For example, several groups have measured the onset of ice nucleation as a function of temperature using larger particles. See for example [East-

wood et al., 2008; Kanji and Abbatt, 2006; Zimmermann et al., 2007; Zimmermann et al., 2008]. Are the results in the current manuscript consistent with the previous measurements?

11. Also, do the results in the current paper agree with the studies by [Salam et al., 2006]?

12. Also, are the results for ATD consistent with [Knopf and Koop, 2006], comparing only data that was not pre-activated?

13. Also are the contact angles determined in the current studies consistent with the contact angles determined by [Eastwood et al., 2008]?

14. The size dependent measurements by Hung et al. should also be referenced. [Hung et al., 2003]

15. Page 6941, line 11-12. “therefore no specific dust property can be declared as the most important for ice nucleation.” Is this conclusion consistent with previous measurements (see references mentioned above)?

16. Page 6941, line 18-20. Should the authors state that the fitcurve is not quantitative for 100 and 200 nm particles? Or perhaps I misunderstand what the authors mean by quantitiative.

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