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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 #2

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1. The paper addresses questions that have long been fundamental to ice nucleation in general and to atmospheric processes in specific. The capabilities of a new instrument are being exploited to further knowledge about the substance and size dependence of nucleation activity. Not surprisingly, the results confirm some expectations and indicate some new questions, both factors adding to the value of this contribution.

2. The data obtained in this work has clearest interpretations and permit conclusions to be reached for conditions below water saturation. This fact should perhaps be more clearly presented to the readers both in the title and in the abstract. The measurements at or above water saturation are both more limited and more puzzling. Passing Sw=1 should lead to a discontinuous increase in the fraction of activated particles, if freezing activity starts in addition to deposition. The authors state this well, and point to Fig. 6

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as containing evidence. However, that evidence is sparse: only in two cases there are inflections in the curves, and one is quite continuous. Because this points to a possible blurring of the distinction between deposition and freezing nucleation in these tests, or to a limitation arising from the short transit time of the sample through the instrument, the situation should be discussed more fully. Both for reasons of basic understanding and for practical applications of the results.

3. The Introduction is written at a rather simplified level. More work on this would benefit the paper.

4. 6932/23: The authors indicate the commercial sources of the samples. What were the the methods of preparation, purification or pre-treatment of the samples? What is known about their hygroscopicity?

5. 6934/6: An important point is being made here, yet the remainder of the paper doesn't reflect that. Could, for example, the size normalization be done with the inclusion of the estimated numbers of multiply-charged larger particles?

6. 6935/10...: While the Stetzer et al. 2008 paper has more detail on this, it would be helpful to make some mention here about equilibriation times, transients etc. and their potential impacts on the results. In the same vein, is that the case that even with 24°C differences in plate temperatures there is no convection or other flow instability?

7. 6936/10: Agreement with nucleation theory is stated here a bit too lightly. No quantitative match is shown.

8. 6938/3: It is unclear what the 30% difference refers to. Relative to one another, or in absolute terms of RHi

9. 6938/10: Constant chemical and crystallographic characteristics really don't exist in any strict sense of the words. The assumption that nucleating sites occur with the same probability per surface area independent of overall particle size is an obvious simplification. It is usually made due to lack of knowledge of the nature or sizes of

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sites. In this study, no information is given about how similar are the shapes of particles of different sizes. That uncertainty, the effects of multiply charged particles on the surface adjustments, and possible variations in properties with size due to all kinds of uncontrolled factors associated with the production and dispersion of the samples, add up considerable fuzziness in the degree of agreement that could be expected as a result of surface normalization. Thus, the lack of full agreement should not be given much theoretical interpretation. The mention (6939/7) of a size limit prediction from classical nucleation theory is stretching the point.

10. 6939/23: How were contact angle values determined? Isn't there too much uncertainty in such calculations to make them worth discussing?

11. Overall, the paper presents important experimental results but the level of rigor applied in the presentation should be raised.

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