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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of ice nucleation by mineral dusts in the contact mode" *by* K. W. Bunker et al.

Anonymous Referee #3

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General comment

The Bunker et al study presents measurements of ice nucleation on mineral dusts in contact mode. Given the dearth of measurements aimed specifically at contact freezing, this work represents an important contribution to the field. The experimental setup is a novel one and offers the opportunity to conduct important measurements in contact freezing mode, which are of high interest to the field of ice nucleation.

Major comments Aside from SEM images in a separate figure, the only figure of merit is figure 3. I will focus my comments on that figure.

Experiments were conducted on Arizona Test Dust (ATD) over a range of sizes and at ∼one temperature +/- 1.5 degrees. The three temperatures were nearly the same, -15,

-17 and -18 °C, this data set is not adequate for evaluation of temperature differences cannot. I would recommend combining this data into a single data set at -16.5 +/- error. This brings up a related question- what is the uncertain in experimental temperature? Temperature of the surface under the drop is measured accurately, but what about the air temperature, I think it is not controlled? So how uncertain is the determination of the actual drop?

Changes in freezing events/deposited particle due to particle size differences can be evaluated or can begin to be evaluated, at least, with the current data set. At each temperature the ATD experiments were conducted at more than one size. For ATD at -17 and -18 °C, data is collected at five sizes, from ∼60 nm diameter to 1000 nm diameter.

The trend in freezing efficiency is more or less unchanged across all sizes, with the interesting exception that the smallest size, 50 nm is an order of magnitude higher in freezing events/deposited particle than other sizes. However, given that only 2 experiments were conducted on IN of this size (at -17 and -18 °C), it is unclear how certain we can be of the results. (If we are to assume that the authors wanted us to consider these three temperatures as different data sets, then to be complete, data collected at -15 °C is incomplete since data for the two smallest diameters, and most interesting data points are not included in Figure 3.)

I see no theoretical reason why the smallest particles would be the best nuclei. In fact, a number of studies indicate the opposite, that larger particles are the best nuclei. Do the authors have any explanation for these interesting observations?

Experiments were also conduct on kaolinite but only at the 2 largest sizes. It would be very interesting to see if the usual improvement at 50 nm is also observed for kaolinite particles.

The error bars shown in span several orders of magnitude. Given the stochastic nature of freezing, a degree of variability is to be expected, but the degree reported here seems even larger than reasonable expectations. Is contact freezing really this unpredictable or is experimental design contributing the majority of the uncertainty here? Does this number vary from experiment to experiment or from beginning of experiment to end or is this simply uniform variation throughout?

Assuming some particles contact the surface and do not freeze instanteously, how can be known at a later time whether freezing occurring at a later point is due to a collision at that moment or due to immersion freezing on the nucleus which has reached the particle earlier? This is not an easy question, I realize. But is it an important one, and some additional commentary on the subject is warranted.

How is it known that the flow is turbulent? Has a Reynold's number been calculated? Since this is pivotal to assessing how many particles have the opportunity to contact the droplet surface, please elaborate.

Minor Comments: Pg 29293, line 14. Need to explain how condensation and/or deposition freezing are measured in the atmosphere and why contact is not.

Pg. 20295 line 14. "sizes ranging from 1 micron to 62.5 nm" Need units to be consistent and the smaller size stated first.

Pg. 20297, last paragraph It is not clear from the text how collecting SEM images leads to a determination of projected dust particle area. Please elaborate.

In the same paragraph, it is stated that the SEM diameters are "quite" different than DMA mobility diameter. How different is "quite"?

Summary In short, the authors appear to have a good method for making measurements of a nature important to the field. However, I strongly recommend more measurements be conducted prior to final publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20291, 2012.