

Interactive comment on “A laboratory investigation of the ice nucleation efficiency of three types of mineral and soil dust” by M. Paramonov et al.

Anonymous Referee #1

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This paper provides a thorough investigation of the different IN active components on mineral and soil dust by using various treatments on the dust and then testing the INA pre and post treatment. It is a very nice paper that is well written and adds to the body of literature on soil dusts and their INA.

Major Comments: Page 5 line 14: More information is needed about the soil collection. What was used to collect it? How was it transported and stored? How many cm below the surface was collected? Was it all from one spot or was random sampling done over a gridded area and the samples combined to give a representative sample? Please give information about the specific sieve you used and the procedure. Was it sieved by

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hand or with a machine?

I very much enjoyed the discussion of the surface area uncertainties and how the washing and heating procedures can alter the surface area. However, it makes me wonder if the paper would benefit from also including $n_{s,geo}$ as a means to compare to other studies that use $n_{s,geo}$ and to see if the results hold (meaning Himalayan n_s is highest, Iceland lowest etc.). It might be an interesting exercise, especially since you call out BET as potentially not being a good way to go about calculating n_s . Adding $n_{s,geo}$ to the discussion would complete the thought exercise of which is the best way to present INA data.

Minor Comments: Page 5 lines 7- 12: It would be nice to include the latitude and longitude of these locations.

Page 9 lines 23-24: Instead of “The study” it would be clearer to combine with the previous sentence to read “. . .as presented in Ullrich et al. (2017), which investigated particles in polydisperse. . .”. Or if you think that’s too long of a sentence revise to say “The Ullrich et al study investigated. . .”

Page 9 lines 17-25: It is not necessary, but would aid the reader to have the Kanji et al 2011 and Ullrich et al. 2017 data on a plot with your data (maybe in the supplemental) to visualize the comparison you are describing here.

Page 10 5-7: This sentence is repetitive.

Page 10 lines 22-26: “While it is not possible to directly determine the reasons behind this observed difference, it may be possible that large particles contain more soluble material blocking the active sites and/or that small particles may contain more IN-active material on their surface, e.g. bacteria or active minerals. Particles of approximately 200 nm in size, including mineral dust species, have previously been reported as constituting the majority of the INP found in the ice crystal residual size distributions (Mertes et al., 2007).” Is the activated fraction higher for the 200 nm particles as well?

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Page 14 line 8: Fig. 4 change to Fig. 4

Page 14 lines 9-10: "Figure 3 shows that in deposition nucleation mode the decrease in INA after heating is minimal and becomes more pronounced in condensation freezing mode" change to "Figure 3 shows that in the deposition nucleation mode the decrease in INA after heating is minimal and becomes more pronounced in the condensation freezing mode".

Page 14: Throughout this discussion "the" should be added before the different ice nucleation modes. Example "the condensation mode".

Page 16: "The conductivity of supernatant water and, hence, the deduced amount of soluble material was highest for Iceland dust" Would different molecules or types (ie organic acids versus salts) affect the conductivity differently? Can you say 1:1 that the magnitude of the increase relates to the amount of soluble material? Or would salts increase the conductivity more than organic acids? I'm asking just out of pure ignorance to this type of measurement. It may help other readers who are not familiar if you add a sentence about this whether it would or wouldn't change the conductivity.

Page 17: I really enjoyed the detailed discussion of the H₂O₂ procedure and how each step may impact the results and thus the implications. It was well thought out and clearly explained so the reader could follow along with the logic. Nicely done.

Figure 8: I know it is written out in the figure caption, but it would be easier for the reader if the SEM images were labeled right on the image with Iceland, China, and Himalaya. Maybe on the upper right hand corner of each image. It would just be easier to compare them without having to remember the order.

Page 20: "It was shown that at temperatures of 238–243 K, the ice nucleation activity of the untreated, surface collected soil dust in condensation freezing mode can be roughly approximated by one of the existing surrogates for the atmospheric mineral dust, such as illite NX, for example."

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If this is true, then why does one even bother with the post-treatments? Also, I would not expect a 1:1 correlation for the amount of material removed and the decrease or increase in INA because it is not linear. It is based on active sites but those may not be evenly distributed on the particles and coatings covering active sites can be covering the whole particles or a blob on one side and so the mass of something removed will not always cover the same surface area. This is especially true when you are looking at bulk removal across all sizes and then trying to correlate that to size selected particles. I would not expect it to be linear or to have a simple relationship. This ties back to what you were saying about bulk measurements complicating matters and dust being known to have varying chemical composition with size. The discussion might benefit from a little bit more explanation about these complexities.

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