

# ***Interactive comment on “Heterogeneous Ice Nucleation Properties of Natural Desert Dust Particles Coated with a Surrogate of Secondary Organic Aerosol” by Zamin A. Kanji et al.***

## **Anonymous Referee #1**

Received and published: 17 October 2018

### General comments:

This article reports that coating two types of mineral dust particles with a secondary organic aerosol proxy produced by dark ozonolysis of alpha-pinene made little difference to their immersion mode ice nucleating ability between 253 K and 233 K. No systematic differences in ice nucleating abilities of the dusts were observed with various atmospherically plausible coating thicknesses. Measurements were conducted simultaneously on three well established cloud chamber instruments with broadly consistent results. The study is relevant to the scope of ACP, presents useful and interesting new data and is scientifically sound. The paper is mostly well written. I have a few minor

[Printer-friendly version](#)

[Discussion paper](#)



comments, but am quite happy for the paper to be published once these are addressed.

Minor comments:

The conclusion ‘SOA coatings did not affect the immersion ice nucleation ability of the dust particles in the temperature range 253 to 235 K irrespective of coating thickness (3 – 60 nm)’ and similar statements in the abstract seem too strong. While most of the data does support the statement the very reasonable uncertainties in measured INAS suggest to me that there could be some effect that it is not possible to discern with certainty from the data. The data are certainly suggestive and surprising, I would have thought that SOA coating would make a measurable difference to the INAS of the dusts, but I do not think this study allows the conclusion that SOA will not ‘impede or enhance the ice nucleation ability by immersion mode of mineral dust in the mixed-phase cloud regime’. The authors effectively acknowledge as much in their discussion but this subtlety is at least partially lost in the conclusions and abstract. Additionally, it is entirely conceivable that different results could be obtained if different dust samples were used. I do not think that two soil samples can be claimed to represent all desert dusts. To summarize, I think it should be made clearer that the study covers only a fairly narrow set of circumstances and that this topic likely needs further investigation. I do not think this detracts at all from the usefulness and interest of the study.

On a related note, it is increasingly clear that the ice nucleating ability of a mixed mineral dust depends on its composition, at least potentially (Harrison et al., 2016; Peckhaus et al., 2016). While the information is in the literature as stated I think there should be a table reporting mineralogy of the two samples.

Similarly, it is stated in the conclusions that fit to data in this work ‘yield a parameterization for desert dusts’, which seems a very general statement for measurements conducted on two samples. Also, it does not seem to me surprising that the INAS spectrum fits reasonably to that of Niemand et al. (2012) when the measured samples are two of the five or so used in Niemand et al. I would suggest removing both these

[Printer-friendly version](#)[Discussion paper](#)

statements.

I am a little curious as to why this study was conducted on soil samples dug up from underground or collected from the surface. I would have thought either more directly atmospherically relevant samples or 'pure' mineral samples would be of greater interest. Possibly the authors think these samples are of substantial atmospheric relevance but I think this needs to be more thoroughly explained and justified if so. Finally, I realise it's the established name for this sample but is it really reasonable to call dust collected north of Cairo 'Saharan'?

Recognising it might be slightly sparse, I think the authors may want to consider a figure showing the absence of impact of thickness of coating on ice nucleation effectiveness. Currently, the reader is forced to refer back to table 1 to figure it out, which doesn't aid readability.

Specific comments:

Pg 15 Line 29- considered to be 'in' reasonable. What does reasonable mean? Some sort of quantitative description might be helpful, and perhaps a comment on how data produced by the different instrument types should be interpreted.

Pg 3 line 15- Ammonium sulphate has been observed to enhance ice nucleation of mineral dusts recently (Kumar et al., 2018;Whale et al., 2018). It may be appropriate to note this here.

Pg 4 line 23- Why is immersion freezing being mimicked? Is the process not immersion freezing?

P 7 line 31- Are convective clouds not natural?

Pg 10 Line 20- Why does the number of large particles change reported AF? A bit more discussion may help the reader.

Technical comments:

Printer-friendly version

Discussion paper



Section 4.4 follows section 3.3 currently, this should presumably be section 3.4.

Pg 1 Line 15- The first sentence of the abstract make it seem as if there were two sets of experiments conducted, which was not the case, I would suggest revising this.

Pg 2 line 28- 'inferred' should be 'by inference' or similar I think

Pg 3 line 20- I would use 'INPs' instead of INP. I would suggest checking that INP and INPS are used properly throughout.

Pg 7 line 33- pg 8 line 1- '...homogeneous temperature control of below  $\pm 0.3$  K.' is clumsy.

Pg 8 line 29-32- contribution to aerosol number maybe?

Pg 10 line 16- 'Way above uncertainty' is a bit loose.

Pg 11 line 17- missing word after 'indicated'.

Pg 12 line 8- Favouring immersion freezing over what?

Pg 13 line 7- maybe mention the origin of this factor of 3.

Pg 15 line 20-21 'appreciable' is very vague.

Pg 16 line 6-8- This sentence is poorly written.

Reference list- Ullrich et al. 2017 and several others lack journal names and Megahead should be Megahed I believe. I suggest checking the list carefully.

#### References

Harrison, A. D., Whale, T. F., Carpenter, M. A., Holden, M. A., Neve, L., O'Sullivan, D., Vergara Temprado, J., and Murray, B. J.: Not all feldspars are equal: a survey of ice nucleating properties across the feldspar group of minerals, *Atmos. Chem. Phys.*, 16, 10927-10940, 10.5194/acp-16-10927-2016, 2016. Kumar, A., Marcolli, C., Luo, B., and Peter, T.: Ice nucleation activity of silicates and aluminosilicates in pure water

[Printer-friendly version](#)

[Discussion paper](#)



and aqueous solutions – Part 1: The K-feldspar microcline, *Atmos. Chem. Phys.*, 18, 7057-7079, 10.5194/acp-18-7057-2018, 2018. Peckhaus, A., Kiselev, A., Hiron, T., Ebert, M., and Leisner, T.: A comparative study of K-rich and Na/Ca-rich feldspar ice-nucleating particles in a nanoliter droplet freezing assay, *Atmos. Chem. Phys.*, 16, 11477-11496, 10.5194/acp-16-11477-2016, 2016. Whale, T. F., Holden, M. A., Wilson, Theodore W., O'Sullivan, D., and Murray, B. J.: The enhancement and suppression of immersion mode heterogeneous ice-nucleation by solutes, *Chemical Science*, 9, 4142-4151, 10.1039/C7SC05421A, 2018.

---

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2018-905>, 2018.

Printer-friendly version

Discussion paper

