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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Concentrations, composition, and sources of ice-nucleating particles in the Canadian High Arctic during spring 2016" *by* Meng Si et al.

Anonymous Referee #2

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Review for "Concentrations, composition, and sources of ice-nucleating particles in the Canadian High Arctic during spring 2016" by Meng Si and co-authors, submitted to ACPD:

The manuscript reviewed herein is a well written, up-to-date and informative piece of work. Impactor samples were taken in the Arctic and analyzed wrt. atmospheric concentrations of ice nucleating particles. The results were brought into context with information on the chemical composition and on air mass origin. INP that were ice active at -25°C were shown to correlate with mineral dust particles, with the Gobi desert being a possible source. For INP ice active at higher temperatures, no correlation was

found, although sea spray and anthropogenic tracers were tested.

There is a number of (mostly smaller) issues listed below. Other than that, the manuscript well merits publication in ACP once these issues have been tackled.

Abstract: p1, line 17: Add "at that temperature" behind "INP population", as this is likely not the case at higher temperatures.

Also, mention that you examine immersion freezing – best do that already in the abstract, and/or maybe again later.

And either here or maybe in the methods section, mention how many separate samples you analyzed for INP concentrations. I did not know what to expect until I saw Fig. 2.

Introduction: p2, line 31 ff: It should become clear that different ice active substances are ice active at different temperatures. Sea spray, anthropogenic aerosol and mineral dust all are a bit intermingled, here. Please make this clearer.

Methods: p3, line 17: It is not clear to me why there are three circular glass slides on the second stage? It sounds as if they are there simultaneously? Or did you only use three during the whole time? Please clarify!

p4, line 20: It is crucial what happens to the RH during cooling, in terms of the effect of RH: did the droplets grow or shrink during this phase, if yes, to which extent? (They might get in touch with each other, or they might dry out, as the two extreme ends of what could happen.) Is that prevented somehow? How? A bit of text discussing this should be added here.

p4, line 28 ff: Do really several separate droplets form on one spot? How large is such a spot? You may want to give this number here.

p5, line 5 ff: Freezing of "non-spot droplets" may come from impurities on or in the glass, particularly as particles as small as 180 nm may not be very ice active. And if this is the case, your way of analyzing the data leads to an overestimation. Did you

try the experiment on a “field blank” glass (glass slides treated as the ones used for sampling, including to put them into the sampler, just without turning on the air flow)? Are those “blanks” you mention on p5, line 20 such “field blanks”? How do the data from your “blanks” compare to the number of “non-spot droplets”? And should not the counts obtained from the “blanks” be subtracted from the counts of the samples, instead of adding “non-spot droplets” to the counts of the samples?

p5, line 14 ff: The number 300 confused me. Is that the number of impactor spots that were sampled on that stage? If yes, please clarify! Also, above you discuss a number of sources for uncertainty (several droplet on one spot, merging droplets, . . .), and here now you say that the uncertainties were derived based on Koop et al. (1997). So were these issues (several droplet on one spot, merging droplets, . . .) not considered for the uncertainty?

p5, line 19: More information on the background measurements is needed. Right now, you basically only show this background data in Fig. 2, but it is not said if it had been subtracted, and depending on if you did or did not subtract it, why it was done how it was done. This should be added here.

p6, line 13 ff: I now know that it is discussed later that the daily filter samples described here have a different time resolution than the samples taken for INP analysis (p-8, line 31). This confused me when reading it for the first time, and it would be good to mention already here, that this is the case and that it is discussed later. A the later discussion of this issue, it would be good to mention if air mass trajectories were at least similar throughout the whole days when you sampled. For days when that was not the case, less correlation between INP and any information on chemical composition should be expected.

Results and discussion: p9, line 27: “The North Pacific Ocean is not likely the source of mineral dust either.” The high signal in Panel D of Fig. 9 confused me in the beginning. But after looking at the plots a bit, I now assume that it originates in the fact that this

Pacific region gets its high signals as it has small signals in both, plot B and C. So it would be the result of bad statistics (dividing a small signal by yet smaller signals).

But still, would not all regions in B have to have higher concentrations as those in C? C, as I understand it, is a subset of B, and yet, there are regions in C that indicate larger concentrations, which cause the high values in the Pacific in D. - How can this be? Please explain the latter, and also mention that high values in the Pacific in “D” come from the division of small signals by small signals, if that is the case.

p10, line 12-16: I agree that you show that mineral dust contributes INP that are ice active at -25°C. You also show that INP that are ice active at higher temperatures have other sources (as they do not correlate with mineral dust). So the statement you make here is a bit twisted, as you do not show what contributed the INP that are ice active at the higher temperatures, which is the temperature range for which INP from sea spray (generally assumed to be biogenic in origin) may be ice active. And for mixed phase clouds INP active at higher temperatures might be even more important than INP active at -25°C. Please correct your statement here accordingly, particularly the last sentence.

p10, line 21 ff: It is VERY well known that salt in general causes a freezing point depression (that’s why salt is put on icy roads in winter time). That is why I was somewhat surprised by that part in general, as I would not consider this a new result. There is also other older work on that, not for NaCl but for salts and other soluble material in general, on the temperature depression of ice nucleation for minerals (Koop & Zobrist, 2009, Wex et al., 2014), in which even more general ways of describing this suppression are given. Although this originates from laboratory studies, it may merit including that and mentioning that your results is not such an astonishing finding. Also, as you know the salt concentrations on the filters, it would be interesting to see how much freezing point depression could be expected from that. This is easy to do, and, albeit only being an estimate, it could show if an effect may be expected at all.

p11, line 17: Do you mean “at any of the three temperatures”?

p11, line 19ff: As you discuss other non-Arctic studies here, you may want to include a recent study on INP in Beijing, where it is also said that: "All these results indicate that Beijing air pollution did not increase or decrease INP concentrations in the examined temperature range (down to -25°C)." (Chen et al., 2018).

Figures Caption of Fig. 9: What do you mean by “for all mineral dust sampling periods”? The time when samples were analyzed wrt. INP concentrations? Please clarify, here or in the respective text.

Supplement There is a supplement with nothing else than one table that is not even large. This should be included in the main manuscript. It makes no sense to have a separate document for so little of information.

Literature Chen, J., Z. Wu, S. Augustin-Bauditz, S. Grawe, M. Hartmann, X. Pei, Z. Liu, D. Ji, and H. Wex (2018), Ice nucleating particle concentrations unaffected by urban air pollution in Beijing, China, Atmos. Chem. Phys., 18, 3523–3539, doi:10.5194/acp-18-3523-2018.

Koop, T., and B. Zobrist (2009), Parameterizations for ice nucleation in biological and atmospheric systems, Phys. Chem. Chem. Phys., 11(46), 10839-10850, doi:10.1039/b914289d.

Wex, H., P. J. DeMott, Y. Tobo, S. Hartmann, M. Rösch, T. Clauss, L. Tomsche, D. Niedermeier, and F. Stratmann (2014), Kaolinite particles as ice nuclei: learning from the use of different kaolinite samples and different coatings, Atmos. Chem. Phys., 14, 5529-5546, doi:10.5194/acp-14-5529-2014.

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