Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-678-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size-Resolved Atmospheric Ice Nucleating Particles during East Asian Dust Events" *by* Jingchuan Chen et al.

Anonymous Referee #2

Received and published: 10 November 2020

In this manuscript, a study is introduced for which size segregated airborne aerosol samples, sampled during desert dust events in China, were examined concerning the particles ability to act as ice nucleating particle (INP). Besides for the effect of size, also possible contributions from biogenic materials were examined.

The study was well done and is well presented. The results are very timely. I have a few concerns, but nothing really major. Major revisions are only recommended (instead of minor revisions) due to the number of comments. However, after addressing my below comments, the manuscript will very likely be suited for publication in ACP.

major remarks:

line 23-24: What is the advantage of a size dependent parameterization over a single

n_s curve that could be used with the total surface area of an aerosol? This could be discussed later in the text. Also, you could estimate which error would be done if one used such a single n_s curve, compared to your higher size resolved information, for which, however, more modeling capacity will be needed.

section 2.1: You do discuss losses in a MOUDI a bit. But I still wonder if the filters put on the MOUDI-stages did not change the collected sizes, as this is sensitive to the distance between the plates. Also, the filters are much larger than the collection area of a MOUDI - were the filters cut or how was this issue dealt with? And how were they kept in place?

line 149: It is clear that either diameters as measured by the SMPS needed to be adapted, or the aerodynamical diameters as measured by APS and as selected by MOUDI. But without checking literature thoroughly, I would have expected that geometrical diameter is the one most previous studies refered to, not aerodynamic diameter, when retrieving n_s. Please check this and give an estimation of the deviation this may cause.

line 170 ff: The description of the definition of the dust events needs to be adjusted: from Table 1, it seems that it was based on CMA observations, only. Otherwise M5, D6 and D7 would also not be dust events.

lines 175ff: Were results from M4 included in this study at all? If yes, how? If not, why mention it? And why are M1, M2, M3 and M4 described here explicitly, but not all the other samples? A sentence or two on the samples, even if they are only summarized in groups, would be helpful, so that for each sample its specifics are clearer (like the one found in line 214 - you can repeat some of that information there again, but also add it here).

line 178: The data shown in Fig. 1 in "pale yellow" - are they all for size segregated filters? If so, why is the set of colorfully depicted data well above these curves for the three larges particle sizes? If this colorfully depicted data-set is one with particularly

high ice activity, mention this explicitly.

Line 183-185: Based on f_ice curves, it cannot be judged which size class is more ice active. It is theoretically possible that there are just so many more (or less) particles in one size class than in others, so that this number is the overwhelming influence on the overall measured ice activity at that impactor stage, causing high (or low) f_ice. I suggest that you at least mention this in an additional sentence.

line 194-195: "For mineral dust particles, their freezing temperatures were similar." Do you mean that the dust curves were all similar? That is not true, and you should normalize to dust surface area first before you make comments on that, anyway. You even mention that in the next sentences. This sentence here needs to be deleted or revised.

line 203: Fig. 2 (b): Is this based on one sample, or an average from all? Mention this! And again, it might make more sense to show this for size-normalized data, but number is fine, here, too.

line 214: Why did you choose to only show results for this subset of samples? This needs to be justified in the text, otherwise the impression could arise that you did "cherry picking", i.e., only displaying those samples that fit what you want to see.

line 215: Is Sample D7 included in Fig. 3 (b)? To me, only 6 curves are visible in Fig. 3 (b), too. Please check, and if D7 is not included, mention this in the text.

line 217: The airborne INP concentration depends on getting dust suspended in the air. So even the same source region can yield different airborne concentrations for different conditions such as changing wind speed. Mention this additional restriction. Different shapes, however, really depict different types of INP. (Like, based on what you describe later, a biogenic component in the "red" samples.)

line 233: Concerning Fig. 4 (a) and (b), it seems as if here only 10 different data-sets are shown. Is that correct, and if yes, why were the other data not shown? This is in

line with my comment concerning line 214.

line 235: It would be more informative to mention the temperature span at a single temperature, as this overall span depends on the measurement method you use! (It varies with the amount of air you collect, the surface area (hence the size distribution) and, to a lesser extent, to the number of droplets you examine, but NOT on any characterization of the INP.)

line 286: Again, why is only a subset of all 14 sets of filter samples shown?

line 292-293: "This may suggest that after heat-sensitive INPs was removed, the two transport pathways are now dominated by similar material, which is probably mineral dust." I totally agree - but that makes any discussion of different feldspar contents, which you did above, futile. Please check the content of the text for consistency! Or, when you mention feldspar, already point out that this may not be important as the importance of the biogenic content will be discussed below.

line 299-300: What does that sentence refer to (in Sect. 3.3)? Please explain what you mean.

line 223ff: My advice is to not overinterpret such observations. There are measurement uncertainties on all of these curves, and there are different approaches. The n_s derived in a lab-study from mineral dust samples refers to the surface area of dust particles, only, while in your study, you naturally have to refer to the surface area of the total aerosol. Also, you used the aerodynamic diameter as the reference, while this, to my understanding, was not the case in the other studies. So please be careful when discussing such details.

line 333-335: How does that fit with the fact that you see such a high biogenic / proteinaceous fraction being responsible for the ice activity at higher temperatures??? Also: The deviation can be seen at high temperatures, where your fits are much above the mineral-dust parameterizations - mention that explicitly. Also: Fitting a straight line

over such a broad T-range might be misleading.

line 340-341: As said above, this temperature range rather characterizes your method than saying anything about the INP. Give the span at a single temperature, as this signifies who different your different samples were.

line 346: “the common effect of the activity” - what do you mean by that. This could be elaborated somewhat more, maybe even in an additional sentence.

line 350: Is that really what you find. You argued with different feldspar content at some point, then with different biogenic content, and now you summarize all this in saying “all desert dusts are the same”. Make the message of your text consistent throughout the manuscript.

line 362ff: “Larger particles are more active INPs, as particle size reflects the mineral composition to a large extent” - This is not necessarily true. If larger particles have a higher n_s, then it’s true, but in general larger particles are more ice active because they have a larger surface area. Formulate this with more care.

Technical issues and minor remarks:

line 29: “relatively high temperatures” - say more precisely what you mean by that.

line 37: What exactly do you mean by “mid-level clouds”. The use of “mixed-phase clouds” (as in the next sentence) seems more appropriate here.

line 52: “supplement for feldspar” - it is not clear what you mean, here. That the two always occur together? That would not be correct, as feldspars are weathered clay minerals, and quartz is not a clay mineral. Please check and reword.

line 91: “stages . . . were detected” - wrong wording, needs to be changed.

line 108: Are you aware of the fact that the use of ultrasonic waves may change the structure of proteins and therewith change their functionality? (see. e.g., DeLeo et al., 2016) At least mention this in your text, so that future readers know about this issue

when they consider repeating what you did.

line 115: change “will not be expected and” to “is not”.

line 120: This is the first time that active sites are mentioned, so you may want to add a few words on explaining what you mean by that.

line 138: “Gross” seems a bit misplaced here. I suggest to use a different word. Or, as you use “gross” more often, what you mean by that.

line 287: Change “originated” -> “originating”.

line 289: “For example, ðİŚĄ_ðİŘijðİŚĄęðİŚČ near temperature at -10 âĎČ.” This is not a complete sentence - check and correct.

Fig. 2: “b” is missing in the legend for Chen et al. (2018b). Also, change the color either for the Bi et al. (2019) datapoint, and/or make it an open symbol (maybe an open star?), as it is difficult to discriminate between these data and those from D2.

Literature: De Leo et al., Effect of ultrasound on the function and structure of a membrane protein: The case study of photosynthetic Reaction Center from Rhodobacter sphaeroides, Ultrason. Sonochem. (2016), http://dx.doi.org/10.1016/j.ultsonch.2016.09.007 .

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-678, 2020.