

Interactive comment on “Ice nucleating particle concentrations unaffected by urban air pollution in Beijing, China” by Jie Chen et al.

Anonymous Referee #1

Received and published: 10 November 2017

Review of “Ice nucleating particle concentrations unaffected by urban air pollution in Beijing, China” by Chen et al.

General Comment: This manuscript reports the ice nucleating abilities of urban aerosol particles from Beijing (China) using two different ice nuclei counters (i.e., LINA and INDA) during “clean” and heavy haze episodes. The authors did not find a major influence of the air pollution in Beijing on the ice nucleating particle (NP) concentrations as the INP levels did not correlate neither with PM_{2.5} nor with black carbon (BC) concentrations. Additionally, the predicted INP concentrations from the concentration of aerosol particles larger than 500 nm using DeMott et al. (2010) and DeMott et al. (2015) parametrizations did not correlate with the measured INP concentrations. The authors suggest that the INP concentrations in Beijing may have biogenic sources or

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non-urban dust and that is why high levels of pollution did not increase the INP concentrations. This is one of the very few studies that measures the INP concentrations in an urban location, and it could be useful to help the ice nucleation community to understand the role that urban aerosol could play in ice cloud formation. However, the paper requires major corrections before it can be accepted for its publication in ACP.

Major comments: 1. Given that the “insignificant” influence of air pollution in the INP concentrations is not clearly supported I suggest to soften the tone of the Title. How about: “Ice nucleating particle concentrations under urban air pollution in Beijing, China”

2. The reviewer is surprised the authors completely ignored meteorology in this study. A detailed analysis of the meteorological variables and air masses is required to explain ambient observations, even in urban areas.

3. I am not sure if the comparison of the BC and INP concentration is completely fair given that the INP concentrations were obtained from particles collected on 8-h filters, while the BC data was obtained in-situ. Is it possible that the BC particles collected of the filters may have change their ice nucleating abilities during the 8-h period (i.e., aging, coagulation, oxidation, and coating)?

4. It is mentions that secondary particle formation did not contribute to the INP concentrations. However, it is complete unclear how secondary particles were measured or identified in this study.

5. The ice nucleation activation scans from the INDA and LINA are directly compared. However, given that the operational principle from both instruments is different, I am wondering if this is a fair comparison. Did the PTFE and quartz filters collect the same particles mass? Would it be necessary to normalize the INPs concentrations?

6. There are not uncertainties reported in this study at all. The correlations performed in this study do not present any statistical analysis.

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7. I am not sure why the results from this study were compared with the Petters and Wright (2015) precipitation data. I would rather compare the present data with the results from Knopf et al. (2010) and Corbin et al. (2012) that were obtained in urban ambient air instead of precipitation samples.

8. The authors claim that the measured INPs are non-urban and they suggests that the sources of the INPs could be dust or bioparticles which are non-urban. Do the authors think that is it not possible to have urban dust and urban bioparticles?

9. The conclusions are not well supported by the shown data. They are mainly qualitative, in comes cases speculative, given the lack of meteorological analysis, and the non-detection of secondary organic particles, dust, and bioparticles.

Minor Comments: 1. It is unclear how BC was calculated/determined for the PM_{2.5} reported in Figure 1.

2. Why the measured INP concentration time series is not included in Figure 2?

3. There are several sentences and paragraphs that require a citation (e.g., Lines 34, 44, 250, 252, 264, 275, 280, and 352).

4. The introduction is quite disorganized. It jumps between bioparticles, dust, bioparticles, ash, soot, urban, soot and ash. I suggest to re-organize it and to focus on urban particles only. When introducing literature studies, make sure the ice nucleation modes are clearly stated.

5. Lines 85-87: Knopf et al. (2010) also performed immersion freezing experiments relevant to mixed-phase clouds.

6. Was the PM_{2.5} time series obtained from the particles collected on the PTFE or quartz filters?

7. Were the freezing experiments performed with the LINA recorded with pictures taken every 6 seconds? My experience is that droplets freeze very quick and if pictures are

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taken every 6 seconds very important information can be missed.

8. It was said that particle removal by washing the filters was frequently incomplete. Can the authors indicate by how much? What percentage of the particles was not possible to be removed from the filters? This calculation is very important for the direct comparison of the LINA and INDA data.

9. Be consistent with the references format (lines 495, 505, 517, 553, 574, 604, 626, 629, 635, 642, 666, and 669).

10. Table 1. How is it possible to perform deposition ice nucleation at water saturation? Given that S_i is higher than S_w , how is it possible to obtain conditions with $S_i = S_w$?

11. Figure 2. Are the INP concentrations in std L-1? Add here the measured INPs with their corresponding uncertainty.

12. Figure 3. Add all four panels to one single figure. I mean, one figure with 4 panels in one page.

13. Figure 4. Axis and symbols are too small. Add r^2 and p-values.

14. Figure 5. I don't see the purpose of this figure given that Petters and Wrifgt (2015) study focused on precipitation samples.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-884>, 2017.

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