Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-922-RC1, 2022

Comment on acp-2021-922

Anonymous Referee #1

Referee comment on "Ice-nucleating particles from multiple aerosol sources in the urban environment of Beijing under mixed-phase cloud conditions" by Cuiqi Zhang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-922-RC1, 2022

Review

Ice nucleating particles from multiple aerosol sources in the urban environment under mixed-phase cloud conditions

By Cuiqui Zhang et al.

Summary:

The study presented by C. Zhang and co-authors investigates the variability of immersion-mode INP concentrations at -30 °C in an urban environment. INPs were quantified with a high-time resolution which allows to classify daily INP variability, and to investigate the impact of multiple aerosol sources such as fireworks during festivities, local traffic, secondary aerosol particle formation and mineral dust. While only during dust impacted times the ice nucleation ability of the aerosol population was found to be higher as the background, no dependence on aerosol particle concentration larger than 500 nm is observed. Still, the majority of INP observations are predictable within a factor of 2.5 using the parameterization proposed by DeMott et al. (2010) during the dust event. In general, no effect from anthropogenic activities on INP number concentration is observed. The study will be of interest for the ice nucleation community, as it strengthens the findings that anthropogenic aerosol particles are not contributing to the ice formation in mixed-phase clouds. The manuscript is very well written and I only have minor comments.

Minor comments:

I recommend to highlight better the novelty of this study over existing ones. E.g., Chen used filters to quantify INP concentrations at temperatures above -25 °C; Chen et al. (2019; 2022) and Bi et al. (2019) did not compare their measurements to an AMS, which allows to distinguish different anthropogenic aerosol emission sources.

The correlation coefficient analysis of the INP concentration measurements using HINC and aerosol particle size distribution does not consider the cut-off size of HINC. Therefore I believe that the results can be biased by the non-sampling of larger particles in HINC. I recommend to first define the cut-off size of HINC, and only compare the size distributions measurements in this size range with the INP concentration measurements.

Introduction: As you give an overview of the impact of anthropogenic aerosol particles on ice nucleation for heterogeneous freezing in the mixed-phase and cirrus cloud regime it might be useful for the reader to explain a bit more about these freezing modes and cloud regimes.

Specific comments:

Lines 45 – 47: This statements requires a reference.

Line 47: What do you precisely mean with "regional transport"? Regional transport of e.g., dust particles?

Lines 49 – 50: These are examples for this statement, more literature exist about the impact of aging on aerosol properties. Please include "e.g." to the citations or complete the list of citations.

Line 52: "… and catalyze ice crystal formation below 0 °C…" this is almost unneccessary information; it would be more interesting to give information about the onset temperatuer of freezing for each particle type.

Lines 53 – 54: I find this statement problematic as you mix INP number concentration from two different cloud regimes, mixed-phase and cirrus clouds. Moreover, studies exist where INPs are measured at warmer temperatures (e.g., -8 °C; Conen et al., 2015).

Lines 81 – 82: "In other words" sounds not correct in this context.

Lines 107 – 108: Was this upper size limit for the inlet quantified in other studies, or did you measure this size-cut? Please give more information how this cut-off size was derived.

Line 116: I believe that this is the upper limit to operate this kind of SMPS; towards the larger sizes, you are likely impacted by the effect of double charged particles; please give an error estimate here.

Line 119: Did you change the value of effective density when you were impacted by pollution? And if not, what was the reason to choose a constant value?

Line 130: What are the errors associated with the lamina temperature and the relative humidity with respect to water in HINC?

Line 145: What was the sampling averaging time in HINC, e.g., 20 minutes as in Lacher et al. (2017)? And how did you treat INP concentration measurements below the detection limit?

Figures 2 and 3: It might be useful to also plot the INP concentration timeseries in these plots.

Table 1 and related discussion in the text: It might be interesting to the reader to also receive these information about local traffic and firework emission (e.g., as a sub-category of 'pollution').

Lines 215 – 217: Did you also calculate the ice-active surface site density? This might be the better parameter to compare to other studies, as it is a normalized quantity. Moreover, I suggest to compare INP number concentrations only to measurements conducted at the same nucleation temperature.

Lines 240 – 242: Does the non-relation between particle size and INP concentration during the dust event suggest that the mineralogy of the dust aerosol might be the driving factor for ice nucleation? Do you have any information about the mineralogy of the dust particles?

Line 335: The INP concentration measurements were not performed continuously; e.g., there are several days especially before the 22nd February when no INP concentration measurements were performed.

References

Conen, F., Rodríguez, S., Hüglin, C., Henne, S., Herrmann, E., Bukowiecki, N., and Alewell, C.: Atmospheric ice nuclei at the high-altitude observatory Jungfraujoch, Switzerland, Tellus B, 67, 10.3402/tellusb.v67.25014, 2015.