

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

General Comment:

The original manuscript has been significantly improved. Most of my previous concerns were nicely addressed in the revised version. The reviewer has three additional comments that would like to clarify before the manuscript is accepted for its publications in ACP.

Additional comments:

1. 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.

Authors: We added two plots showing trajectories and also wind direction and wind speed, to show the meteorological condition during the sampling period, together with the following text (line 234-245):

Additionally, Fig.2 shows 2-day back-trajectories obtained by the NOAA HYSPLIT model, with one trajectory related to each sampled filter, starting at the median sampling time of each filter. Fig. 3 shows minutely recorded data for wind-direction and wind-speed collected by (Met One 591) and (Met One 590) located on the same roof top as the aerosol sampling equipment. Both pictures are colored-coded with respect to PM_{2.5} mass concentrations. The air masses that came from north or north-western directions were generally coincident with higher wind-speeds. They brought clean air with lower PM_{2.5} mass concentrations. They did cross desert regions, however, Beijing was reported to be affected by desert dust in mainly only spring (Wu et al., 2009). Typically, the air masses coming from south and south-west of Beijing moved slowly and spent much more time over industrialized regions, resulting in high particulate matter mass concentrations. This here observed pattern is typical for Beijing, and these connections between wind-direction and pollution levels in Beijing have been analyzed in detail previously in Wehner et al. (2008).”

Reviewer: The reviewer appreciate the addition of the new plots and new text. This is very useful information for the readers. However, what is still missing is the correlation of the

meteorological variables with the INP concentrations. I assume that one of the main goals of this study is to identify the source of the INPs. Meteorology could help the authors to understand this. Is the INPs concentration lower or higher when the air masses were from the north and north-west with low PM_{2.5}? Is the INPs concentration lower or higher when the air masses were from the south and south-west with high PM_{2.5}? How about wind speed? Does it have any influence in the INP concentration?

2. 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?

Authors: Indeed, there may be urban dust and also urban bioparticles. But these are no major contributor to the increase in PM_{2.5} mass concentrations during winter times – rather, it is well known that this increase is related to anthropogenic pollution. On the other hand, considering biogenic and dust particles, these particles emitted from urban areas will only contribute little to the overall atmospheric dust and biogenic particle load, as the non-urban sources are much more dominant for these types of particles. Therefore explicitly mentioning that dust and biogenic particles might also be emitted from urban sources does not really make sense. If there is, however, a specific passage in the text that you feel is miss-formulated, please tell us where this is exactly and why precisely you think this is wrong. For the time being, nothing was changed.

Reviewer: Here I disagree with the response. The authors agree that there are urban dust and bioparticles; however, they provide two arguments to say these particles are not important. It is said that i) they “are no major contributor to the increase in PM_{2.5} mass concentrations during winter times” and ii) that “particles emitted from urban areas will only contribute little to the overall atmospheric dust and biogenic particle load”. First, I agree that they are not a major contributor to PM_{2.5}, but this is not a good argument because PM_{2.5} was not found to correlate with the INP concentrations. Second, although the concentration of urban bioparticles could be small, if they have good ice nucleating abilities, they can significantly contribute to the INP concentrations. I don’t clearly understand why urban bioparticles are negligible and do not make sense to the authors.

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

Authors: The BC was measured by a multi-angle absorption photometer (5012, MAAP, Thermo Fisher Scientific, Waltham, MA, USA) which got the sampled air through an inlet with a 2.5µm cut-off. This had been included in the previous version of the manuscript. The instrument measures the absorption of particles collected on a filter with a time resolution of 5 min and automatically derives BC mass concentration from the measurement while accounting for multiple scattering occurring on the filter. The MAAP is a well known and often used instrument for the measurement of absorption coefficients and BC mass concentrations.

The respective retrieval of BC values is now added in line 143 ff: *“The instrument measures the absorption of particles collected on a filter with a time resolution of 5 min and automatically derives BC mass concentration from the measurement while accounting for multiple scattering occurring on the filter.”*

Reviewer: The reviewer is very familiar with the MAAP and perhaps my questions was not well formulated. If I understood correctly the concentration of the ions was obtained from the PTEF filters, right? But what it is unclear to me is if the BC concentration reported in Figure 1 is obtained directly from the MAAP or if this was obtained off-line (using other technique), similar to the ions concentrations. If the reported BC data in Figure 1 is from the MAAP, is it directly comparable to ions off-line data?