Anonymous Referee #1

Received and published: 22 January 2013

For clarity and easy visual distinction, the referee comments are copied here in black and the authors' responses are offset in blue below each referee statement. Page and line numbers refer to online ACPD version.

The authors present simultaneous observations of ice nuclei concentrations and measurements of biological particles, using a variety of methods, from a field campaign in a temperate forest. Based on these observations, they argue that an observed increase in biological particle concentrations (as estimated from fluorescence measurements) immediately after rainfall can explain a simultaneous and highly correlated increase in the atmospheric concentration of ice nuclei observed at -25 deg C. They present a compelling case for this scenario. In addition, they identify two previously unknown warm-temperature ice nuclei among the fungal spore species in their samples. This is an excellent and novel contribution to the literature on both atmospheric biological particles in general, and the ice nucleation activity of atmospheric particles. While the release of biological particles after rainfall has been reported previously, this may be the first such report using real-time fluorescence measurements. Furthermore, I believe this is the first time that such a direct link has been demonstrated in field observations between atmospheric concentrations of naturally-occurring biological particles and ice nuclei. I highly recommend this paper be published in Atmospheric Chemistry and Physics after a few very minor revisions, discussed below.

We thank the referee for his/her helpful comments and for the recommendation of publication after minor revisions. We have processed the referee's comments into the revised manuscript as detailed below.

p. 1775, lines 4-8: "The total concentration of coarse particles ... increased ... which is in contrast to the traditional view ... that assumes efficient removal of large aerosol particles by precipitation." This sentence seems to question that large particles are removed efficiently by rainfall, but I don't think that this is what you mean. Perhaps consider rephrasing, e.g. something like "...in contrast to our expectation that the concentration of large particles would decrease following rainfall due to efficient removal"?

We have rewritten the sentence to be clearer. The text now reads:

"The total concentration of coarse particles (> 1 µm), including non-fluorescent material, increased less dramatically but also substantially (by 10-65% per minute, Fig. 1C). In contrast to the expectation that the concentration of large aerosol particles would decrease during rainfall due to efficient removal mechanisms (e.g. precipitation scavenging), our online measurements show that …"

p. 1777, l. 29 – p. 1778, l. 1: The authors write "that aerosol samples collected during rain events exhibited the strongest IN activation at temperatures above -20 deg C and sizes around 1.8-5.6 _m (Figs 3 and S1)." I find this phrasing imprecise and a bit confusing; I'd suggest something like "that for aerosol samples collected during rain events, the atmospheric concentrations of warm-temperature IN (active at -15 deg C) were highest at sizes around 1.8-5.6 _m (Figs 3 and S1)."

We have rewritten the sentence to be clearer. The text now reads:

"The microscopic experiments showed that for rain events, the concentration of IN at relatively warm temperatures (-15 ^{o}C) was $\geq$ 0.79 L^{-1}, and these warm-temperature IN had sizes around 1.8-5.6 µm (Figs. 4 and 5) …"

p. 1778, lines 4-6: there is a similar problem here. All samples (not just those collected during dry weather) display higher IN concentrations at T < -20 deg C than at T > -20 deg C, but the text seems to imply otherwise; also, Figs 1 and S3 show IN number per liter of sampled air, not the IN activation efficiency (# IN / # particles) as the text seems to imply.

Again, we have rewritten the sentence to be clearer. The text now reads:

"In contrast, during dry weather conditions dominated by dust the concentrations of IN at -15 ^{o}C were between 0.01 and 0.02 L^{-1}, and a relationship between size and IN concentration was not obvious at either -15 ^{o}C or -20 ^{o}C (Figs. 4 and 5)."

SOM:
Fig S1: This figure shows IN number per liter of sampled air, rather than the IN activation efficiency (# IN / # particles). If it is easily available, it would be interesting to also see the IN activation efficiency for the sampled particles in a second, similar figure in the SOM.

If we understand correctly, the referee is requesting a plot similar to Figure S2 (now Fig. 6), separated for each individual filter sample. The data for this plot are not readily available, however, and would require a significant amount of time to process. We thank the referee for the idea, but feel that the effort would be beyond the scope of the current formulation of the paper.

I have a final comment on Figs 1(E) and S1: For individuals with some common types of color-blindness, the colors of all lines in Fig S1 except stages 4 and 5 are indistinguishable or nearly so; as a result, this figure will be partially illegible to some readers (you can cross-check this at www.vischeck.com). The UV-APS data in Figure 1(E) is also somewhat reduced in legibility for color-blind individuals. While this of course not a scientifically critical issue, it could be easily corrected by choosing different colors. If the authors wish to address this, many good recommendations for improving colorblind accessibility of scientific graphics are available on the internet, for example here: http://jfly.iam.u-tokyo.ac.jp/color/.

This is an important point that we had not addressed ahead of submission, so we thank the referee for raising this issue. We changed the color scheme of Fig. 1E and made the symbol choice more systematic for the traces in what was Fig. S1 in the submitted version (now Fig. 5).