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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice-nucleating particles impact the severity of precipitations in West Texas" *by* Hemanth S. K. Vepuri et al.

Anonymous Referee #2

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Review of Vepuri et al.

The authors present a set of observation of INP concentrations from rainwater samples collected over West Texas during a 13 months period. They also measure data of precipitation properties, atmospheric temperature, relative humidity and air quality. In addition to that, they performed a metagenomics analysis to obtain information about bacteria present in the rain samples. I personally think that the technical methods used are correctly presented and used. However, the interpretation of their results and the relations they claim be proving between INP and precipitation intensity seems to me completely inaccurate and not backed by their method nor their data at all. There are 4 major points why I think this paper should be rejected:

-Correlation does not imply causality: The intensity of rain is subject to change due

to dynamical a thermodynamical factors. For example, I would expect that the large increases in CAPE over the summer season make convective clouds much more intense due to stronger updrafts than those observed on the more stratiform precipitation characteristic of winter like cyclone driven rain. None of these points is addressed minimally in the paper although they are the main drivers of precipitation intensity. INP concentrations can also change seasonally due to a variety of factors (dust transport, dryer conditions, higher biological productivity etc. . .), such factors are also barely mentioned. Finding a correlation between these 2 variables does not imply any type of causality between them. Also, in case you find a strong correlation, you should attempt to see what direction is this going, is it INP affecting rain or rain affecting INP? You could likely do a similar study with any variable, and you might likely find similar correlations.

-Wet deposition on rain particles is not properly addressed: Whereas they mention that surface PM does not correlate with INP, this does not discard that wet deposition might be affecting their results. Surface PM is not necessarily a measure of free tropospheric aerosol concentrations, and it is well known that during strong precipitation, aerosol concentrations tend to decrease due to wet deposition. The non correlation between PM and INP is perhaps showing that INP concentrations might be independent on the total aerosol concentration, which is likely given their rareness. The authors could measure the importance of wet scavenging by analysing the number of particles in their rain samples collected at the surface and just below cloud. There are strong evidences in their data that point towards wet scavenging being critical, such as how their largest INP concentrations occur on snow samples, which are best at wet scavenging.

-Their statistical analysis is not presented in detail and strongly limited to a few self-selected data samples: The two-sample t-test is a parametric test. Therefore, first they need to show that their distributions are normal, which I think they probably are in logarithmic scale but not on linear scale. Then, they need to present their results clearly and broadly in a reproducible manner, showing the number of datapoints going in each of the calculations and which dataset are you comparing. Currently they only show the final p-value for a couple of comparisons at high temperature which to me seems not valid at all for a scientific publication.

-Their data seems to show many times the opposite to what they claim: Looking at the available data in the supplementary, I can see that intensity of the rain types increases from snow to weak rain to long-lasted rain to hailstorm (being this last one the most intense) (Table S1-3). The INP values presented in table S3-1 do not correlate at all with their conclusions, being typically snow the precipitation category with the highest INP measured (at -10, -15, -20 and -25C) while having the weakest intensity. Of course, this is not the same analysis as performed by the authors, but given the data available in the paper, it seems that the conclusions should, in any case, go the other way around.

Extra comments.

Section 3.2. I like that the authors address the wet deposition factor in this section. However, I do not understand why they relate directly wet deposition with the ambient PM. Wet deposition depends on many factors (size distribution of particles, height from where the droplet falls, etc…) It could have been much more accurate to measure directly the number of particles in each of their precipitation samples.

L366. Whereas a measurable decrease in surface PM during rain suggests a clear removal by wet deposition, a non-measurable decrease in surface PM does not discard wet deposition as the particles could have been absorbed higher up and in amounts below the detection limit.

L393. Snow is a much better scavenger of aerosols than rain. This might be a likely explanation on why you get higher INPs in snow samples. You could test this by measuring the number of particles (and their size distribution) in your snow samples.

L397-397. I do not see the link here between these 2 ideas.

L426-429. This observation goes against the conclusions of the paper. You observed

lower -10C INP values during the May-Aug season when precipitation is stronger (due to the appearance of convective storms) than in the Nov-Jan season.

L430-433 How many points with -5C INPs in the hail/thunderstorm type where included in the analysis. It seems from the plot that there was only 1 point or that all points had the same value. In the supplementary this information is not included.

L440 As per my previous comments, I am not sure how many points are included in this analysis.

L445 what are the results of the statistical analysis over the other temperatures? Showing only the -5C p-values is not enough.

L447 I don't think this statement is backed by your results.

L515-517 Showing some correlations that might be affected by seasonal variations is not enough to claim such a statement.

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