Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-863-RC1, 2020

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 #1

Received and published: 2 October 2020

Throughout the course of 13 months the authors have sampled 42 precipitation events in the Northwest of Texas and analysed INP concentration in the hydrometeors. Parallel observations included the size distribution of hydrometeors, airborne particulate matter, air temperature and humidity. Precipitation samples were further subjected to metagenomic analysis, together with a dry deposition sample collected at the same site and suspended dust samples from a cattle feedlot about 50 km away. Data on this variety of parameters was then combined in an interpretation involving numerous implicit and some explicit assumptions, but neglegting two important issues: (a) that surface level air mass on a plain is not necessarily the same as the air mass where precipitation forms (typically "... 2 km to 9 km above ground level ..." (line 66); for vertical gradients in INP concentrations see He et al. (2020)), and (b) that hydrometeors scav-

Printer-friendly version

Discussion paper

enge particles between cloud and ground level. Latter was clearly demonstrated by Hanlon et al. (2017), who produced showers of artificial, sterile rain from a road bridge and collected the artificial hydrometeors, including microbial ice nucleators scavenged during 55 m of free fall, on the field below.

Figure 4 exemplifies the problem I see with the combination of little-related data and ignored processes. The Figure combines INP data on airborne dust samples near ground (feedlot, 50 km away from other observations), INP estimates of atmospheric INP concentrations at cloud height derived from precipitation samples and an assumed cloud water content (ignoring scavenging of particles and loss of water through partial evaporation of raindrops during free fall (ground level RH during rainfall 31% to 71% (line 309)), and an atmospheric INP estimate based on a dry deposition sample suspended in an (arbitrary?) volume of pure water and transformed into an atmospheric concentration value. I think the data from these three kinds of sources can not be directly compared because of mentioned issues.

However, the paper definitively contains new and interesting observations that may be interpreted to a certain extent, without making too many implicit or explicit assumptions. These observations are foremost the INP concentrations in precipitation samples combined with the precipitation properties, including kind of precipitation, size spectra of the hydometeors, precipitation duration and intensity. Such an interpretation needs to address the issues of below-cloud scavenging and also the higher scavenging efficiency of snow as compared to rain (Wang et al., 2014). I also found interesting the occurence of marine bacteria in the precipitation samples. In contrast, data on particulate airborne matter near ground level is something I would put aside when revising the manuscript.

Minor issues

I found it tedious to read through listings of data in the Results and Discussion section. Somehow, I missed a clear storyline. It would have been a more engaging reading

experience, if Figures were not introduced by full sentences that resemble Figure legends. To give an example (lines 372 and following): "Figure 4 shows the IN spectra for different precipitation types analyzed in this study superposed on the IN spectral boundaries adapted from a previous precipitation INP study (Petters and Wright, 2015). This figure also displays other reference IN spectra, including our 24-hour dry deposition blank sample (collected from January 2 – 3, 2019 at our sampling site) and IN spectra measured for dust suspension samples collected from the downwind side of a local feedlot (identity purposely concealed), where substantial and consistent dust emission historically persists (Whiteside et al., 2018). For the measured T range, nINP values from dry deposition blank sample were at least an order of magnitude lower than that from our precipitation samples." This entire section could simply be replaced with: "For the measured T range, nINP values from dry deposition blank sample were at least an order of magnitude lower than that from our precipitation samples (Figure 4)."

What is meant by (line 313): "...substantial number of precipitation particles with a cumulative number of 2E+05 to 6.6E+05 per event." Perhaps "...precipitation particles recorded by the disdrometer...", or "...precipitation particles per square metre..."?

Lines 323-327: It is not clear why the range of intensities is indicated as "0 to 150 mm hr-1", when maximum intensity was 129.3 mm hr-1 and minimum intensity 1.1 mm hr-1?

References: Whiteside et al. 2018: I would have liked to learn more about this study, but could not find it. A link to the paper, if available, would have been usefull.

Gabor Vali determined INP spectra in rain and hail samples from numerous storms in various parts of North America (Vali, 1968). The authors may find it helpful to have a look at his work when revising their manuscript.

References

Hanlon et al. (2017) Microbial ice nucleators scavenged from the atmosphere duringsimulated rain events scavenging of particles by hydrometeors. http://dx.doi.org/10.1016/j.atmosenv.2017.05.030

He et al. (2020) Aircraft observations of ice nucleating particles over the Northern China Plain: Two cases studies. https://doi.org/10.1016/j.atmosres.2020.105242

Vali (1968) Ice nucleation relevant to formation of hail. https://escholarship.mcgill.ca/concern/theses/h702q709t?locale=en

Wang et al. (2014) Development of a new semi-empirical parameterization for below-cloud scavenging of size-resolved aerosol particles by both rain and snow. https://doi.org/10.5194/gmd-7-799-2014

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-863, 2020.