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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mixed Phase Orographic Cloud Microphysics during StormVEx and IFRACS" *by* Douglas H. Lowenthal et al.

Anonymous Referee #2

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The authors present the analysis of an observation of orographic mixed-phase clouds with ground-based in-situ instruments at the Storm Peak Laboratory. The 92 hours data were analysed in a statistical way to explore relationships between microphysical properties and draw conclusion about the ice crystal formation processes.

The impressive dataset of 92 hours of mixed-phase cloud measurement is relevant for a publication and it fits in the scope of ACP, but I suggest major correction are needed before publishing this paper. A large part of the argumentation uses the cloud particle concentration between 25-100 μm. This is a delicate size range. On the one hand, the CIP have a larger uncertainty in the smallest sizes bins because of diameter corrections. On the other hand, it cannot be assumed that the transition between liquid droplets and ice crystals is under all condition at the same size range. A more thorough

discussion of the uncertainties and assumptions would strengthen the argumentation of the paper.

For some argumentation, the interpretation of the data is inconclusive. Why is a 10-fold increase unrealistic, although the MTAS it doubled? Could the relationships between large cloud droplets (CDNC25-35) and small ice crystals (Conc75-200) also caused because a large percentage of the CDNC25-35 are ice crystals? Can a relative enchainment of small ice crystals be excluded using the relative PSDs in Figure 7?

Specific comments:

Line 119 – 121: A more detailed discussion about the setup, including a picture, would be beneficial to discuss the influence of the local surrounding including building. In particular, if the high Conc75-200 in the NNW sector (discuss in Line 210-214) could be due influence of the railing, terrace, etc.

Figure 3 right side and Figure 5: To decide if the difference is significant, I suggest an estimate of the measurement uncertainty (either by error bars or by a discussion of the measurements uncertainty).

Line 229 and Figure 5: Could the particles larger than 35 μm also be ice crystals?

Line 235-237: How can from Fig. 1 and 4 concluded that the liquid cloud was not effected by the ice phase?

Line 241 – 243: Without the plot, it is hard to follow and visualize the argumentation. Consider to include the plot in the paper?

Figure 5: Shading of the times used for the low/high IWC analysis would increase comprehensibility.

Line 259 – 263: I suggest giving the two cases a clearer name, e.g. Low-Ice and High-Ice. I was confused that in line 259 – 263 concentration where give behind IWC and thought for a while that with IWC is the acronym of ice water concentration.

Line 261-262: Could the particles larger than 35 μm also be ice crystals?

Line 269 – 273: As the amount of blowing snow non-linear increases with the wind speed, I would assume that the MTAS is more relevant for the amount blowing snow particles. I find a 10-fold increase not unrealistic, in particular as in Beck et al., 2018, a case is shown in the upper panel of figure 9, where a 10-fold increase is measured above a sharp threshold wind speed.

Line 275 – 279: Could also a higher percentage of the CDNC25-35 be ice crystals than indicated by the wet/dry ratio? During wet condition, processes like hoar/surface frost (Lloyd et al., 2015) could have produced more small ice crystals or a stronger overestimation of the concentration in smallest bin of the SSP due to wrongly correction of the size of ice particles. A look at the overlap of in the size distribution (similar to Fig. 1) stratified by wet and dry condition could help to understand it.

Line 333 – 340: In my view, there is a misunderstanding in some of the conclusion from the Beck et al., 2017 paper. In the Beck et al., 2017, paper the authors conclude that ICNCs decrease with height. ICNCs near the ground are at least a factor of 2 larger also when the SBO was in liquid clouds.

Line 347 – 354: If the CDNC25-35 were dominated by ice crystal produced by a ground-based process depending on the TAS, the high importance of CDNC25-35 could be also a consequence of the TAS.

Line 356 – 363: The authors want to find a relative enchainment of small ice crystals due to blowing snow by using the relative PSDs of low and high wind cases (Figure 7). What effect would a relative a relative enchainment of small ice crystals have on Figure 7? In my understanding, the relative PSD would slightly increase for small diameter (because an increase of small ice crystal concentration, but also an increase in the total concentration) and would lead to a stronger decrease for large diameter (because the big ice does not increase much, but the total concentration increase). This changes, where observed in Figure 7. Maybe the author could show how different

PSD would translate to differences in the relative PSD of Figure 7?

Technical Corrections/Minor Comments

Line 17: to be consistent: “cloud condensation nuclei”

Line 40: write out acronym “mixed-phase clouds (MPC)”

Table 1: Unclear where the parameters measured by the CIP stops and where the SPP-100 parameter starts. For consistency write out TAS.

Line 182: To be consistent with units it should be “sampling flow speed”

Line 183: In my understanding with higher flow speeds in the inlet than outside you have superisokinetic sampling, which leads to an undersampling of the large particles and not an oversampling of smaller droplets.

Line 198 – 207: This part was hard to follow and a rephrasing might help. If the first two CIP channels cannot be trusted, which I agree to, than the argumentation might be obsolete.

Figure 3: Mention that the colors on the left side are the liquid water contents

Line 240: The sentence “Henceforth, the Spearman Rank correlation is displayed in parenthesis after the Pearson correlation.” is confusing, in particular as the Spearman Rank is not in parenthesis two sentence before.

Line 286: In the text is written “29 data points” are used, but in Figure 6 N=79.

Line 296: I think it should be “IFRACS (Figure 6b).”

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