Anonymous Referee #2

The reviewer's comments are presented in italics, followed by our responses. The authors thank the reviewer for his insight.

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 um. 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.

One of the objectives was to test the hypothesis that ice particles were formed by heterogeneous freezing of large cloud droplets. Correlations between large droplet (25-35 µm, CDNC25-35) and small ice particle (75-200 µm, Conc75-200) concentrations were examined. In response to the reviewer's concerns, a closer look at the 1-second data revealed that the FSSP reported concentrations of 0 for particles larger than 25 µm for 86 and 44% of 1-second measurements during StormVEX and IFRACS, respectively. The paucity of non-zero CDNC25-35 during StormVEx is probably the result of a measurement issue related to the lack of the FSSP-100 scarf tube during that study. StormVEx is thus excluded from this analysis in the revised manuscript.

The reviewer rightly points out that it is not possible to observe the degree to which large droplets measured with the FSSP were actually small ice crystals. The implication is that if large droplets (25-35 µm) were actually ice crystals, the relationship between CDNC25-35 and Conc75-200 could simply represent autocorrelation between two segments of the ice crystal distribution. In the original Fig. 2, we demonstrated that on average, the CIP distribution was dominated by droplets in its smallest size bin, which corresponds to the large droplet size range. The reviewer suggests that while this may true in general, there may be periods when there is more ice in this size range than on average but it is also true that there may be periods when there is less ice than on average. Since our analysis is statistical in nature, variation in the proportions of liquid and ice in the 25-35 µm size range of the FSSP should manifest as noise in the relationship between CDNC25-35 and Conc75-200. Further, if large droplets were actually crystals, and this accounted for the relationship between CDNC25-35 and Conc75-200 at <-8 ºC, why do we see not see this relationship at >-8 ºC? In the revised manuscript, we acknowledge and discuss the limitations of the instrumentation in this regard and note that higher resolution instruments are needed to directly address these issues.

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?

Correlation of wind speed with crystal concentrations during the day on 9 February 2014 does not necessarily imply blowing snow. The revised analysis of this case (see below) shows an inverse correlation between MTAS and Ni during the High- and Low-Ice periods. This behavior is not consistent with blowing snow. Production of ice is dependent on mesoscale and orographic dynamics (Neiman et al., 2002; Stoelinga et al., 2013). Higher wind speeds across the barrier produce more lifting and produce new ice particles upwind and above the barrier. Clearly, not all of the variation in ice crystal concentration at mountaintop laboratories is due to blowing snow although blowing snow does occur. We provide evidence against the blowing snow hypothesis but acknowledge in the revised Conclusions that it can be a factor: "It is possible that both primary production and blowing snow were active at SPL.".

A key point is that higher-resolution instruments such as holographic imagers used by Beals et al. (2015) (HOLODEC) and Beck et al. (2018) (HOLIMO) are needed to distinguish liquid droplets from ice crystals in mixed-phase clouds. This is noted in the text and at the end of the revised Conclusions.

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.

A new Fig. 1 showing the probes on the wind vane and a view upwind of SPL is included in the revised text. There is no upwind railing, terrace, or building in any direction to the west. The wind data were re-examined to explain high Conc75-200 in the NNW sector. During StormVEx, mostly all of the NNW cases were on 22 January 2011. The 5-minute average wind direction was exactly the same (351.9º) for 3.5 hours. It is not likely that a 5-minute average value could be the same to a tenth of a degree for two consecutive 5-minute periods, much less eighteen. During IFRACS, many of the NNW wind directions exhibited the same value for thirty minutes or more. The reason is that the wind vane becomes iced by riming and doesn't move. The data were screened for repeated 5-minute wind speeds and these were eliminated. This reduced the number of 1-minute observations by 2 and 4.7% during StormVEx and IFRACS, respectively. Table 2 was modified accordingly. The largest difference was in the NNW sector (330-360º) during StormVEx, where Conc75-200 decreased from 460 to 150 L^{-1}.

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).

The reviewer is referring to comparisons of FSSP PSDs in the original Figs. 3 and 5. The implication is that the differences in the PSDs at high and low LWC, which occurred in both 2011 and 2014, could have arisen by chance. This seems highly implausible. A statistical comparison of mean PSDs could be used to test the hypothesis that these apparent differences were not significant, i.e., they were fortuitous. In any case, for such a comparison, a simple t-statistic would be the difference divided by the standard error (standard deviation divided by the square root of the number of observations). The standard errors are plotted in the revised Figs. 4 and 7. They are very small because of the large number of measurements used to calculate the mean concentrations as a function of size.

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

In general, the answer is yes, but as noted in below and in the revised text, measurements of large particles by the FSSP is problematic.

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

We do not conclude here that the FSSP distribution was *not* impacted by the ice phase. We stated that such interactions are not evident in the original Fig. 1. Figure 4 hadn't been introduced at this point and should not have been included in this sentence.

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

A new Figure 5 showing these scatter plots is included in the revised text.

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

The high IWC period is between the black lines in the revised plot. The low IWC periods are shaded in grey in the revised plot.

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.

This section was revised, referring to “High-Ice” and “Low-Ice” periods. Characterization of Ni during the High- and Low-Ice periods has been clarified.

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

Again, yes.

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.

As noted in the Methods section, the probes at SPL are mounted in front of a railing 6 m above the snow surface. Winds did not pass over the deck or from the direction of the building during sampling periods. The reviewer suggests that high ice particle concentrations (Ni) should have accompanied or followed the maximum wind gusts (MTAS). Figure 10 (revised), below, plots 1-second MTAS (Fig. 10a) and the corresponding 1-minute average TAS (Fig. 10b) against 1-minute average Ni for High-Ice, Low-Ice, and all other (Intermediate-Ice) periods on 9 February 2014. MTAS was highly correlated with TAS [0.90 (0.90] over the course of the day. The highest Ni correspond to the highest MTAS (and TAS) and *visa versa*. Average MTAS was 16.6±2.4, 8.9±2.0, and 11.3±2.8 m s^{-1} during High-, Low-, and Intermediate-Ice periods, respectively. This could imply that high Ni resulted from blowing snow when the winds were stronger in the early afternoon. Contrary to results reported in Fig. 9a in Beck et al. (2018), there was no clear MTAS threshold that produced a step function in Ni. In Fig. 10 (revised), there appears to be an inverse relationship between 1-minute MTAS and 1-minute Ni, especially for the High- and Low-Ice regimes. Beck et al. (2018) discussed how a relationship between MTAS and blowing snow could be averaged out if the averaging time is too long or obscured because of an [indeterminate] lag between the arrival of the gust and the particles that were lofted by it. Beck et al. (2018) suggested using an averaging time of 10-15 seconds. Figure 11 (revised), below, plots 15-second average (using the 75% data completeness criterion) Ni against MTAS for the High-, Low-, and Intermediate-Ice periods. The figure shows that while both MTAS and Ni varied considerably, there was no obvious wind speed threshold and the correlations between MTAS and Ni were actually negative under High- and Low-Ice conditions. These results are not consistent with the blowing snow hypothesis. This discussion and the new figures are included in the revised manuscript.

Line 275 – 279: Could also a higher percentage of the CDNC25-35 be ice crystals than indicated by the wet/dry ratio?

This is discussed in detail in response to the reviewer’s introductory remarks. There could be more but also less ice particles in CDNC25-35 than indicated by the average wet/dry ratio, which suggests that CDNC25-35 represents droplets. The fact that we see a relationship between CDNC25-35 and Conc75-200 at cold but not warm temperatures also suggests that CDNC25-35 represents droplets and not ice crystals.

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.

The suggestion that splintering of hoar frost on the snow surface leads to high Ni is speculation. There is no direct evidence in our data to support this. It is difficult to see how hoar frost on the surface would have been a source of ice particles since snow was always accumulating on the surface during the measurements at SPL. The most plausible surface-based mechanism for ice production is blowing snow, which is discussed at length.

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.

This paragraph was replaced by the following sentence at line 331: "Beck et al. (2018) reported a large increase in Ni when the maximum wind speed increased from 14-16 to ≥16 m s^{-1} at the Sonnblick Observatory in Rauris, Austria when winds were from the south.".

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.

See above. The reviewer keeps coming back to a hypothetical while the evidences suggests that in general, CDNC25-35 was not dominated by ice crystals.

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).

We assume that by "enchainment" the reviewer means "enrichment". First, Fig. 7 (revised Fig. 12) was modified to represent the same low wind speed range as presented in Table 3, i.e., 1-3 m s^{-1} rather than 1-4 m s^{-1}. The result is that there was an enrichment in small crystals (Conc75-200) at high wind speed of 10% during StormVEx and 8%

during IFRACS. However, this difference is small compared to the factor of 4.5-6.5 increase in the absolute concentration of Conc75-200 at the higher wind speed. Ice crystal concentration is not simply a function of blowing snow but varies with synoptic and orographic dynamics, i.e., stronger uplift nucleates more crystals upwind and above the mountain barrier (e.g., Neiman et al., 2002; Stoelinga et al., 2013). In fact, there were moderately strong correlations between 1-minute horizontal and vertical wind speeds during StormVEx [0.75(0.72)] and IFRACS [0.66(0.67)].

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?

It is unclear what the reviewer is asking for. The contrast in the original Fig. 7 was used to test a specific hypothesis, i.e., that blowing snow would be relatively enriched in small particles. Is the reviewer suggesting that we address a different hypothesis using relative PSDs?

Technical Corrections/Minor Comments

Line 17: to be consistent: "cloud condensation nuclei"

Done.

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

Done.

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

In the revised table, a vertical line separates CIP and FSSP parameters. TAS is defined as horizontal wind speed in the footnotes.

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

Done, however, since the scarf tube accelerated the flow during IFRACS, the wording was changed to "sampling flow speed at the inlet".

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.

The reviewer is correct. This could explain a loss of large droplets in the aspirated FSSP during StormVEx. Note that the flow accelerator (scarf tube) was used during IFRACS but not StormVEx. Gerber et al. (1999) discussed inertial concentration of large droplets in an aspirated FSSP fitted with a scarf tube (flow accelerator). Because the FSSP and CIP PSDs overlap more closely during IFRACS than StormVEx, the loss of large droplets due to super-isokinetic sampling may be somewhat offset by the inertial concentration of large droplets caused by the scarf tube. This is discussed in the revised text.

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.

We agree and have removed this section.

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

Done in the revised figure heading.

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 240 begins with "Henceforth", meaning after line 240.

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)."

This section has been extensively revised, as discussed above.

References

Beals, M.J., Fugal, J.P., Shaw, R.A., Lu, J., Spuler, S.M., and Stith, J.L., Holographic measurements of inhomogeneous cloud mixing at the centimeter scale. Science, 350, 6256, 87-89, doi:10.1126/science.aab0751, 2015.

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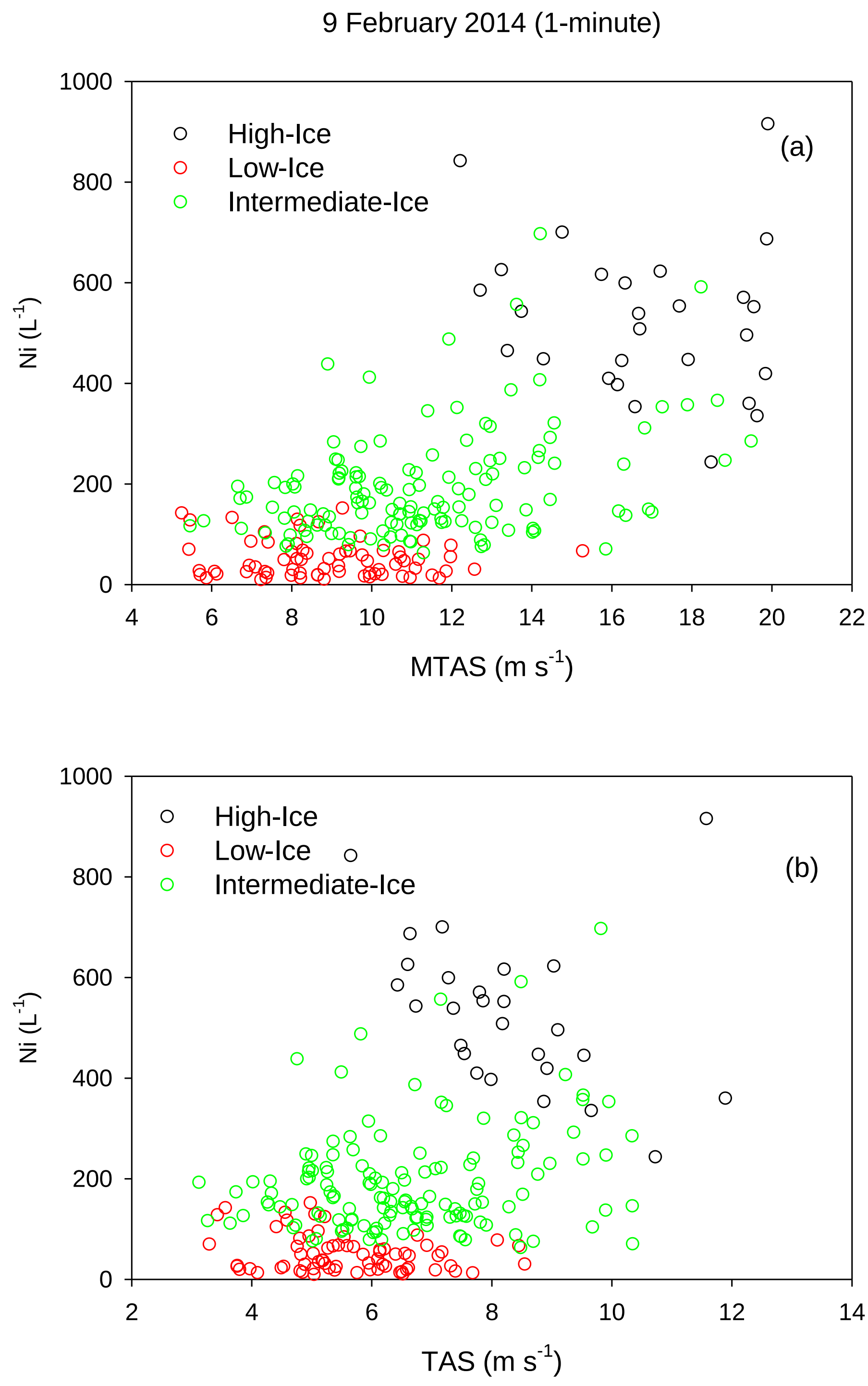

Figure 10 (revised). Relationships between 1-minute average TAS (a) and maximum 1-second TAS (MTAS) (b) and Ni for High-Ice, Low-Ice, and Intermediate-Ice (all other 1-minute periods) periods on 9 February 2014.

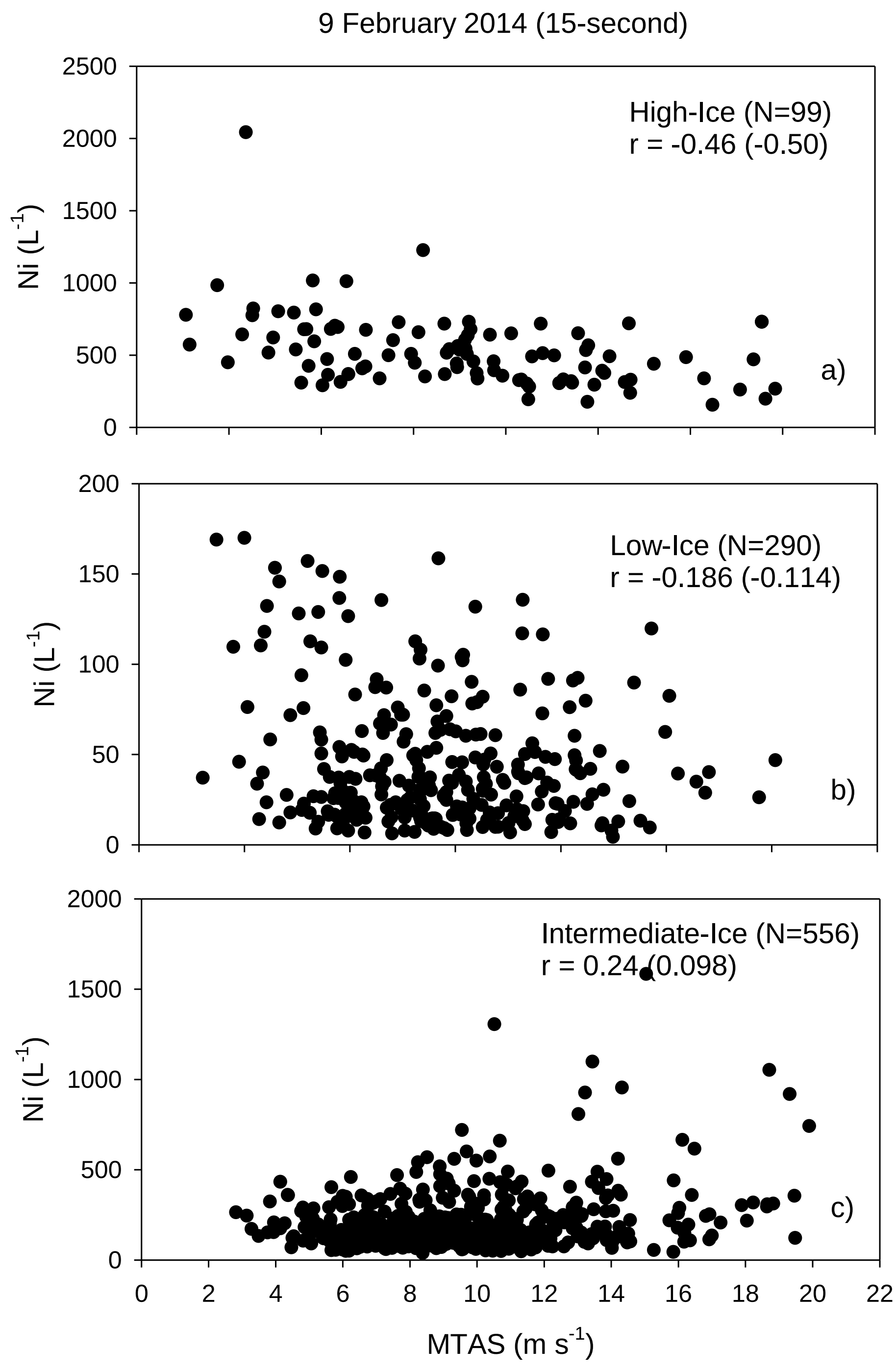

Figure 11 (revised). Relationships between 15-second average Ni and MTAS for High-Ice (a), Low-Ice (b), and Intermediate-Ice (c) periods on 9 February 2014.