Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-281-RC1, 2021


# Comment on amt-2021-281

Anonymous Referee #2

Referee comment on "Cloud microphysical measurements at a mountain observatory: comparison between shadowgraph imaging and phase Doppler interferometry" by Moein Mohammadi et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-281-RC1, 2021

This paper presents a thorough and quantitative comparison of two commercial instruments for measuring droplet number-size distributions, based on shadowgraph and phase Doppler interferometry. The study is particularly useful because the two techniques have not been commonly used in cloud measurements, so this helps define the strengths and limitations of the methods. The paper is well written and the presentation is generally clear. I recommend that the paper is accepted, subject to minor revisions. The following suggestions should help improve the work:

Figures 3 and 4 and the associated text leave some confusion about the orientation of the instruments relative to the wind direction. On line 225 I understand that the instruments are facing in a direction that allow optimal sampling of wind coming from 320 degrees. This could be indicated in the last panel of Figure 4, or at least mentioned in the caption. If that understanding is correct, what does it imply about the measurements on 13-Jul, 14-Jul, and 13-Aug when the wind direction is close to 90 degrees? Is it expected that the measurements are heaviliy influenced by the instrument housings? That may cause problems with the sample volume estimation for the phase Doppler interferometer. This is relevant to the presented results, e.g., see line 263 where average wind direction is stated as 225 degrees. Please address this aspect of the study, and clarify the directions as needed.

In the presentation of results on droplet number concentration (e.g., Figure 9) please provide some information on the range of number of droplets sampled for the data points that are shown. That would allow Poisson uncertainty to be evaluated. I expect the numbers are large enough that Poisson sampling uncertainty is not a significant contribution, but please show or discuss this directly.

In several of the plots I believe more insight could be obtained by showing results on logarithmic axes. For example, log-log coordinates might help in the interpretation of the

droplet number concentration results. But for sure it would be helpful in the presentation and interpretation of the LWC results in Figure 13. This would allow a broader range of LWC to be observed, rather than results being compressed near the origin because of a relatively small number of outlier points. I expect the least-square fits will be significantly different in log-log coordinates as well.

Another comment on data presentation: in Figure 10 it would make sense to show the y-axis in log scale so that the full variability can be observed. The polynomial fit seems rather arbitrary. Is there any justification for this form? Perhaps log-scale will suggest something else.

Another comment related to Figure 10: At large mean diameter there was a strong undercounting of the shadowgraph method or overcounting of the PDI method. If you use the polynomial fit to 'adjust' for this bias, how much improvement would result in the LWC results in Figure 13? This would be one way to further disentangle the number concentration and D^3 contributions. It seems possible that the PDI instrument is overestimating number concentration when the mean size is large, and that this is leading to the unreasonably high values of LWC.

The approach of the study is an instrument intercomparison, but without a known reference or standard. Generally, the authors point out differences without suggesting that any specific technique is correct or incorrect, and I agree with that cautious approach. (Note that one exception is on line 311, which should be reworded, or evidence shown that one method is indeed overestimating size.) In some places, however, it may be possible to argue that one instrument is more closer to reality, such as in Figure 13 where the PDI is showing significant numbers of data points with liquid water contents above 1 g/m^3. Based on other measurements at the UFS and similar mountain stations, I expect these values are much too high. More importantly, the paper is missing a discussion in the Conclusions section about how the investigation could be taken to the next level. Is there a way to perform an absolute calibration or provide additional instruments in future studies, so that the discrepancies can be understood and resolved? In the end, the community needs to know which of these instruments can be considered reliable, in what range of conditions.