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Atmospheric Measurement Techniques Discussions

Interactive comment on "Water droplet calibration of a cloud droplet probe and in-flight performance in liquid, ice and mixed-phase clouds during ARCPAC" *by* S. Lance et al.

Anonymous Referee #1

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Review of "Water droplet calibration of a cloud droplet probe and in-flight performance in liquid, ice and mixed-phase clouds during ARCPAC" by S. Lance, C.A. Brock, D. Rogers and J.A. Gordon

Recommendation: Acceptable for publication provided that recommended changes are made

This paper describes a laboratory calibration and the in-flight performance of a relatively new cloud microphysical probe, the Cloud Droplet Probe (CDP), which operates by determining particle size from the amount of forward scattered light by applying Mie

theory. I believe that this paper should be published for a number of reasons: 1) this is the most comprehensive paper I have seen that examines the behavior of the CDP and will serve as a standard reference for other interpretation of data from the CDP (it somewhat echoes a series of papers that were published about 20 years ago in JAOT investigating forward scattering spectrometer probes); 2) it describes a way of calibrating the probe using laboratory-generated liquid droplets, a nice technique that might ultimately serve as a standard calibration technique for such probes; 3) it offers an interpretation for the observed concentration-dependent discrepancy between the liquid water content derived from the size-resolved CDP and bulk measured values by a King probe, showing that coincidence errors may be responsible for these differences. Given these three contributions, I believe the paper should be published as the paper is also technically sound, well-written and easy to follow.

However, certain aspects of the presentation need to be improved the paper can be published. I document my concerns and suggests for improvement below.

MAJOR COMMENTS

1) The paper read more like a technical document than a journal article. A lot of details presented were not needed. I would encourage the authors to reorganize the paper leaving in only those details necessary to reach the conclusions of their study.

2) A main conclusion of the study is that an optical redesign of the CDP needs to be explored to reduce the coincidence bias (mentioned in both abstract and conclusions). The authors claim the CDP optics should be modified to limit the area viewable by the sizing detector. However, this may introduce another problem, namely an inability to obtain a statistically significant sample of cloud particles in multiple size bins needed to derive a drop-size distribution. If the area is reduced, fewer particles will be detected. In conditions of low droplet concentrations, this could be a problem especially the larger droplets important for warm rain formation and drizzle development that occur in lower concentrations than smaller droplets (i.e., a reduction in sample area would

hinder the ability to get statistically significant samples of large droplets). Hallett (2003, Measurement in the atmosphere in Handbook of Weather, Climate and Water: Dynamics, Climate, Physical Meteorology, Weather Systems and Measurements, T.D. Potter and B.R. Colman, Eds., John Wiley and Sons, 711-720) examines various criteria for defining a statistically significant size distribution. Assuming that the uncertainty is proportional to the square root of the number of particles measured in each size bin, Hallett quotes the statistical significance in terms of either the averaging time or flight distance required to get a large enough sample in each bin (assuming 100 particles in each bin giving a 10% uncertainty is adequate for statistical significance). What averaging times/distances would be required to get statistically significant samples of large particles and how does that impact the desired modifications to the CDP sample area?

3) The authors conclude that ice crystal shattering on the CDP cannot be significant because few particles are observed with the CDP in ice-phase conditions. However, I think more analysis is needed here (see also comment 5 on identification of phase). Several experiments are starting to show that at least some versions of the CDP do not seem to record any particles in ice clouds. Is this because no small ice crystals exist in ice-phase clouds, or is it because the CDP cannot detect these ice particles? Can the difference in the rectangular slit configuration of the CDP mask (instead of the circular masked central region of the FSSP) lead to the rejection of small ice crystals? This is especially concerning because some recent experiments presented at the Oregon cloud physics conference showed that the CDP does not detect ice particles even when an un-shrouded FSSP does detect ice particles. I think a more thorough investigation of the CDP's ability to measure ice crystals is needed before the statements about ice in this manuscript can be substantiated. In particular, can the calibration be repeated with non-spherical ice analogues?

4) I am curious to what degree the conclusions presented in this paper refer to the NOAA CDP in particular, and to what degree the conclusions also pertain to other CDPs. In this regards, I have a couple of questions. During ARCPAC, there was

one flight where the NOAA P-3 flew in coordination with the NRC Convair-580. It would seem that comparing the CDP data from the two aircraft would help answer this question, and also give more confidence on the data that were obtained by the NOAA CDP. I believe that the Convair CDP did not suffer the same systematic offset that is quoted in this paper. How did the size distributions between the probes in similar clouds compare? Also, to what degree did the calibration of the CDP drift during the field campaign (i.e., were the pulse heights as a function of diameter consistent with time)?

5) What phase identification scheme has been applied to the collected data? Very often phase varies with horizontal and vertical position, so phase identification typically needs to be determined at each point in cloud (where point is defined by the averaging interval). I'm not convinced this has been done here and it should be doneâĂŤthen the analysis can be segregated according to those data points collected in liquid and those data points collected in ice. I think that the definition of ice-only for King hot-wire contents less than 0.1 g/m3 will misidentify some liquid and ice-phase clouds. Was there a Rosemount probe for identifying supercooled water? Was the shape of the droplet size distribution used to help identify phase?

DETAILED COMMENTS

Page 3139, line 13: I do not understand why the baseline drift cannot be corrected for. Granted, one must take into account the uncertainty in correcting for the baseline drift in deriving the final LWC product, but to not do so would seem to produce a systematic error in the data (i.e., even LWC values greater than 0.1 g/m3 would seem to be somewhat offset if the baseline is not removed).

Page 3139, lines 16 to 18: How can the accuracy of the King LWC measurements be evaluated from the adiabatic profiles? The departure from adiabatic conditions could be much larger than the uncertainties of the measurements.

Page 3139, lines 26 to 28: If the baseline offset is removed from the J-W LWC values, how do they compare against the King LWC values? This would seem to be a good test to perform before categorically rejecting all measurements from a particular probe!

Page 3140, line 4 to 5: There are many algorithms readily available for removing shattered particles from CIP/PIP probes, for correcting the sizing of particles due to out-of-focus images, and for identifying other artifacts in the data. I understand not wanting to discuss all the uncertainties in these data, but I think these corrections should be applied so that the corrected data are used.

Page 3143, lines 24 to 27: Can you be more specific in how the probability of less than 5% is calculated from the quoted concentrations, sample volumes, and sampling frequencies (i.e., give equation or other source)?

Page 3147, line 30: Is this 10% ratio measured here or from the Wendisch study? If from the Wendisch study, how can you know that the ratio is the same?

Page 3149, lines 29-30: This seems surprising. Given the importance of this calibration can you find or speculate on some reason to explain this monotonic response?

Page 3151, line 20: Why not show data from all flights together to show that the behavior is consistent between all flights?

Page 3152, line 5: What is the fractional contribution to the mass of the liquid in the mixed-phase clouds? Korolev and others have shown that mixed-phase clouds are typically dominated by either liquid or ice. If you estimate the ice content from the size distributions (e.g., through application of some m=a D^b relationship, where a and b chosen dependent on the habit types that are dominant), you should be able to estimate this. If the clouds are liquid dominated, the King probe probably will work to give a reasonable estimate of liquid water content.

Page 3153, lines 13-15: Can you state what the sample areas Q and E are?

Figure 2: how did pulse amplitude with droplet diameter vary over the course of ARC-PAC?

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