Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2020-476

Anonymous Referee #2

Referee comment on "Application of cloud particle sensor sondes for estimating the number concentration of cloud water droplets and liquid water content: case studies in the Arctic region" by Jun Inoue et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-476-RC2, 2021

The authors present a new instrument, a Cloud Particle Sensor (CPS), that can be used for vertical probing of cloud particle number concentration, phase and size from radiosondes or similar platforms. The technical details of the instrument were introduced in a paper by Fujiwara et al. (2016) and in the current paper, the authors present field data taken from 40 CPS sonde launches in the Arctic region. While development of reliable and inexpensive in-situ measurement techniques for tethered balloons and radiosondes is important to complement aircraft and remote sensing measurements, which are restricted in time and space, the proposed method to correct for CPS raw data to get scientifically meaningful quantitative values for total concentration and liquid water content (LWC) is not convincing. The sensitive area of CPS is not characterised either using theoretical calculations nor using droplet injector mapping. The assumption to use cross-sectional area of the CPS inlet as sensitive area is wrong and the applied correction factor to account for flow dynamics is not convincing. Without proper characterisation of the sensing area, the presented results cannot be recommended for publication.

Major comments

1. In order to derive particle concentrations, the information of the sensitive area of the instrument is crucial. The sensitive area is defined as the overlap between the laser beam profile and the detector filed-of-views (FOV). However, no information is given for their geometry. Instead, the authors claim that the “detection domain” is 1x1x0.5 cm based on the dimensions of the slits and in the derivation of total concentration the counts are divided by the cross-sectional area of the CPS inlet (1 cm2), which is not the same as the sensitive area.

2. Since the sensitive area is not known, estimation of total concentration is not possible.

3. The analysis is based on the assumption that the particles have a constant PSW. In order this to be true, the laser profile should be uniform in the detection area and the flow of the particles should be constant. Why no technical efforts were made to fulfil these conditions (e.g. beam shaping, focusing air flow, etc.)?

4. The size calibration is based on standard particles. Although the differences in refractive index is taken into account, the authors could have repeat the calibration using water droplets distributed with piezo-injector, which is the common practise for cloud instruments. Same method could have been used to map the sensing area.

5. According to flow calculations, the flow speed at the inlet is reduced by 17.2% due to the instrument housing. At the same time air pressure increases. After reaching the instrument, the flow speed accelerates to a value close to the flow speed before the instrument. The authors interpret these calculations so that "the chance that the air mass can enter the CPS inlet in a unit of time is reduced to 17.2%" and calculate a correction factor for the total counts to be 5.8 (= 1/0.172). However, I consider this reasoning to be incorrect.

Minor comments

p.2, lines 49-52: Cloud phase can be determined in-situ using number of different methods but the authors only mention Cloud Particle Imager. I would suggest referring to paper by Baumgardner et al., 2017.

p.4, line 93: The terms "particle signal width" (PSW) and particle transit time are used. Why not to use the term particle time-of-flight (TOF) that is frequently used in the community?

p.5, Section 2.5: No explanation is given for the chosen DOP threshold. Additionally, the separation is based on particle sphericity rather than actual ice/water phase. This should be mentioned.

Fig. 6: I don't see that accumulated relative PSW frequency is a good way to illustrate the PSW distribution. Why not to show normalised PSW frequency? Why is the data limited to water cases (DOP>0.5)?

Fig. 10: What is the upper detection limit of the OPC?

Fig. 10b: Why is the OPC counting particles with concentration >100 L-1 below the cloud base?

References

Baumgardner, D., Abel, S. J., Axisa, D., Cotton, R., Crosier, J., Field, P., … Um, J. (2017). Cloud Ice Properties: In Situ Measurement Challenges. Meteorological Monographs, 58, 9.1-9.23. https://doi.org/10.1175/amsmonographs-d-16-0011.1