Reply to Reviewer #2

We would like to thank you for providing highly technical comments regarding our manuscript. We substantially modified the content in the revised form and believe the modifications will be satisfactory and suitable for publication. The point-by-point responses (in blue) are made below.

Overview of the revision

The reviewer may consider that we are the original members who developed the CPS sondes (this is probably because the title of our original manuscript might provide misleading information); however, this is not the case. Note that we have not been involved in the development of the CPS sondes. We contacted the manufacturer (i.e., Meisei Electric, Co. Ltd.) to obtain the technical information.They explained to us that the shape of the laser beam is not uniform as it is but is adjusted to be uniform with Biconvex lends (the reference is not available); the actual dimensions of the two parallelograms are not evaluated by a optical software.

We substantially revised the paper, including its title, from the viewpoint of a CPS sonde user to clarify our approach and deliver the message to the manufacturer for further development and experiments (e.g., mapping of the sampling area as suggested by the reviewer). Based on CPS sonde measurements of Arctic low-level clouds, we demonstrated that the original approach published in AMT (Fujiwara et al., 2016) should be improved. For this reason, we still believe that AMT is the best platform for reporting our experiences of CPS sonde measurements in the Arctic and then discussing the need for a new correction method.

The structure of this paper has been simplified as (1) Introduction, (2) Experimental designs, (3) Data processing, (4) Comparison with other data sources, (5) Discussion, and (6) Conclusions. The previous Figs. 3 and 7 and their explanations related to the optical design and sampling area mapping have been deleted because we would like to highlight the observed data, particularly the smaller particle signal widths (PWDs) than those expected by Fujiwara et al. (2016). We also removed some simulated results (previous Figs. 9 and 13) to keep the manuscript simple. Since we realized that additional laboratory experiments would be necessary in future work, their needs have been discussed in the new section by citing literature. The correction factor was newly estimated in section 3.4 by the idea of collection efficiency based on Noll and Pilat (1970), improving the theoretical robustness of estimation of the factor, although the main result was not changed.

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.*

   By the definitions by Fujiwara et al. (2016), the volume of the detection area is 0.5 cm^3, while the cross section of the detection area is 1 cm^2. We followed the value of the cross section of the detection area to calculate the number concentration. Therefore, "cross-sectional area" has been reworded as "the cross section of the detection area". The results were not changed.

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

   Because the detailed assessment of the sensitive area is beyond the scope of this study, which the manufacturer would hopefully do, we assessed how to treat and interpret the output of total count by the CPS sonde from an end user perspective. Even if the cross section of the detection area is unknown for an end user, we think that the efforts to validate the total concentration are still meaningful.

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.)?*

   The analysis by Fujiwara et al. (2016) was based on the assumption that the particles have a constant PSW. However, we were not based on the assumption because our field measurements indicated much lower PSW values. This was a central motivation of our study; however, the optical assessment and consideration were removed (previous Figs. 3 and 7) because we focused on the observational results from the standpoint of a CPS sonde user. In addition to this, we communicated with the manufacturer to obtain information on the shape of the laser beam. The issue has been included in the new section 5.3.

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.*

   The additional laboratory experiments using optical spectrometers and precise droplet generators would provide more realistic data but are beyond the scope of this research because the aim of this study is to provide the practical method to correct the CPS data under the existing system from a user perspective. Instead of this, we cited papers Lance et al. (2010) and Beswick et al. (2014) to introduce how the state-of-art has been applied to calibrating cloud microphysics probes, which must stimulate the manufacturer (i.e., Meisei Electric, Co. Ltd.) for further development of CPS sonde. This issue is discussed in the first paragraph of the new section 5.3.

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.*

   In this paper, we used the data based on the field experiments. We do not conduct laboratory experiments to validate the simulated flow field. Instead, we compared the flow speed data at the bottom of the CPS inlet observed by anemometers in Fujiwara et a. (2016). The flow speed was 15% smaller than the ascending speed. Our simulations also have the same tendency even if the ascending speed is changed from 4 m/s to 6 m/s, suggesting that our simulations are valid for further investigating flow characteristics around the CPS housing. This content has been included in the new section 3.4. Then, the correction factor was introduced by the idea of collection efficiency in the new section 3.4 based on Noll and Pilat (1970), improving the theoretical robustness of estimation of the factor, although the main result was not changed.

Minor comments

1. *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.*

   Thank you for introducing the paper. We added Baumgardner et al. (2017), Lance et al. (2010) and Beswick et al. (2014) in the introduction.

2. 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?

   We'd like to just follow this term (PSW) from Fujiwara et al. (2016, AMT) for the consistency and readability in AMT papers.

3. 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.

The threshold of 0.3 was originally proposed by Fujiwara et al. (2016) based on laboratory experiments using standard particles; however, they also showed that the DOPs for liquid clouds were usually higher than 0.5 in actual observations (Figs. 4a, 7a, 10a in Fujiwara et al. (2016)). Because the mixed-phase clouds are typical form in the Arctic, the value of 0.5 would be more suitable than 0.3 to reduce the chance of counting ice particles as liquid particles. This explanation has been included in the section 2.4.

4. 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)?

   The accumulative relative frequency of PSW is a suitable indicator to demonstrate how PSW takes a smaller values than 1.0 ms.

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

   Unfortunately, the detection limit is unknown by the instrument specification.

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

   These are not cloud particles but aerosol particles.

References newly included

1. Baumgardner et al. (2017), Cloud ice properties: In situ measurement challenges, Meteorological Monographs, 58, 9.1–9.23.

2. Beswick et al. (2014), The backscatter cloud probe – a compact low-profile autonomous optical spectrometer, Atmospheric Measurement Techniques, 7, 1443–1457.

3. Craig et al. (2013), Design and sampling characteristics of a new airborne aerosol inlet for aerosol measurements in clouds, Journal of Atmospheric and Oceanic Technology, 30, 1123–1135.

4. Lance et al. (2010), Water droplet calibration of the Cloud Droplet Probe (CDP) and inflight performance in liquid, ice and mixed-phase clouds during ARCPAC, Atmospheric Measurement Techniques, 3, 1683–1706.

5. Murakami and Matsuo (1990), Development of the hydrometeor videosonde, Journal of Atmospheric and Oceanic Technology, 7, 613–620.

6. Noll and Pilat (1970), Inertial impaction of particles upon rectangular bodies, Journal of Colloid and Interface Science, 33, 197–207.