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Discussions

Interactive comment on "Development of a cloud particle sensor for radiosonde sounding" *by* Masatomo Fujiwara et al.

Anonymous Referee #3

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This paper describes the new balloonborne instrument of cloud particles whose signal data is transmitted with a radiosonde to a ground station. The new sensor, CPS has capabilities to count particles larger than about 2 microns and to distinguish between cloud water droplets and ice crystals. The authors also mention some limitations using the test flight data in several launch sites.

However, it is not clear what levels of accuracy and range of uncertainties the new sensor has during the ascent flight. Although it might be the first light-weight cost-effective balloonborne sensor over the cloud particle size range with a polarization function, the more quantitative evaluations are required for detecting/measuring variety of atmospheric particles.

I am ready to recommend the publication after the authors revise the manuscript taking

into account the following comments, especially in terms of more supporting facts and figures which clearly indicate the measurement uncertainties and limitations. In my opinion, the manuscript will require a major revision for publication.

General comments:

Even if the main purpose of the paper is the demonstration of the new sensor, not quantitative discussions in detail, the basic performance of the sensor under the various atmospheric conditions is necessary since the CPS is a balloonborne sensor, rather than a ground-based instrument.

The manuscript is well written, however, I did not fully understand the strong points of this paper, compared to the current airborne instruments of cloud particles. There are many other airborne instruments for cloud and aerosol particle measurements with the polarization function: for instance, CASPOL (Baumgardner et al., 2001, 2011; Glen and Brooks, 2013; Nichman et al., 2016), SID (Hirst et al., 2001; Cotton et al., 2010), and CPSPD (Baumgardner et al., 2014). These instruments have a higher sensitivity with more detectors, and those should be referred in the paper at least. Also, a polarization optical particle counter has developed and demonstrated (Kobayashi et al., 2014), although it covers the different measurement size range (0.5 – 10 microns) and is the ground-based instrument. Therefore the comparison in laboratory experiments with other sensor or instrument is strongly recommended to exhibit the basic performance of polarization measurements.

If the CPS has an advantage of cloud phase determination in natural clouds, it remains ambiguous only from the frequency distribution of the degree of polarization (DOP). Thinking of the ice initiation from supercooled water clouds, not so many particles are detected as ice since the number of natural ice nuclei is generally scarce in the troposphere. So it is quite difficult to detect the signal of few ice crystals from the DOP frequency distribution. Even in the water clouds, there is certain amount of ranges of DOP, seeing from the Figs. 6a and 9a (I think that Fig. 4a must indicate the signals of aerosols, not water droplets. Please see the specific comment below.).

Or if the CPS has an advantage of counting of particles, the number concentrations in clouds, the sampling volume and counting efficiency are more thoroughly evaluated. To evaluate the sampling volume and its uncertainty during the ascent flight is essential to estimate the number concentration of particles. In the paper, only the rough estimate of detection area, ∼0.5 cm3, is written in the text. Only the fixed typical ascent rate, ∼5 m/s, is used, although it will be variable from the surface to upper levels even in the same case.

Is the real counting efficiency considered to be uniform inside the cross section of detection area (1cm x 1cm)? Even if the intensity profile of a laser device and the air flow inside the area are assumed to be uniform, in fact, the efficiency and uncertainty of counting particle might not be distributed with uniformity between in the center and near the edge of the detection area. Does the intensity and peak wavelength of the laser change during the ascent or under different ambient conditions? If so, does it influence the measurement accuracy at what amount of uncertainties?

Moreover, how are the capabilities of measuring coarse aerosols (for instance, dust or sea-salt particles) in real atmosphere? In other words, does the CPS have the capability to distinguish among water droplets, ice crystals, and various types of coarse aerosols over the micron-sized range?

Specific comments:

The following comments do not cover all general points above, but explain specifically as well as some minor points.

Page 3, Lines 19-20: Why the two photo detectors are placed at angles of 55 and 125 degrees? Do these set of angles have the best performance to distinguish between water droplets and ice crystals?

Page 3, Lines 29-31: As for the calibration or ground test before the launch, in what

ways is the sensitivity of both channels confirmed to be equal in terms of size measurements? In what ways is the calibration of polarization signal done quantitatively using the rough-surface particles? And the information on the rough-surface particles is required in the text (commercially available? mean size and its standard deviation? refractive index?).

Page 4, Lines 1-11: The paper describes the laboratory data focused on the relationship between I55 and the particle diameter. The performance of I125 is also needed to be examined to show the accuracy and uncertainty.

Page 6, Line 5: Using the hot-wire anemometer sensor data, the flow speed in the duct is compared with the balloon ascent rate. Thinking of the outlet area (3cm x 3cm) is wider than the inlet area (2cm x 2cm), what is the mechanism of significantly higher speed in the duct in some cases?

Page 6, Lines 11-18: The actual maximum number counted per second is thought be much smaller than the estimated maximum number (∼1000) from limitation of the interface board. What was the maximum number per second measured from the test flight data (in water clouds)? The upper limit of number concentrations in the CPS measurements can be much smaller than ∼2 cm-3? The typical number concentration of cloud droplets in natural clouds covers from tens to thousands of particles per cubic centimeter, so basically is it difficult to reliably measure the number concentrations of water clouds even with development of any correction algorithms?

Page 8, Lines 22-23: The manuscript states that there is a cloud layer from the surface to ∼2km. However it is considered more reasonable that the signal should indicate the layer of coarse aerosols (possibly in swollen state) since the RH is subsaturated with respect to water and it is inconceivable to have very low concentrations for cloud water droplets in the subsaturated layer. That is why I think it is quite difficult to determine the cloud phase only from the DOP frequency distribution.

Page 11, Lines 21-29: Although it might be impossible to compare the figures related to depolarization between the CPS and the remote-sensing measurements, is there any way to evaluate the DOP uncertainty? The comparison with other airborne sensors in laboratory experiments is useful to show the CPS performance for depolarization.

Pages 13-14 (Appendix A): The laboratory experiments are described using the standard spherical particles with different types and sizes. The description on how to enter the particles into the detection area is also required in the text. Is the air vacuumed from the bottom of the particle inlet duct at 5 m/s? How do the particles dispense before entering the particle inlet? Are there any calibration data to be described using non-spherical particles? That might be helpful to show the uncertainty of the CPS polarization measurement.

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