

Interactive comment on “In-situ cloud ground based measurements in Finnish sub-Arctic: Intercomparison of three cloud spectrometers” by Konstantinos-Matthaios Doulgeris et al.

Anonymous Referee #3

Received and published: 28 May 2020

The submitted manuscript serves as a record of activities involving three cloud spectrometers during the Pallas Cloud Experiment in 2013. Whilst the title suggests it is an intercomparison between these instruments, it is really a comparison of the rather specific experimental setups employed during this campaign. The paper therefore serves more as a campaign report than a reference on how best to set up experiments for ground-based in-situ cloud measurement. Nevertheless, it does highlight some of the pitfalls.

I am aware of the motivation within communities such as ACTRIS for the establishment of long-term ground-based in-situ cloud measurements, and as such this paper is a

C1

step in the right direction with respect to evaluating how these might be established. However, the conclusions do not seem robust enough to form the basis of wider recommendations. The paper does highlight the considerable difficulties faced by any attempt at long-term observations, and it is evident that any plans for unattended operation would pose particular challenges, especially in the sub-Arctic environment. In fairness, the authors are conservative in their recommendations and focus on Pallas campaigns (past analysis and future experiments).

I note that this experiment was performed contemporaneously with that at Puy-de-Dôme (Guyot et al, 2015) and hence the insight from the findings of the latter were not available to provide guidance on what complementary instruments, based on ensembles of particles, might be installed and used to explore scaling of the number concentration and related parameters. In fairness, the authors do recommend such instrumentation for future campaigns.

Guyot et al (2015) found that FSSP measurements suggested anisokinetic sampling and a high sensitivity to the wind speed and direction. It would be helpful if the authors could comment on how their findings relate to this earlier analysis.

The authors go into great detail regarding alignment relative to the wind direction, and the discussion is rather laboured and lengthy. The effects on number concentration are not particularly surprising, but are elaborated in great detail, no doubt because the specific instrumental setups (e.g. the brake on the FSSP) require it. This discussion may benefit from being shortened.

I note the authors specifically mention the frequent occurrence of supercooled clouds at this location. Do they have further corroborative evidence that the clouds being sampled contained only supercooled liquid water drops. Whilst LWC is readily calculated in terms of the measured parameters, it would be useful if the authors could comment on whether any data relate ice particles.

Whilst the manuscript appears to be in scope for the journal, I would recommend revi-

C2

sion before it could be considered for publication.

On a technical level, I believe the quantity pro_i defined on p.7 line 22 should be the reciprocal of that displayed. Also, the quantities b_{i+1} should, I believe, be b_{i^*+1} .

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-3, 2020.