Review of "Remote-sensing measurements during PaCE 2022 campaign" by Simo Tukiainen et al.

The present study discussed the important measurements over Kenttärova during the 9th PaCE campaign using cloud remote sensing instrumentation including two ceilometers, a Doppler cloud radar, and a Doppler wind lidar. The manuscript is well written discussing the campaign datasets, and methodology to estimate the cloud micro- and macrophysical properties briefly. The retrieved cloud products (ice water content, ice, and droplet effective radius) are beneficial to complement PaCE in-situ measurements. I believe this valuable dataset would also be helpful for the ongoing calibration and validation work for EarthCARE satellite data. In this context, the present study assumes its importance and thus I recommend the manuscript for publication. However, the authors missed some of the important details which I would suggest the authors implement in the revised manuscript.

Specific Comments:

1. The importance of the site: The author mentioned the importance of an intensive field campaign over the Arctic and Subarctic region in the introduction, but failed to connect it to their campaign site: Kenttärova. I understand the site is situated in a Subarctic region, but the site description lacks the importance of the site from the cloud remote sensing perspective. The author needs to mention why the location is strategic to establish a cloud remote sensing campaign over there. It can be done by a brief discussion of the cloud climatology over the site and the necessity of studying those clouds and their interaction with aerosols using cloud remote sensing data. If there is already a detailed description in other studies please mention the reference.

2. Quality control of any data set is very crucial. The cloudnetpy-qc software that the author mentioned, is a Python package implementing the Cloudnet processing scheme based on the study of Illingworth 2007. The author mentioned Doppler wind lider performed a background noise measurement once per hour, which was used to correct the measurements during data post-processing. Apart from this information, nothing is provided in the present study or the Documentation on the Cloudnet site. For example; do the authors use wind lidar or LDR from radar to screen out the insects? Therefore, I suggest the authors to be more elaborative about the criteria and methodology they have used for the quality control of each instrument. The authors can also add it as documentation in the Cloudnet Data Portal and mention the reference in the present study.

3. Further, the cloud classification methodology is adopted from Hogan and O'Connor (2006). Hogan and O'Connor were unable to distinguish supercooled drizzle from ice, but the present study shows the category of ‘ice and droplets’. Also, the target classes shown in the present study are different than Hogan and O'Connor's (2006). A systematic explanation is necessary on how the authors are achieving those 11 classes.

If the quality control and cloud classification are two general methods used in the Cloudnet site for the stations with Cloud Remote sensing facility, then adding the details as a document in the site would be useful for the readers of the present study as well as the Cloudnet users.

4. All the derived products depend on the radar reflectivity. Hence, the radar reflectivity offset calibration is essential. Since the 94 GHz radar is highly attenuated to rain, and hail, and the authors have no option to check the offset due to the lack of any Disdrometer observations, they need to keep that in mind while validating with in-situ observations, and also the data need to be ‘flagged’ during heavy rain condition. Further, for vertically looking radar, mis-pointing could be an issue and it can lead to high bias to Doppler Velocity. I suggest the author discuss how they took care of the mis-pointing calibration of the radar.

5. In regard to estimating droplet effective radius, Frisch et al. 2000 study is limited to stratus cloud only. Does the present study also focus on the stratus clouds while estimating droplet effective radius? If so, how the authors are classifying different types of clouds?

6. The goal of comparing cloud fractions with the ECMWF IFS model is not clear. The model having very low resolution clearly could not capture all the cloud features and hence nowhere it have any similarity with the observation in Figure 3. In spite, I would suggest the authors keep the estimated ice and liquid water content in the middle and the effective radius at the bottom panel in that figure for the same example.

Minor Comments:

1. Please mention if all the heights discussed in the present study are above mean sea level or ground level.

2. In lines 84-85, I understand by 'large atmospheric targets' the authors meant compared to wind lider, but W-band radar would be attenuated for light rain, so please modify the line as ‘making it sensitive to relatively large atmospheric targets such as ice particles, cloud droplets, and insects.’

4. In Figure 2. please mention in the caption, the time axis over which the parameters are averaged. Since the authors are showing data for 3 months, the radar reflectivity and also other parameters might be hourly or half-hourly averaged, so please mention that.

5. Modify the Figure 4 to include the y-tick label 100 %. Does the exclusion of 'clear sky' cause the first column below 100%?

6. What does it signify by ‘Melting & droplets’? Does it quantify the ice that is melting near the bright band? There is no occurrence of 'Melting & droplets' in Figure 4, is it because the radar is not attenuation corrected for the melting layer and hence the use of the melting layer to detect melting ice and droplets is not possible in this study?