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Discussions

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Interactive comment on "Improved Cloud Phase Determination of Low Level Liquid and Mixed Phase Clouds by Enhanced Polarimetric Lidar" by Robert A. Stillwell et al.

Anonymous Referee #1

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This paper describes a novel processing technique, using data from the CAPABL instrument at Summit, Greenland, which incorporates both the instruments' multiple polarization capabilities, as well as its photon counting and analog detection modes. Radiation, radar, and liquid water path measurements taken at the same site are used to validate this method.

This paper is well written; it provides an excellent background and theory of the measurements, as well as good description of the different instruments (Table 2 is very useful as well). It is clear that the authors are deeply familiar with this instrument and the portrayed methodology. The latter is new (to the extent of my knowledge), interest-

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Discussion paper

ing, and significant (to relevant lidar instruments).

However, there are several important issues that should be addressed. Therefore, I recommend this work for publication after the following issues will be treated:

Major comments:

-M1: Section 3.2: I understand that this is not a HSRL or a Raman lidar, where the lidar ratio can be extracted from the measurements. However, In Nott and Duck (2011) the variability of the lidar ratio is quite large, and is more towards 20 on average (see also Thorsen and Fu, 2015, DOI: 10.1175/JTECH-D-14-00178.1). There are several studies where lidar ratio of 10 was concluded (for certain aerosols), but these should be cited directly, and not through Nott and Duck (2011). This is a very delicate point, as the variability of the lidar ratio of different hydrometeors can significantly affect the backscatter ratio extraction, which is needed for the classification procedure. As the authors rely on this assumption for quantitative analysis, I wonder to what extent the results would change by using other lidar ratio values. Was there any other reason such a low lidar ratio was used (e.g., problematic resolved parameters, etc.)?

-M2: Section 3.3: Lidars operating at the same wavelength and probe the same air volumes should produce in theory the same output parameters. However, differences in the configuration and electronics, as well as in the data processing (e.g., in DABUL vs. CAPABL), all induce deviation in the resolved parameters, in particular in LDR values. In addition, fixed thresholds using only the LDR and backscatter values can be quite problematic in general for classification, as they don't take into consideration the atmospheric variability (I suggest looking at Figure 3 in Thorsen and Fu, 2015, and Figure 4 in Luke et al, 2010, doi:10.1029/2009JD012884), and the possible ambiguity of lidar returns (which is controlled to a certain degree using multiple polarizations). These effects are emphasized when utilizing a constant lidar ratio (which results in backscatter coefficient uncertainty, and hence, backscatter ratio uncertainty). Change of the LDR threshold to high values won't alter by much the FO as most lidar returns

with high backscatter values are located at low LDR (as seen in the figures mentioned above), so the description in p. 11, l. 23-29 is not surprising. The region where the sharp shifts in FO of liquid and ice at low LDR are relevant, and I think an analysis of this region, and how much changing the thresholds would affect the results, should be presented here (or in a supplementary material / Appendix).

-M3 Sections 3.4, 4 and 5: examination of Figures 1, 2 and 4 increase my concerns in M1 and M2, as the resolved (upper, at ∼1.5 km) liquid layers are much thinner than I would expect, when inspecting the LDR and relative backscatter (the signal becomes extinct due to the optically-thick liquid, which extends deeper than deduced). I suspect that this is a result of the constant lidar ratio, and consequently, the backscatter ratio threshold for liquid detection. Thus, based on this plot, the results seem to underesti-mate the liquid amount. This underestimation is not detected and remains "under the radar" in the validation section (e.g., figures 6 and 7), as columns are treated there (and not voxels), i.e., it is enough that a single ice/liquid voxel is kept after the filtering for the column to be treated as ice/liquid bearing. It might be relevant to these figures in cases of liquid containing air-volumes above intense precipitation, where the sig-nal is strongly (but not completely) attenuated, but these cases are relatively rare at Summit. In addition, the weak overlap between the analog and PC percentiles shown in Figure 3 makes me suspect that there is a "blind" zone around 1.5 km, where liq-uid voxels might saturate the PC while the signal having below threshold SNR (due to extinction/tenuous layer) in the analog.

-M4 Section 5: In continuation of the latter, missed detection analysis is needed in this paper as well, to confirm that hydrometeors of a certain type were not missed by CAPABL (as long as the signal is not extinct), thus strengthening the reliability of the study's methodology. E.g., CDF of periods when the MWR detected liquid above the uncertainty level but the CAPABL did not detect any liquid voxel (directly related to significant LWP levels missed in ∼10% of ROIC, HOIC, and clear, as stated in p.19 l.24-32), percentages of missed MMCR bins above SNR of -14 dB (i.e., hydrometeors,

most likely ice, after Shupe, 2013) which were missed by CAPABL.

-M5 Section 5: What I get from Table 5 is that the MPL is not a good instrument to validate the CAPABL retrievals, but to show CAPABL's superiority. The low percentages in boxes G and J, and the high ones in B and C, mean (as mentioned in the text) that it is not possible to genuinely compare the two instruments. This leaves the validation merely to the MWR and MMCR. I suppose that the delicate fixed liquid/ice determination thresholds have (a more significant) role in the MPL data analysis as well.

Minor comments:

-m1 p.9 l.25: Add 'volume pixel' in parentheses after 'voxel'.

-m2 p.14 l.9: Change 'is' to 'are'

-m3 p.15 l.0 and the entire paper: Please be consistent and use either 'Fig.' or 'Figure'.

-m4 p.16 l.27-28: Please provide a citation for this low LWP uncertainty.

-m5 p.19 l.17: Change 'has' to 'have'

-m6 p.20 l.2-3: Please provide a citation for this argument regarding the cirrus mode artifacts.

-m7 p.20 l.4-7: Could there be a height effect of CAPABL measurements as well (e.g., due to different operated modes, varying lidar returns' true lidar ratio below a certain altitude, which affect the integrated column above, etc.)? The two possibilities stated by the authors might be valid, but it will be suitable to mention them after comparison of both MMCR modes above 3 km will be performed.

-m8 Figure 1: Please consider changing the colorbar around 0 to grey, as it is impossible to distinguish between missing or "bad" lidar returns and values near zero. In addition, consider extending the scale of the relative backscatter panel, as it is impossible to separate the intense lower liquid layers' returns from the noisy background.

C4

I suggest adding (twice) daily sounding temperature profile to the plot, to enable the examination of the reliability of the HOIC classification given the temperature range.

-m9 Figure 4: Similarly, consider extending the scale of the relative backscatter panel, as it is impossible to separate the intense lower liquid layers' returns from the noisy background.

-m10 Figure 6: Please have a citation for the approximation given in the caption.