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Interactive comment on "Low-Level, Liquid-Only and Mixed-Phase Cloud Identification by Polarimetric Lidar" *by* Robert A. Stillwell et al.

Anonymous Referee #3

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This is a review of the manuscript "Low-Level, Liquid-Only and Mixed-Phase Cloud Identification by Polarimetric Lidar" submitted by Stillwell et al. to AMTD.

The paper explores different methods to measure lidar depolarization using the CA-PABL lidar. Specifically, uncertainties in the determination of thermodynamic phase of clouds in the Arctic caused by depolarization measurement errors are discussed.

This is a highly technical paper. Unfortunately, for a casual lidar data user as myself, the relevance of the work is difficult to assess. The paper shows differences in measured depolarization and cloud classifications when using different measurements strategies, but it does not provide an objective validation about which strategy is more accurate. It also does not adequately discuss the implication that this work might have on previous statistics of cloud phase using different lidars. Also, the importance of these errors

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Discussion paper

relative to other expected errors, especially those from multiple scattering, should be better discussed.

In its current form, I do not advice publication of the paper. With some major revisions, the paper might prove relevant for publication. Below I list my recommendations on how to improve the paper.

General comments:

1. The structure of the paper is not optimal which makes it difficult to follow. Much of this could be corrected by moving section 3 forward, as terms such as “analog detection” and “photon counting” are described here, but already used earlier in the paper. Also, multiple scattering is discussed on page 12 and 16, but I suggest to bring this forward to convince the reader that the analysis would still be relevant even if multiple scattering is considered.

2. Although the issue of multiple scattering is discussed, in my opinion it needs to be quantified more. How do the increases in depolariation due to multiple scattering relate to the biases discussed in the paper?

3. On page 3, line 2 it is stated about polarimetric lidar that “If not properly designed or considered, measurements can be misinterpreted casting doubt on critical measurements like cloud phase.” Are there references backing up this statement? Are there indications that phase is misinterpreted in previous studies because of design issues? The references on page 2, line 20 (and other papers) very consistently conclude that thin liquid clouds or mixed phase clouds, often with ice precipitation, are prevalent in the Arctic, at least in the summer (e.g., Intrieri et al., 2002). Thus, more discussion on previous studies is needed to place the current work into context.

I suggest to add a short review on measurements made by other lidar systems. The main question is how other lidars acquire depolarization? Is either “analog detection” and “photon counting” the norm? Are the errors shown here also expected for other

lidars? Are statistics from previous lidars biased and are they inconsistent because different lidar systems were used? What are typical dynamic ranges for lidars? Such a discussion would improve the relevance of this paper. This context should also be repeated in the conclusions and abstract.

4. Much of the analysis in section 2 is in terms of "count rates". Could you please relate/convert this quantity to any physical value that lidar users might be accustomed to? It is also confusing how count rate relates to voltages measured in the Analog measurements. I cannot judge whether large depolarization errors at a given high count rate of, e.g., 100 MHz, are of any concern. Count rate probably relates to backscatter coefficient with units of per meter per steradian, so, if possible, convert it to that quantity. In addition, please discuss in which situations 'high' count rates are to be expected. It is stated later that in regions of multiple scattering, low count rates are expected because of attenuation. However, it seems that high count rate implies dense clouds and hence substantial multiple scattering.

5. Statistics of CAPABL data using different processing approaches are shown in 5, 6, 7 and discussed in section 5. Obvious differences are seen between the results with different measurements techniques. However, there is no objective verification of which of the results is most realistic. For example, it is stated that "there is a dramatic underestimate of liquid water by CAPABL's PC acquisition, which worsens with decreasing altitude, shown in Fig. 5." (page 12, last line), but this implies that one of the other acquisition approaches is "correct". There might be a good reason for assuming the analog method is best here, but please explain this reasoning before presenting these results. Figure 6 then presents results in terms of cloud height statistics. Again, the differences are clear, but I do not see how these are validations of the methods. Both liquid and ice can be present below $\sim$4km, depending on temperatures (Intrieri et al, 2002), so all the statistics seem plausible to me. I suggest to remove this figure. Figure 7 suffers from the same problem. There are differences, but no indication of which result is most realistic. Please provide a more appropriate validation of the

measurements.

6. Section 2.2 offers a rather technical discussion on dynamic range. As an example, measurements in clear air are used. It is stated that" to measure clear air from 50m to 10 km would require no less than 6 orders of magnitude." These "clear air" measurements are of course very different form the targeted measurements in clouds. It is unclear how this discussion relates to measurement requirements and errors for cloud cases. Is this a best or worst case scenario? Elsewhere in the paper (page 17), optically thick clouds are referred to as "high dynamic range targets", so could we expect larger dynamic ranges?

Specific comments:

In equation 1, should the transmission not be a function of wavelength too?

Page 4, line 19: Please place equation 2 here in the text, embedded in a sentence. Please use this common style of equations embedded in sentences throughout the paper.

Page 4, line 20: Please define what is meant with "tilt angle". Also, I suggest to change "polarization" to "polarized signal" in this sentence (in 2 places).

Page 5, line 4: I am not sure what is meant with "which is not a function of range but only receive polarizations". Is there a part missing? Please correct.

Page 5, Eqs. 4, 5, 6 and 7. Please embed these in a sentence.

Page 6: I am puzzled what is meant with: "If one were to consider the differences in signal strength of a depolarization ratio of 1% vs 100%, there is substantial variations as well." Please rephrase.

Page 6 and other places: I suggest to add "of signal" after "orders of magnitude" everywhere in the paper.

Section 2.3: Please cite the paper by Gimmestad (Appl. Opt. 47, 3795-3802,2008) and

relate the definitions of depolarization used here to those discussed by Gimmestad.

Page 7, line 6: Here it is stated that “These are the theoretical arrival rates and not the observed arrival rates.” This is a confusing statement, since the ones described in Eq. 9 are still theoretical. Please rephrase this statement.

Page 7: Please define “non-paralyzable system”

Page 7, line 24: Please note in the paper that the threshold of 0.11 is very arbitrary. Theoretical values for most ice crystals are much higher (e.g., Noel et al, Appl. Opt., 41, 4245– 4257, 2002). Also, mixing of aerosols could lead to misclassifying ice cloud (precipitation) as liquid, as shown by Bourdages et al. (Atmos. Chem. Phys., 9, 6881–6897, 2009) and Van Diedenhoven et al. (J. Appl. Meteorol. Climatol., 50, 2184-2192, 2011). Values around 0.1-0.2 might be commonly used, but they are not very robust.

Page 8: last sentence: What are “noise mitigating discriminators”? Is there a reference for this?

Page 9, line 10: I assume the range should be 0.03-0.11.

Page 10, last sentence: Please define backscattering ratio. Is backscattering ratio also affected by the measurement method?

Page 11, line 3: The backscatter cut-offs (26, and 50) are mentioned here, but discussed in the next section. Please move the discussion upward. I suggest to rename the “sub-visual” class to “aerosols” as it is confusing later when it is stated that these do not contain any ice or liquid. In any case, the distinction between clear-sky and subvisual is not very clear and might be very arbitrary. I would suggest combining them.

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