1 General remarks

This is a review of the manuscript "Improved Cloud Phase Determination of Low Level Liquid and Mixed Phase Clouds by Enhanced Polarimetric Lidar" submitted by Robert A. Stillwell et al. to Atmospheric Measurement Techniques Discussions. This paper appears to be a revised resubmission of the original version with the title "Low-Level, Liquid-Only and Mixed-Phase Cloud Identification by Polarimetric Lidar", which was submitted by Robert A. Stillwell et al. to AMTD in 2016.

This manuscript delves further into the application of depolarization lidar meaasurements to retrieve cloud phase and cloud particle orientation in the Arctic. While the phase determination of Arctic low-level clouds are a challenging task, current phase statistics of these clouds still lack the accuracy to constraint the Arctic energy budget. Here, polarization-sensitive lidars can help to discriminate ice from liquid clouds since ice particles are depolarizing, while liquid particles are not.

To this end, the manuscript makes use of the depolarization measurement capability of the CAPABL lidar at Summit, Greenland. Specifically, uncertainties in the determination of thermodynamic phase of clouds in the Arctic caused by depolarization measurement errors are discussed.

The key innovations of this manuscript are focused on two methods to deal with the problem of detector saturation for polarization measurements using the CAPABL lidar. First, the generalization to non-orthogonal polarization channels within the approach described by Neely et al., 2013, who added a fourth polarization channel to detect saturation effects. Second, the comparison and the combination of photon counting and analog signals to enhance the dynamic range for cloud phase determination. Statistics conducted over several months show, that up to 30% of optically thick liquid or mixed-phase clouds can be misclassified when using only photon counting or analog signals.

The manuscript starts off rather technically, before it introduces the two new methods for cloud phase determination. The differences in cloud phase when using only photon counting or analog detection should definitely get the attention of a broader scientific audience.

In this regard, this work provides an important contribution to current and future research and is worth to be published as it helps to address the current bias in surface flux estimates in the Arctic caused by the undetected liquid phase of low-level boundary clouds. In my opinion, however, the manuscript lacks of five major issues which have to be addressed in detail before publishing the manuscript. Below, I compiled a list of comments which have to be considered in a revised version of the paper.

2 Major comments

1. **Structure of the manuscript**

   The manuscript is some times hard to read, since some sections are not well embedded into the overall structure. I found mainly two reasons for this:

   – First, section 2.1) "Polarization Measurements and Mueller Formalism" is very long and detailed while your objective (phase determination of Arctic low-level clouds) and your approach (combination of analog and digital detectors for cloud polarimetry) become stifled by too much formalism. This makes it hard for the reader to understand and appreciate the novelty of your approach. I agree, that polarimetric measurements and your non-orthogonal retrieval can not completely be discussed without an introduction to the Mueller Formalism. But considering the length of this section I would prefer an introduction to the problem caused by sensor saturation and into your approach to derive a cloud phase fractional occurrence by combining analog and digital detector measurements. While the introduction of a fourth polarization channel with opposite sensitivity to detector saturation was already explained in Neely et al., 2013, your work should focus more on your novel merged best estimate cloud product.

     There are various passages throughout the text, which should be moved (or even get their own subsection) in front of section 2.1) to motivate its lengthy formalism:

     – Page 7, Line 13-21

Observed depolarization ratios are a function of atmospheric scattering, optical system setup, and recording systems. Traditional two-channel polarization systems can not unambiguously measure atmospheric depolarization without additional information. Separating atmospheric depolarization from systematic effects is non-trivial. Alvarez et al. (2006) show, for example, how to calibrate differential detector sensitivity and receiver cross-talk, while Hayman and Thayer (2009) show how to remove depolarization effects caused by receiver optical retardance and scattering. However, recording systems that are subject to saturation, or underrepresentation of signal strength compared to incident irradiance, can also cause depolarization ratio effects, which are not constant in range and can not be calibrated using methods like that presented in Alvarez et al. (2006) or Hayman and Thayer (2009). Depolarization effects related to saturation couple polarization measurements with terms in the SVLE like cloud base height, range, and optical thickness through signal intensity measurements.

– Page 10, Line 18-23

By design, CAPABL uses 4 polarization channels to measure 3 elements of the scattering Mueller matrix: F11, F12, and F33 with one additional measurement to monitor saturation. If saturation is not an issue, any 3 of the 4 channels may be used for the inversion of polarization properties. Thus, the utility of the generalization given in Sect. 2 is that the 3 signals with the least error can be used at any time. For example, the 3 strongest signals for measurements of high ice clouds where backscattered signals are weaker or the 3 weakest measurements for low liquid clouds where the backscattered signal is stronger. Using non-orthogonal polarizations allows the dynamic range between polarization components to be accommodated and optimized.

– Page 11, Line 8-15 Covers data processing and not classification and therefore belongs into section 3.2) (somewhere around P9, Line 27)

- Page 15, Line 1,4 (“First, ...” [...] “Second, ...”) While this paragraph is a nice and comprehensible discussion of Figure 4, the two problems of photon counting and analog detection should be mentioned much earlier!
- Page 22, Line 21-23: This is a very important summary sentence which I would like to see already in the abstract!

- Second, I suggest to drastically reduce or move several equations and paragraphs from section 2.1) into the appendix. This was already suggested by a referee (RC2) for the original submitted manuscript (amt-2016-303), but not implemented by the authors. In my opinion, this section should briefly describe the “Generalization of Neely et al., 2013” to non-orthogonal polarization channels and should not discuss the Stokes vector lidar equation or its components. For this reason, I can only reiterate the comment already made by RC2 that the *few equations and definitions that could be considered relevant to the data analysis (e.g., Eqns. 7, 8, 9 and 10)* can remain, while Eqns. 1-6 and the corresponding text should be removed or moved to the appendix.

  Moreover, I would recommend to change some section headlines to improve the clarity of the manuscript:

  - Page 6, Line 13: New subsubsection 2.2.1) “Depolarization”
  - Page 7, Line 12: Subsection to subsubsection 2.2.2) “Diattenuation”

2. Lidar ratio

Section 3.2, Page 10, Line 16:

> *The inversion technique of Klett (1981) is used to calculate total scattering coefficient as described by Neely et al. (2013). A lidar ratio of 10 is assumed, following the review of Nott and Duck (2011) and references therein, to convert the total extinction derived by the Klett inversion to total backscattering coefficient.*

The constant lidar ratio of 10 used in the Klett inversion technique seems to be quite small when used for complete profiles. Theoretical and measured values for most ice crystals are much higher with average lidar ratios of around 22-28 (see e.g. Larroza et al., 2013, doi:10.5194/amt-6-3197-2013). Even liquid cloud droplets have an average lidar ratio of around 18 and rarely go below 10 (see e.g. Thorsen and Fu, 2015, doi:10.1175/JTECH-D-14-00178.1). This low lidar ratio should cause a low bias in estimated extinctions and therefore should lead to a low bias in backscatter ratios. This should, in particular, have an influence on the distribution of clear sky, sub-visible and aerosol voxels with backscatter ratio thresholds of 2.6 and 6.5. Could you elaborate your decision to use a lidar ratio of 10 and/or check if your results change for other lidar ratio values?

3. **Multiple scattering**

   In this resubmitted version of the manuscript, the issue of multiple scattering on depolarization measurements is not mentioned at all. While the authors made it clear in a previous answer that "the effect is consistent across all 3 data sets" and thus "is recognized for future work but not implemented in the masking scheme", the effects of multiple scattering cannot be left unmentioned! Multiple scattering is known to enhance depolarization ratios in liquid clouds, which can lead to a misclassification as ice clouds. With the different field of views of 100 μrad for MPL and 1.6 mrad for CAPABL I would expect to see differences in measured polarizatiion ratios in non-saturated signal regions. Can you quantify this difference and make an assessment if this affects your classification?

4. **Choice of diagrams**

   The author's choice of diagrams is sometimes not very helpful in guiding the reader to comprehends and verify the statements made. The following two figures should be reexamined:

   – **Figure 3:**

Statistics for liquid, ice and clear air voxels using the different detection techniques are illustrated in terms of the median and percentile detection height. While this plot shows significant differences in the median height of liquid voxels, no clear trend is visible for ice or clear voxels. I do not agree with the authors' statement that "As a result of the increased sensitivity, the median altitude of the clear-sky data shifts upwards". Detection sensitivity (is there a cloud?) and efficiency (how much of it was detected?) can and should be tested in comparison with different instruments (e.g. MPL and MMCR). Using the median detection height is only useful to explore the impact on cloud height statistics, not detection sensitivity. For this reason I would remove the sub-figures for ice and clear voxels and only focus on the misclassification of liquid clouds. In contrast, Figure 5 does a very good job to detect differences in fractional occurrence of liquid or ice clouds, which could be easily compared to different datasets (e.g. Mioche et al., 2015, doi:10.5194/acp-15-2445-2015).

– **Figure 7:**

Page 19, Line 19-21:

> *Note that though they contain and represent the same data, this work will choose to represent instrument comparisons in terms of their cumulative distribution functions as opposed to the probability density function. Both facilitate comparisons of large quantities of data but cumulative distribution functions allow simple comparisons of differences of shape and median whereas the probability density function allows for investigations of modes and biases.*

While this might me a minor issue, in my opinion, CDFs are harder to evaluate. While making differences more visible by using CDFs in Figure 7 might be acceptable, their significance is not discussed at all. In the end, Table 6 is much more valuable for comparisons with other studies. It would be nice to see PDF versions of Figure 6 and Figure 7 in your answer to decide if CDFs are really helpful here.

5. **Discussion of radiative impact**

My last objection is the way how the “Impact on Attribution of Cloud Effects on the Surface Radiation Budget” in Section 6.2) is discussed. While the observation that

Page 22, Line 32-33

> *The median value of downwelling LW radiation is higher for liquid clouds than it is for ice clouds, which is higher still than for clear air*

or

Page 22, Line 1

> *Likewise, the downwelling SW flux is highest for clear air and reduced for ice clouds, which is further reduced for liquid clouds.*

can be considered a scientific fact, a comparison of absolute values from the cited literature would be insightful. Moreover, it would be nice to use the measured absolute flux values from Table 6 when comparing to different studies like Miller et al. (2015).

3 Minor comments

- **P2, 22:** “... for example present in **the** Antarctic”
- **P2, 37:** Could you give an actual optical thickness range for *high*?
- **P3, 22:** “... using **a** co-located ...”
- **P5, Eq 7:** Your volume depolarization is also a function of θ_i, so $d(R, \theta_i) = \ldots$
- **P6, 9:** “If randomly orientated ice crystals (ROIC) are observed, diattenuation will be strictly $D = 0$ and the scattering Mueller matrix simplifies to a function of two elements,

depolarization d and the volume backscatter coefficient β." This is not obvious to a non-familiar reader and would need a citation or a descriptive explanation.

- **P7, 30:** Where is the problem, when the system dynamic range is in the "order of 4 to 5 orders of magnitude" and the two polarization signals can differ between "2 orders of magnitude" or "a factor of 2"?

- **P7, 20:** Incomprehensible sentence: "Depolarization effects related to saturation *couple* polarization measurements with terms in the SVLE like cloud base height, range, and optical thickness through signal intensity measurements." What do you mean with "couple"?

- **P3, 22:** "... or the system is insensitive to orientation (as is the case within a few degrees of zenith or nadir) ..." Why is this the case? Could you explain this statement in a little more detail?

- **P8, 30:** "... were accordingly changed ..." → "... were changed accordingly ..."

- **P9, 32:** "... from vertical." → "... from zenith."

- **P9, 11:** "this allows **8** methods to invert and solve Eq. 4." What do you mean by that? What/Are there consequences of having 8 different methods?

- **P9, 18:** "These scans are parsed by *like* polarizations ..." What are *like* polarizations?

- **P10, 3ff:** Could you introduce an explanation of the "opposite sensitivity" of D_1 and D_2 to saturation for the readers which are unfamiliar with these kind of measurements?

- **P11, 5:** "As a final check, data that is classified as clear air must have substantial signal ..." This is too vague, please give actual threshold.

- **P18, 14-19** Unfortunately, this paragraph is a little hard to follow but key to understand the differences between CAPABL and MPL. Could you rewrite it using an illustrative example?

- **P19, 14:** "... has been pushed ..." → "... has been interpolated ..." (please change this phrase throughout the text)
- **P19, 14:** "... are filtered conservatively" Please elaborate!
- **P21, 31ff:** Can you refer (Figure, Table) from where you take the given percentages?
- **P21, 18:** "An analysis of ground- and space-based observations of HOIC strongly indicates differences based on viewing orientation." Please give a reference.