Review of "Low-Level, Liquid-Only and Mixed-Phase Cloud Identification by Polarimetric Lidar" by Stillwell et al.

The manuscript discusses application of the CAPABL lidar at Summit, Greenland to retrieve cloud phase. Accurate identification of cloud phase is important for constraining the Arctic energy budget, but is a challenge for remote sensors. A polarization-sensitive lidar is able to discriminate ice from liquid cloud since ice particles are depolarizing, while liquid particles are not. However, it is well known that multiple scattering in optically thick liquid clouds can bias the measured depolarization ratio, making liquid clouds appear as if they were ice. Multiple scattering effects are not explored in this paper. Instead, this work focuses on two aspects of the CAPABL lidar (results succinctly summarized on Pg. 15, Ln. 22-28):

> 1) using an additional polarization channel at 45^{o} together with the traditional parallel (0^{o}) and perpendicular (90^{o}) channels to improve signal strength both in the clear air below cloud and in the region above cloud where the signal is weak due to significant attenuation
>
> 2) comparison of photon counting and analog signals for extending the dynamic range of the detector and for assessing similarities and differences in the cloud phase discrimination map (Figures 3 and 4).

Summary statistics for a 4-month period show that the altitude profiles for the analog and photon counting signals are similar for both ice and clear air pixels, but large differences are observed for liquid pixels (Figures 5 and 6).

The manuscript is quite long and full of technical details that do not seem necessary to understand the results and discussion, and hence detract from readability. This makes is hard to assess the novelty of the work, since much of the data processing and analysis seems to follow directly from Neely et al., 2013. Depolarization ratio is still the major discriminator between ice and liquid, and the threshold value of δ = 0.11 is unchanged from Intrieri et al., 2002. This is not helped by a weak conclusions section – the "three key points" are vague and do not offer the reader much guidance as to how this work would inform their efforts to use polarization-sensitive lidar to discriminate ice from liquid (as discussed below). In sum, while a revised version of this paper may be publishable in AMT, the current technical scope (focused on detectors and photon count rates) seems more appropriate for a more specialized optics journal. The authors would also need to address the novelty of the revised submission in the context of the prior instrument paper (Neely et al., 2013).

Specific Comments:

1. Section 2 (Polarization Theory) is superfluous and should be removed. The few equations and definitions that could be considered relevant to the data analysis (e.g., Eqns. 6, 7, 8, and maybe 9) can easily be rolled into Section 4 (Data Analysis and Cloud Phase Identification).

2. Throughout the paper, the authors repeatedly bring up interesting science questions that are important for the data analysis and interpretation, but then summarily dismiss these considerations as beyond the scope of this paper. This sort of writing is weak, and the paper (and its scientific contribution) would be made all the much better if the authors were to delve more deeply into these issues. Since, in my opinion, the current scope of the paper is not

necessarily worthy of publication, tackling some of these issues in a novel way would improve my review of the paper. Specific topics include:

a. **Constant bias in detector signal associated with multiple scattering, Pg. 12, Ln. 14-20:** "There are many techniques to deal with multiple scattering including multiple field of view lidar systems or post processing tools like those used by Shupe (2007), which reclassify shallow ice layers identified at the top of mixed-phase or liquid-only layers as mixed phase or liquid. For this analysis, multiple scattering clearly skews some of the interpretations towards ice but as the signals from A, PC, and SCPC are all subject to the exact same detector signals, the effect is consistent across all 3 data sets. This results in a constant bias for all three detection methods but as the purpose of this paper is to examine differences between the data sets, multiple scattering is recognized for future work but not implemented in the masking scheme."

b. **Optimum combination of orthogonal/non-orthogonal depolarization channels, Pg. 14, Ln., 30-32:** "The results of this work highlight the differences in signal dynamic range that propagate through the provided analysis altering the physical interpretation of the measurements made. While combining the measurements into the optimum combination of signals is beyond the scope of this work, it is useful to broadly understand the way to combine all the different signal approaches to utilize the available data to extend the work started with CAPABL to different lidar systems"

c. **Signal depolarization caused by multiple scattering of liquid droplets, Pg. 16, Ln. 23-32:** "One of the major topics to discuss is the handling of multiple scattering. Multiple scattering tends to increase signal strength but is important primarily within regions of high optical thickness. Even with scatterers that are purely spherical, multiple scattering can cause signal depolarization. In the CAPABL data set, this is most noticed in the middle and top of low-level liquid-only and mixed-phase clouds. The focus of this paper has been differences caused by count rate and signal strengths…The effect of multiple scattering is suggested for future work to further refine the measurement capabilities of CAPABL."

3. The discussion on "gluing" at the top of Pg. 15 seems unnecessary since this method is not actually applied in this paper.

4. The conclusions section summarizes the results of the manuscript in terms of "3 key points" that are demonstrated by this work:
    a. "cloud phase classification by polarimetric lidar is sensitive not only to the cloud phase but other cloud properties such as base height (or range) and optical depth, and to lidar design properties such as the power aperture product, field of view, receiver polarization and detection schemes."
    b. "this associated signal diversity in the lidar observations must be recognized in order to flag conditions unsuitable for determine cloud phase, an inherent problem in two-channel polarization lidars."

c. "by employing multiple planes of polarization in the lidar receiver, in the case of CAPABL four linear planes, the diversity in backscattered intensity may be handled more judiciously making the characterization of cloud types more accountable."

Regarding Pt. A, there is no discussion of how lidar design properties influence cloud phase classification in this manuscript. The conclusion is the first place that power aperture and field of view are mentioned in the manuscript. I don't follow how cloud base height influences the cloud phase classification – I would think that the signal attenuation and the range of the feature of interest would be much more important than if the cloud base is at, e.g., 500 m or 1000 m.

Regarding Pt. B, I'm not sure what this key point means, nor why two-channel polarization lidars are particularly problematic. Recognizing signal diversity in order to flag is not a particularly strong finding.

Regarding Pt. C, I don't know what this means either. What is meant by the phrases can be "handled more *judiciously*" or makes "the characterization of cloud types more *accountable*"? As in Pt. B, this is not an important finding.

5. On Pg. 17, Ln. 29-30, it is reported that the polarization configuration and signal combination allow the instrument to self-analyze limitations in a channels performance and correct some of the behavior. How is this self-analysis and correction done?

6. The recommendations for future analysis on Pg. 18, Ln. 10-13 sound great, and it's disappointing that none of these efforts were included in this paper. Are there other ancillary measurements of this kind at Summit that can be used to independently evaluate the lidar retrievals and assess the accuracy of the cloud phase discrimination? If so, I would strongly encourage the authors to incorporate such data into evaluating their lidar retrievals.

7. The terminology in Figures 3 and 4 is confusing and requires clarification on what exactly is being presented. I assume that "Total Backscatter" is really the "Total Attenuated Backscatter" or has an inversion been applied here beyond just adding the two channels to each other? Similarly, the label "Depolarization (F_{33})" seems inconsistent with *d* as in Eqn. 6, and the same inconsistency seems to apply for "Diattenuation (F_{12})" and *D* in Eqn. 7. It's unclear what is meant by Backscattering Ratio (e.g., ratio of backscatter coefficient to molecular scattering coefficient, or ratio of attenuated backscatter coefficient to molecular scattering coefficient) and how the inversion technique of Klett (1981) was applied here – does the inversion account for both particle and molecular attenuation or just the molecular? If particle attenuation is removed, then how was the inversion carried out (e.g., starting at high altitude or low altitude)? What lidar ratios were assumed? Last, it would be helpful to have the units for all of these graphs, and to report backscatter coefficient in terms of the more traditional km^{-1} sr^{-1} rather than photon count rate.

8. At the end of the day, what key finding or recommendation or technique is provided by this paper that allows someone like me to better employ a polarization-sensitive lidar to accurate determine cloud phase? How does the technique employed here compare to, or improve upon,

the cloud phase retrieval techniques employed by other polarization-sensitive lidars, e.g., the CALIOP lidar?

9. The author contributions statement on Pg. 19, Line 20 reads: "R. Stillwell prepared the manuscript with contributions from all co-authors." The brevity and lack of detail in this statement is completely unacceptable. Based on the acknowledgement of an NSF GRFP Fellowship, presumably the first author is a student so I would expect to see someone with the contribution of advising and supervising the research. Similarly, who took the data? Who maintained the instrument? Who analyzed the data? Why is this a 5-author paper?

Minor Comments:

1. In Figure 2, the y-axis is incorrectly labeled depolarization instead of depolarization ratio.
2. It's hard for me to interpret Figure 7 other than to note that PC seems to be seeing liquid clouds less often than the Analog, and SCPC is similar or in between. Which is correct?
3. Appendix A and Figure 8 are not meaningful. I suggest that this section be removed or moved to the Supplementary Material.