Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-327-RC3, 2017

Atmospheric Measurement Techniques Discussions

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 #3

Received and published: 21 November 2017

Fiedler and Baumgarten (2012) have shown that the importance of understanding the relationship between lidar sensitivity and findings from a time series, e.g. of cloud occurrence. The paper tries to shed new light on this topic. But to my point of view, the authors focus on the wrong issues. It is common knowledge in the lidar community that statistics from analog and photo counting signals are not comparable due to their different sensitivity and dynamic range. For a comprehensive analysis, both signals should be combined (i.e. glued) and not considered separately. In addition, special care needs to be taken to avoid signal saturation. This is usually done by either decreasing laser power or adding neutral density filters into the receiver setup to decrease the strength of the return signal. Saturated signals should not be used at all for data analysis. Fi-

nally, it is known that cloud statistics derived only from lidar measurements are biased as a result of signal attenuation by low clouds.

The authors describe that the lidar system they use has been updated from a purely photo counting system to a combined analog and photo counting system. These changes have an effect on the derived cloud phase classification: different statistics of cloud phase classification are found from measurements before and after the update. It would be good to see if these changes are significant. The authors introduce a new algorithm to combine analog and photo counting but the paper comes short in comparing the new algorithm to common methods (e.g. signal gluing). The paper requires some work and can be published after major revisions in AMT.

Major comment:

The comparison presented in Section 3.4 is rather pointless since it cannot be expected that the use of analog and photon counting signals with their different dynamic range will yield the same result. It would be of stronger scientific interest to show that measurement statistics from before and after the system update are consistent. For this comparison, only profiles with unsaturated signals should be used. Without a presentation of measured profiles, it is not possible to assess the reliability of the photo counting channel close to the ground. For example: in Figure 2 (photo counting) below 500 m the depolarization parameter (d) is larger than 0.4 and the backscatter ration (R) close to 0 while in the analog system d is around 0.1 and R around 5. Due to the colour scale is is really hard to give exact numbers but clearly the results are different. In both cases the observations are classified as clear sky but d suggests that in case of photo counting a small number of ice crystals are present compared to the analog signals. Isn't it more likely that the photo counting signal is saturated at lower altitudes. Further, looking at the analog signal close to the ground ($\sim$ below 100m) d is always larger than 0.5. Are these height bins trustworthy? What is the overlap of your system? How does the profile look like?

Section 4, page 4, line 11 to 16. I disagree with the statement that gluing is impractical. It is widely used in the community and does not need atmospheric calibration. Licel provides software for cluing analog and photo counting signals together with their transient recorders. Why do the authors not use this method? There should be no jumps between the analog and photon counting signals (Figure 4) when the gluing is done properly. Can you please show examples of glued profiles and compare the findings to you method? Further, detailed statistics should be provided between the standard gluing method and your new method. What is the effect of using different methods on the cloud phase classification?

Section 5. Comparison of CAPABL to the MPL. Can you assume that the cloud phase classification scheme can be used for the MPL as well? What is the error of d in the MPL? Since the MPL is photo counting only, what did the comparison between MPL and CAPABL look like before the update? Also, how did the CAPABL classification compare to the MWR and MMCR before the update? Are there significant differences to the findings with the new system?

Section 6.2. The authors need to show that the difference between merged signal and analog/photo counting is significant. Why do you use median values and not mean? Is there a big difference between both values?

Minor comment

- Due to the pure technical aspect of the paper I would recommend to omit the first paragraph of the introduction. The introduction should focus on lidar systems (i.e. how many system operate in analog and photo counting mode or only use one of these) and lidar analysis (i.e. what methods are used for phase classification – especially in connection to counting system). Also the statement on page 3, line 5 to 9, needs to be discussed in more detail and citation should be provided. - Page 6, line 16, please include e.g. before the citation - Page 7, line 10, How was the transmitter and receiver polarization purity measured? - Page19, line 5 to 8, since the LWP is a column

integrated value: Do you do the comparison when only one cloud layer is observed? What would you expect when you have e.g. a liquid cloud close to the ground and an ice cloud above? - Page 19 line 10, how do you assign LWP when you have more than one cloud layers present? - Page 21, line 18 to 24, HOIC observation are not shown in the paper and entire section should be omitted

Fiedler, J., Baumgarten, G., 2012. On the relationship between lidar sensitivity and tendencies of geophysical time series. In: Reviewed and Revised Papers at the 26th International Laser Radar Conference, Porto Heli, Greece, pp. 63–66.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-327, 2017.