

## ***Interactive comment on “Lidar measurements of thin laminations within Arctic clouds” by Emily M. McCullough et al.***

**Anonymous Referee #1**

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The submitted manuscript provides observational evidence that thin cloud layers may exist in the Arctic troposphere given certain atmospheric stability conditions. This manuscript demonstrates that high range resolution measurements are possibly needed to capture full cloud processes using lidar observations well in excess of previously considered range resolution. The manuscript is well written, well sourced, and provides, to my knowledge, previously unreported data of scientific interest. However, this manuscript is also speculative in nature providing few concrete explanations for observational phenomena. The conclusion calls for work that should, in my opinion be in this work and can not be removed from the scope. It is my recommendation that this manuscript should be accepted with major revisions (really major additions) with a more complete analysis of the critical data accumulated.

C1

Major Comments:

1) Figure 2: There appears to be very little attenuation of your light within this cloud. This raises concern for me about multiple scattering enhancing your signal. The Nott et al. 2012 paper described the field of view of the system at 0.3-2mrad. What were you running for this data collection? Is multiple scattering a concern? 3d multiple scattering effects could be very difficult to separate from physical structure and could add (or smooth out) structure on the order of a few range bins depending on the physical features of the cloud.

2) You say several times that taking data at lower resolution would cause the thin features to be covered (example on Page 2, Line 19-20). I am skeptical that this would completely remove some of the features you see, though I do not doubt it will change them. For example, the thick count layer at 3km from 4-5.5 UTC in Figure 1 would possibly remain. I believe you should show high vs. degraded resolution to better illustrate this point. Further, it will allow you to quantitatively assess, both what other investigators should be looking for in their lower resolution data and define to what extent data is masked. Specifically, it would help place your work in the context of the previous authors you describe on Page 2, Lines 28-33. Additionally, it will suggest how fruitful further analysis might be, combining data with the low-resolution lidar data products.

3) Why do you not apply overlap corrections? Showing data below 500 meters and not overlap correcting is confusing to me and a bit misleading in places. Suggest either applying the corrections or removing all data below full overlap for clarity.

4) Page 10, Lines 27-29 and Figure 3 and Figure 4: Depolarization contours are very noisy. I would argue they are almost unhelpful. In fact, given the results of the McCullough et al. (2018) paper, I am questioning if you have the sensitivity in the depolarization channel to make the described measurements at 1 minute resolution. At the very least, contours of depolarization error bounds should be shown to inform your reader how far they can trust the interpretation of depolarization.

C2

5) Section 4.3: Perhaps this is best used as an appendix. It is less convincing than the other 2 cases based on the level of information you are able to provide. It might be more helpful to summarize your measurements to describe the percent of time you see clouds with such vertical laminations.

6) Section 5.1-5.3: The discussion in these 3 sections is a major weakness of the paper in my opinion. I do not find the discussion particularly convincing because the topics discussed, while likely being familiar to a reader knowledgeable of lidar hardware, is not particularly well constructed in my opinion. My concerns are as follows: a) PMT or saturation more generally should serve to smooth your profiles in every case I can imagine. If the section of your glued profile originates from photon counting data, photons will be under-reported and thus thick clouds will seem thinner. If the portion of the profile is from the analog counting system and you are under reporting intensity (or even clipping the ADC), you are operating so far outside of the designed regime of the detectors that the data is likely not valid. Additionally, you claim to have corrected it in Section 3. b) Signal induced noise should be slow (microsecond time scale) and extensive in altitude. c) PMT ringing on the other hand is something I would think could cause vertical structure on the scale described. I would think this is the major instrument effect to investigate. d) I agree with your conclusion about laser power fluctuations. So much so that I would likely not even mention it in this analysis. e) I agree mostly with your timing electronics conclusions but if you have an issue, it might not be stable in altitude. If you have 2 or more different clock speeds (from triggering, seeding, q switching, or your counting system clock drifting slightly), you could possibly alias one rate onto the other making your observations move in altitude. That would likely be a systematic shift observable at all altitudes though, and as such easy to identify.

7) I am surprised that the authors have not included lidar data that could be very helpful. They do call for more analysis in the conclusion. That said, without this analysis, I am not convinced that this work is a major observational contribution. Some omissions that

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I believe should be seriously considered (at minimum) are: a) I find myself surprised that the authors use radiosondes and not rotational Raman measured temperatures and vibrational Raman measured water vapor. This is especially true of Figure 4 where the thermodynamic structure changes dramatically over the observation period. The data need not be at 1 min resolution to be helpful. b) I also find myself surprised that basic summary statistics of occurrence frequency or bounding relative humidity or temperature are not provided. At minimum, I would expect to see some observational bounds on conditions described in Section 5.4. c) I am not sure raw photon counts are sufficient to quantitatively show the structures within clouds. Calibrated backscatter coefficients would be much more useful. Additionally, they remove uncertainty sources such as laser power fluctuations.

Minor Comments:

1) Page 1, Line 4: It obviously depends on your target but 1 min time resolution might not be particularly high resolution. Suggest dropping the word "high" here. Also on Page 5, Line 10

2) Figure 2: At 3km, the range correction should be  $9 \times 10^6$ . The counts that you are showing are therefore on the order of 10-100. Is that correct? If so, counting statistics worry me. It is impossible to tell here what wiggles are due to scattering phenomena and what wiggles are due to counting statistics. Suggest adding error bars to clarify.

3) Page 3, Line 1-8: The following paper and references therein may be of interest to the authors as motivation for cloud structure size scales: Beals, et al., "Holographic measurements of inhomogeneous cloud mixing at the centimeter scale," Science 350, 87-90 (2015).

4) Page 4, Line 17: Referring to a broad class of elastic scatter lidars as Mie lidars is very imprecise. Suggest modifying to "elastic scatter" as you have no way of verifying that all scatterers are spheres.

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- 5) Page 6, Line 26: This sentence is confusing because your lidar counting system has already binned single photon data to 7.5 meters and 1 minute. Suggest modifying this sentence to something like: “No further binning was performed. ”
- 6) Figures 3 and 4: I believe there are several ways to calculate relative humidity with respect to ice. There are several parameterized versions or more simple versions. They do not all result in identical values given identical inputs. Suggest adding a citation to describe the method you use.
- 7) Figure 4 Caption: Suggest shortening by describing panels a-f as “same as Figure 3” or similar.
- 8) Page 9, Lines 27-29: Low depolarization is consistent with observations of preferentially oriented ice crystals. Suggest clarifying that high depolarization is “. . .inconsistent with interpretation as randomly oriented ice particles.” Note that the following might be of interest as well, especially Appendix A: Silber, et al., “Polar liquid cloud base detection algorithms for high spectral resolution or micropulse lidar data,” J. Geophys. Res.: Atmos. (2018).
- 9) Page 10, Line 15 and elsewhere: I find the use of numbers like  $1 \times 10^{10.5}$  to be difficult to interpret. Suggest changing to integer powers:  $1 \times 10^{10.5} = 3 \times 10^{10}$  or much less preferably changing to dB.
- 10) Page 11, Line 16-17 and throughout the manuscript: I assume your sondes are reporting their raw data with respect to water. Are you reporting all relative humidity values with respect to ice? It is clear in the figures but less so in the text. Suggest adding “w.r.t ice” or “w.r.t. water” throughout the text to clarify or inserting a blanket statement specifying how all data are reported.

Technical Corrections:

- 1) Page 10, Line 5: “. . .the air is remains. . .”
- 2) Page 10, Line 14 and elsewhere: “The clouds[,] which contain. . .” The use of the  
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word “which” requires use of a comma in most places.

- 3) Page 19, Line 18: I believe the paper you refer to here is in the January 2012 publication, not 2011.

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