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Interactive Comment

Interactive comment on "An improved algorithm for cloud base detection by ceilometer over the ice sheets" *by* K. Van Tricht et al.

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1 Response to Reviewer Comment of Referee 1

We would like to thank the reviewer for the careful evaluation of our work. Many useful concerns were risen. We address each of the specific suggestions and questions (*in cyan italic*) in this response document with reference to Section in the revised version in red and *cited text in the revised version in magenta italic*.

A summary of the proposed changes in the revised version of this paper can be found in the document 'AC C4546: 'General response to Referee Comments', Kristof Van Tricht, 01 Mar 2014', published in the Interactive Discussion of the manuscript.

Sincerely yours,
K. Van Tricht, I. Gorodetskaya, S. Lhermitte, D. Turner, J. Schween and N. van Lipzig

1.1 General comments

1) The manuscript states that major improvements over the standard internal operational cloud detection algorithms provided by the manufacturer have been displayed. In some senses this is true, however this particular criticism is unfair. Ceilometers are intended for determining the cloud base height of clouds that substantially impair visibility, primarily liquid clouds. Some major rewording of many paragraphs is required to reiterate that this manuscript is attempting to detect the base of clouds of a different nature to those that the instrument is typically used for.

R1.1: We agree that the original manuscript was suggesting comparisons in terms of better or worse performance between algorithms that have been designed for distinctly different purposes. We have carefully reformulated all such occurrences in the revised version of the manuscript as to stress that the PT algorithm has a different aim compared to the Vaisala and THT algorithms, leading to different results in terms of CBH statistics.

2) The results and conclusion should focus on the following: detection of optically-thin cloud is very dependent on SNR, and hence range. Any statistics on cloud base height are therefore range-dependent. Calculation of optical depth introduces additional uncertainty (state how much). Cloud cover statistics should then be presented in terms of SNR, height, optical depth thresholds (with uncertainties reported).

R1.2: Detection of optically thin clouds is indeed dependent on SNR and therefore range. To address this issue, we have clarified the method description and conducted extra analyses to show the sensitivity to all parameters used in the method (range, SNR, threshold, lidar ratio, averaging time):

1. We showed that our detection method is range-dependent (see R1.6).
2. We assessed the sensitivity of the results on the SNR threshold choice. All data were reprocessed with SNR thresholds ranging from 0.5 to 1.5. The uncertainty in the final results due to this factor is now provided in all relevant figures in the revised version (see major changes).
3. We have included an estimation of the optical depth uncertainty due to the most important assumption that we have made: the lidar ratio (see R1.7).
4. Finally, we also assessed the cloud cover statistics in terms of height and optical depth thresholds. However, since most detections of optically thin clouds at Summit and PE occur near the surface where the ceilometer is most sensitive, the final statistics were fairly insensitive to height. Nevertheless, the sensitivity of the ceilometer decreases with range and the detection of optically thin clouds will decrease accordingly with height. This is a limitation of the instrument and will be apparent in any method. The PT algorithm was designed to detect whatever

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cloud is detectable by the ceilometer, inherently taking into account that sensitivity falls of with range and this has also been explicitly mentioned in the revised version (new Section 5).

3) It is true that optically thin clouds are radiatively important. However, mere detection of all hydrometeors is not sufficient without additional information, such as optical depth thresholds together with instrument sensitivity. For example, using the PT algorithm on powerful lidar systems might return close to 100 % cloud cover at a detection threshold of optical depth > 0.01. *See AHSRL data from Eureka, Canada for example. Cloud cover by itself is not as important as the optical properties of that layer. In Polar night it will be the longwave radiative properties of most importance, while both longwave and shortwave are important in summer. Therefore, stating cloud cover with respect to some optical depth threshold (shortwave or longwave) is of prime importance.*

R1.3: The sensitive PT algorithm has been specifically designed for low-power ceilometers. We agree that this approach is not suited for cloud detection by more powerful lidars. We do not aim at the detection of clouds with a specific minimum optical depth. We rather aim at detecting all clouds, including optically thin hydrometeors, that are detectable by the ceilometer. We found a minimum optical depth of 0.01 that is related to very thin clouds near the surface, where the ceilometer is most sensitive. It is true that cloud cover estimates are only important when accompanied by their optical properties. In theory, the optical depth of all detected clouds could be estimated. However, this would introduce a very large uncertainty. Therefore, we do not provide cloud statistics exclusively in terms of optical depth, due to the uncertainty that is related with the procedure that we used. We rather report cloud statistics in terms of backscatter threshold, indicating the sensitivity of the PT algorithm.

4) Section 3: Should the backscatter threshold not depend on the background light? Especially in summer? Why not use the background value reported by Vaisala? The background light can be derived directly from this voltage value through an appropriate *scaling factor. It scales very well with the standard deviation of the attenuated backscatter signal in the noise for the gates at far range (assuming no cirrus is present). Then, it should be possible to recreate a reasonable proxy for the SNR value for each data point. This is necessary as the SNR varies with range. Note that an SNR value calculated from the ceilometer data in such a manner cannot be guaranteed to show range-squared dependence; this is due to the assumed overlap correction at close ranges. Overlap correction is calculated internally by the manufacturer, but for a generic instrument, not specifically for each instrument. The effect of polar temperatures on the optics is also not fully addressed. However, since full overlap is reached very quickly (certainly within a 100 m or so for both instruments) it is probably safe to assume these effects are negligible for the purposes of this manuscript.*

R1.4: We thank the reviewer for this valuable comment and we believe the proposed method to calculate SNR is in theory very promising. However, we have tried these suggestions and the results are problematic when applied to the polar atmosphere. We found that the background light in the polar air (even during polar day) is extremely small in clear sky conditions (with a maximum of 5 mV within the possible values of 0 - 2500 mV), related to the low solar zenith angle and therefore attenuation of solar radiation in the near-infrared. Moreover, the variation in this background light was small to almost non-existent, which makes it virtually impossible to derive noise levels. We therefore believe that in these particular polar conditions, using Eq. 1 is a reasonable way to estimate SNR. Since we agree that the proposed method by Referee 1 could have been a very good alternative, we have added the following information in the revised version:
This method is different from the common techniques used for lidars to estimate the ceilometer's noise level from the background light (see e.g. Heese et al., 2010; Stachlewska et al., 2012; Wiegner and Geiß, 2012). In theory, the background light, reported as voltages by the Vaisala ceilometers, could be used to derive a relationship with noise present in the data. In application to the polar atmosphere, however, this voltage is ex-

tremely small due to the low solar zenith angle and low scattering in clear polar air. Therefore we propose to work with the method as described in Eq. 1.

5) Equation 3.1: What happens if there are large fluctuations in hydrometeor concentration/size within a 10-minute interval? This can be very common in ice (especially ice fall streaks), so would this method incorrectly flag such periods as noise?

R1.5: Our experience is that in such cases the mean attenuated backscatter of those hydrometeors is larger than its standard deviation within 10 min, therefore having an SNR > 1, meaning that these periods will not be flagged as noise. However, this is a valid concern of the reviewer. We therefore estimated the effect on the results of the SNR threshold choice of 1 by varying it between 0.5 and 1.5, as described in R1.2. The overall results were fairly insensitive to this choice. We included the variation in the appropriate figures (Figures 4, 6, 10 and 11).

6) Section 3.3: The detection limit should be a function of range since it is dependent on SNR. Figures 6a and 6b merely show something about the detection limit of the ceilometer (although this should really be displayed in terms of range as well), not whether the atmospheric profile is clear or not. The sensitivity analysis appears to imply that, at PE, no more clouds are seen once the detection limit falls below about 100×10^{-4} km^{-1} sr^{-1}. *Although the instrument at PE is nominally slightly more sensitive due to higher average emitted power, the difference in range resolution (a factor of three) leads to a relative loss in sensitivity of the data at raw resolution. Maybe the true detection limit of this instrument is, on average over all heights, closer to* 100×10^{-4} km^{-1} sr^{-1}. *How would these curves look if plotted for each height?*

R1.6: The detection method of the PT algorithm should indeed be a function of range. We have addressed this issue in Section 3.2 in the revised version. However, Fig. 6 remains valid, since it merely serves as a sensitivity analysis to define the optimal fixed attenuated backscatter threshold near the surface that must exceed the background signal (we included the uncertainty due to SNR threshold choice). We believe that Figures 6a and 6b are not only showing the detection limits of the ceilometer. The clearest example for PE is Fig. 6a. Up to 3×10^{-4} km^{-1} sr^{-1}, every profile triggers the PT algorithm, regardless of the presence of clouds, although the instrument is able to measure up to a precision of 1×10^{-5} km^{-1} sr^{-1}. This means that such backscatter threshold is located below the background value near the surface. At 3×10^{-4} km^{-1} sr^{-1}, a sharp decrease by 50 % in number of detections is visible, indicating the value above which the threshold exceeds the background value and is able to distinguish optically thin clouds from clear sky. This method is specific for polar atmospheres, where clear sky ceilometer signal is negligible. We agree however that the flat parts of the curves in Fig. 6a (between 3 and 100×10^{-4} km^{-1} sr^{-1} could in reality be accompanied by an increasing amount of clouds with decreasing backscatter threshold, due to the occurrence of thin ice clouds high in the amospheric profile, that remain undetected by the PT algorithm. However, this is a limitation of the ceilometer that would occur with any method. We have acknowledged this limitation in the revised version in the new Section 5.

7) Section 4.3 The lidar ratios for spherical liquid droplets and ice particles are also wavelength-dependent. For example, the range of theoretical lidar ratios for spherical liquid droplets with diameters between 5-25 microns typical of liquid cloud droplets at 532 nm (lidar wavelength in Yorks et al., 2011 paper) are not quite the same as those for a ceilometer at 905 nm. Elsewhere in Yorks et al. (2011) the authors actually note mean lidar ratios of 20.41 sr and median lidar ratios of 17.29 sr (not 16 sr as in the conclusion), which are reasonably close to the range of theoretical lidar ratios values (17-20 sr) for spherical liquid droplets between 5-25 microns. The variability in theoretical lidar ratios for spherical liquid droplets between 5-25 microns at 905 nm is actually much smaller. Note that the observed lidar ratios in liquid were quite variable. The value of the lidar ratio in ice was not found to be constant, in fact it was found to have a wide variation, from about 8 to greater than 50 sr. Other studies show similar wide ranges in lidar ratio. This has important consequences for equation 4. Also note *that most of these studies were not performed at the ceilometer wavelength. If the lidar ratio varies considerably from case to case, then there will be a large uncertainty in the derived optical depth. This uncertainty should be stated since it is optical depth that it is important rather than just the presence of hydrometeors. Multiple scattering for optically-thin ice clouds can probably be neglected but multiple scattering cannot be neglected for liquid clouds. The assumed lidar ratio for liquid layers will vary with range for ceilometers due to their relatively wide lidar beam divergence and wide telescope field of view. Again, this will lead to uncertainty in the derived optical depth.*

R1.7: We fully agree with the reviewer that there is a large uncertainty in the lidar ratio. The main reason we are making this assumption is to be able to turn the lidar measurement (backscatter) into a more physical measure (extinction). We believe that including an optical depth estimate in the manuscript greatly improves our understanding of what the ceilometers are actually detecting. However, it is extremely difficult to report exact optical depths using a ceilometer due to the large inherent uncertainties (e.g., lidar ratio and correction for attenuation). Therefore we make a number of assumptions that should cover these uncertainties. We agree that an estimate of the uncertainty was lacking in the original manuscript. We have added this uncertainty in the revised version (Section 4.3, Page 18, line 25 - Page 19, line 2 and Figure 12) by testing a range of lidar ratios, which is the biggest contributor to uncertainty in our calculation of optical depth. We found that the overall uncertainty in optical depth is 25 %. Although this estimate does not include all possible sources of uncertainty, the final results reported in the manuscript are not impacted to a high degree by this approximation. We inserted this information into the revised manuscript as follows:
The assumptions for both the lidar ratio S and the derivation of the corrected backscatter from observed backscatter make the optical depth calculations prone to a considerable degree of uncertainty. Despite many assumptions simplifying a complex problem, this procedure allows us to make a rough estimation of the optical depth of hydrometeor layers detected by the PT algorithm. We assessed the degree of uncertainty due

to the lidar ratio approximation, by varying this ratio S between $16\,\mathrm{sr} < S < 25\,\mathrm{sr}$. The resulting optical depth uncertainty was 25 % which agrees well to similar studies with ceilometer by e.g. Wiegner and Geiß, 2012.

8) Page 9835, lines 15-18: There will be no liquid clouds below -40 C, so isn't this just due to temperature?

R1.8: It is true that at Summit during summertime, conditions are more favorable for the formation of supercooled liquid. One of the factors that plays a role is the higher temperature, but occurrence and maintenance of liquid depends also on moisture advection, ice nuclei and cloud condensation nuclei and in-cloud turbulence for temperature ranges between -40 and 0 °C. Overall, our detection of liquid-containing clouds agrees well with the results reported by Shupe et al. (2013), as stated in the manuscript (Page 20, line 14).

9) Page 9835, lines 7 - Page 9836 line 19: How much of this is due to SNR falling off with range? After accounting for SNR, what is the range dependence for minimum detectable cloud optical depth? I.e. do you expect to detect any optically thin clouds above 1 km?

R1.9: We do expect to detect optically thin clouds above 1 km, but only when they are persistent enough to survive the SNR noise reduction, because it is true that detecting thin clouds at higher heights is more complicated given the sensitivity of ceilometer. This is explained in the revised methodology Section 3.1, Section 3.2 and the new Section 5. In the latter, we have assessed the extinction profile corresponding to the sensivity of the PT algorithm. The new Figure 12 shows that indeed sensitivity decreases with height, inevitably leading to an increasing amount of optically thin clouds that remain undetected. This is a limitation of the ceilometer that would occur with any method. However, as the SNR threshold itself has only limited influence on the final detections (shaded areas in Figures 4, 6, 10 and 11), we believe that the final impact on our results due to this limitation is not significant.

1.2 Technical comments

Page 9820, line 4: Ceilometers are low-power backscatter lidars, not lidar-based.

R1.10: Our formulation was indeed confusing. We therefore changed it to low-power backscatter lidars.

Page 9820, lines 6-7: As noted in General Comments, standard ceilometer cloud-base algorithms were not expected to derive optically-thin ice clouds.

R1.11: As described in R1.1 and in the general comments, we have carefully modified any occurrence of unfair comparison to the original algorithms as, indeed, the aim of those algorithms is different from the purpose of this manuscript.

Page 9820, line 17: Assume you mean 'discriminate' here, rather than 'differentiate'.

R1.12: Agree, we have corrected this error.

Page 9821, line 1: Would be more appropriate to say 'from various hydrometeor' rather than 'about a wide range of hydrometeor' as it is not straightforward to discriminate between different hydrometeors, and a 'wide range' depends on your choice of classification

R1.13: We agree that our former formulation could be misinterpreted and we do not pretend to be able to discriminate between hydrometeor types. We have reformulated this part taking into account the suggestions by both reviewers as:
The results of this study highlight the potential of the PT algorithm to extract information in polar regions from various hydrometeor layers using measurements by the robust and relatively low-cost ceilometer instrument.

Page 9821, line 14: Precipitation from clouds may be important for surface mass balance

R1.14: We have reformulated this as:
Despite the great importance of clouds on the surface mass balance.

Page 9822, line 4-7: I.e. liquid clouds.

R1.15: We added the term 'liquid clouds' here for clarity.

Page 9822, line 1: Arguably, the standard algorithm is reporting the correct CBH, the liquid cloud base, as this is what is important for visibility, and especially for aircraft safety

R1.16: As described in R1.1, R1.11 and in the general comments, we have carefully modified any occurrence of unfair comparison to the original algorithms as, indeed, the aim of those algorithms is different from the purpose of this manuscript.

Page 9824, line 24: squared transmittance?

R1.17: Consistent with the literature, we have replaced this term by "two-way attenuation".

Page 9824, line 27: And a generic overlap correction - although this should be instrument-specific.

R1.18: In the revised version of the manuscript, we mentioned the generic overlap correction as an instrument-specific factor.

Page 9825, lines 20-23: This is not strictly true, as the detection limit as defined by SNR should be range-dependent. What happens if the calibration changes, laser power output declines etc..

R1.19: We apologize for the confusion this part of the manuscript has created and tried to reformulate it. Here we are not discussing the detection limit in terms of which atmospheric features are detected by the ceilometer. We rather want to stress what is the minimum attenuated backscatter value that is reported by the instrument. We have clarified this issue as follows in the revised version:
After calibration of the Summit ceilometer, the minimum reported attenuated backscatter value is $3 \times 10^{-4}\,\mathrm{km}^{-1}\,\mathrm{sr}^{-1}$*, while* $1 \times 10^{-5}\,\mathrm{km}^{-1}\,\mathrm{sr}^{-1}$ *is the minimum value reported by the PE ceilometer.*

Page 9826, lines 15-19: Does this definition include rain (freezing rain/drizzle)?

R1.20: Indeed, our definition includes freezing rain/drizzle, as these are also important for mass and energy balance. Because of the sensitive nature of the PT algorithm, cloud bases are reported near the surface in case of freezing rain/drizzle.

Page 9827, line 12: Replace 'considerate' with 'considerable'.

R1.21: We have corrected this typographical error.

Page 9837, line 6: Stating that the 'algorithms fail to report' is unfair, as they are expressly not expected to.

R1.23: As discussed earlier, we have reformulated all occurrences that implied an unfair comparison with the original algorithms.

Page 9837, line 14: Identify hydrometeor optical depths of 0.01 at what height?

R1.23: We agree that such a value should be accompanied by a height where such cloud occurs in the data. However, since we have estimated optical depths from the cloud base onwards and do not aim at detecting cloud tops, it is difficult to assess the correct height. We have addressed this issue by providing Figure 12, that gives an estimate of the extinction profile based on the range-dependent sensitivity of the

PT algorithm. A higher cloud must have a higher extinction value to be captured by the PT algorithm. Its optical depth must therefore be greater as well. Clouds with optical depths as low as 0.01 occur rather near the surface if they are detected by the ceilometer, as is now stated in the revised version (Page 23, lines 25-26).

Page 9837, line 22: How much of this finding is related to range-dependent SNR?

R1.24: For answering this question, we refer to R1.9.