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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.

K. Van Tricht et al.

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1 General response

First, we would like to thank both reviewers for their careful evaluation of our work. Many useful concerns were risen. We address each of these 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*. Moreover, if the reviewers accept our responses, we will provide a complete revised version that will include all proposed changes. Important changes in the revised version will be in cyan to clearly indicate where the manuscript has substantially changed.

1.1 Major changes

As a summary, the proposed major changes and the adaptations to the revised version of the manuscript include:

1. We have demonstrated that the Polar Threshold (PT) algorithm is range-dependent. At higher ranges in the ceilometer backscatter profiles, where noise levels are highest, cloud base height (CBH) detection is driven by the Signal-to-Noise Ratio (SNR) of the data, based on the method by Platt et al. (1994). Near the surface, where noise levels are low, the PT algorithm detection method is driven by a fixed attenuated backscatter threshold. The following changes can be expected in the revised version:
   (a) Section 3.2 now contains a clarified description of the PT algorithm, including uncertainties related to range and other factors.
   (b) We have added Figure 4, a conceptual and graphical explanation of the working of the PT algorithm.

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2. The noise reduction procedure that is based on SNR calculation is of utmost importance for the actual CBH detection by the PT algorithm. Choosing a SNR threshold ultimately determines the tradeoff between how much valid signal is filtered out (higher threshold) and how much noise is retained (lower threshold) in the final data subject to cloud detection. We have carefully tested the sensitivity of our results on this SNR threshold choice and we have included this information in all appropriate results in the revised version of the manuscript:

   (a) Section 3.1 and Section 3.2 have been updated to discuss the different SNR thresholds that have been tested.

   (b) Figures 4, 6, 10 and 11 now have shaded areas around the curves, indicating the sensitivity of the results due to different SNR thresholds choices.

3. The calculation of optical depth is based on a number of assumptions that introduce a considerable degree of uncertainty in the final result. The greatest uncertainty is due to the choice of a fixed lidar ratio S. We have tested a range of lidar ratios to assess the impact on the final optical depth estimations, being in the order of 25 %.

   (a) Section 4.3 now contains information about the uncertainty on the calculated optical depth values.

4. Several times, the manuscript suggested comparisons in terms of better or worse between the PT algorithm and the Vaisala and THT algorithms. The latter have been designed for different purposes compared to the PT algorithm. We carefully evaluated all paragraphs and reformulated any occurrence of comparison between algorithms.

5. The PT algorithm, specifically designed for ceilometer backscatter measurements, has its advantages as well as limitations. Increasing noise levels with height cause the sensitivity of the PT algorithm to decrease with height. This

inevitably leads to an increasing amount of optically thin clouds that remain undetected by the algorithm higher in the atmospheric column. In an attempt to quantify this limitation, we have calculated the extinction profile corresponding to the PT algorithm's sensitivity, providing an indication of the minimum extinction coefficient a cloud must have to be detected by the PT algorithm:

   (a) We have added Section 5 that describes the advantages and limitations of the PT algorithm as well as the procedure we have followed to calculate the extinction profile (including uncertainties).

   (b) The new Figure 12 shows the extinction profile for a typical daytime (higher noise levels) and nighttime (lower noise levels) case, together with the uncertainties due to the lidar ratio S.

6. Finally, we propose adding the term 'polar' to the title of the manuscript to stress that the PT algorithm has been designed specifically for these regions, characterized by clear polar air, low aerosol contents (also near the surface) and low background light. The title would thus become: *An improved algorithm for polar cloud base detection by ceilometer over the ice sheets*

Specific answers to the reviewer's questions are addressed in Sect. 2 and Sect. 3.

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

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2 Response to Reviewer Comment of Referee 1

2.1 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):

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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 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.*

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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

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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 extremely 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).

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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.

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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 ra-

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tio 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 $^{\circ}$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.

2.2 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.

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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

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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.

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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.

3 Response to Reviewer Comment of Referee 2

3.1 General comments

This is a very interesting work, aiming at a feasibility study of using a simple ceilometer to detect bottom height of lowermost optically-thin humid layers occuring in the polar regions. It is based on a development of an algorithm optimized for this purpose, which is successfully applied to measurements taken by two types of Vaisala ceilometers. A statistical study of the thin polar cloud layers is also performed and it shows significant diferences in therms of their occurence and optical depths at two stations in Arctic and Antarctic. I reckon this paper is worth publishing in the AMT, although it needs a minor revision beforehand. The possible improvements are suggested in the supplement.

We thank the reviewer for the detailed review. Below we address the questions that were raised in the review.

3.2 Questions

1) Why have thin ice clouds no effects on precipitation which is not reaching the surface?

R2.1: We apologize for the misleading formulation. While we wanted to stress the importance of precipitation for the surface mass balance, precipication not reaching the surface is of course also important in terms of surface energy balance. We have removed "not reaching the surface" in the revised version to include both the importance on mass and energy budget.

2) Standard algorithms by their definition do not aim at all at a detection of thin clouds,

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regardless of whether the thin clouds are important or not.

R2.2: We strongly agree with this point as also brought up by Referee 1 and we clarified every occurrence of comparison between the PT algorithm and the conventional algorithms.

3) "This paper presents the Polar Threshold (PT) algorithm that was developed to detect optically thin hydrometeor layers (optical depth $\tau \geq 0.01$)." At what range of cloud thickness? At what range of cloud altitudes?

R2.3: 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). This answer corresponds to a similar concern by Referee 1 (R1.23).

4) What are the temporal and horizontal resolutions of cloudy conditions?

R2.4: Since we are detecting clouds in separate profiles, the temporal resolution is dependent on the final running mean averaging that produces the final profiles. Therefore a cloud has to be persistent for at least 2.5 minutes to be picked up by the PT algorithm (if not removed by the SNR noise reduction method). This is also mentioned at Page 10, lines 20-22: *For the final analyses, the noise-reduced data were smoothed by applying a running mean over an interval of 2.5 min, determining the final temporal resolution of the data*.

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5) A minimum cloud thickness of 50 m is a very thin cloud. Are you sure that by applied averaging you are able to detect statistically significant amounts of 50 m thin clouds?

R2.5: We agree with this comment and increased the minimum thickness a cloud layer must have to 90 m. This implies 9 range bins for PE and 3 range bins for Summit.

6) "The occurrence of optically thick layers, indicating the presence of supercooled liquid." Particles and/or droplets?

R2.6: We are referring to supercooled liquid droplets in the cloud. This is now being explicitly mentioned several times in the revised manuscript (e.g. Abstract, line 21: *The occurrence of optically thick layers, indicating the presence of supercooled liquid water droplets, shows a seasonal cycle at Summit with a monthly mean summer peak of 40 % ($\pm 4\,\%$).*

7) "The results of this study highlight the potential of the PT algorithm to extract information in polar regions about a wide range of hydrometeor types." Range of types? Or range of sizes? Anyway, you are not able to distingwith type nor size from ceilometer data, are you?

R2.7: As discussed in R1.13, we agree that our formulation was confusing. In the revised version of the manuscript, we have adapted this to a more precise formulation.

8) "Ceilometers typically detect cloud bases at a distinct height and increasing backscatter (see e.g. Fig. 1)." What do you mean? Where is it in Fig.1? At what height range?

R2.8: We agree this formulation is confusing for the reader. We adapted this in the revised version as follows:
Ceilometers typically detect cloud bases in regions with high backscatter (see e.g. Fig. 1), that are likely related to liquid-containing portions in case of a mixed-phase cloud.

9) The fact that you need a person to operate the system on site? Isn't it an important limitation for polar applications?

R2.9: We agree with the reviewer and have added this in the manuscript (Page 5, lines 23-24: *and the need for a manned station to operate such systems on site*.)

10) "including the detection of very optically thin hydrometeor features" I feel it is not clear what you mean by this: hydrometeor = liquid particles and/or ice particles?, feature = layer?

R2.10: We mean to detect hydrometeor layers and have adapted this as such in the manuscript (Page 5, line 25): *including the detection of very optically thin ice layers*.

11) "Since the transmittance of the atmosphere is in general unknown, conversion of attenuated backscatter β_{att} to corrected backscatter β is not straightforward." I am not sure what you trying to say. Are you converting β_{att} to β_{att} corrected, which is not equal to β true?

R2.11: We apologize for the confusion in the definition of β_{att}, $\beta_{corrected}$ and β_{true}. β_{att} is the value that is reported by the ceilometer. It is the true backscatter coefficient β_{true} that has been subject to attenuation in the atmosphere. The true backscatter coefficient β_{true} therefore is the attenuated backscatter coefficient β_{att} corrected for attenuation. To avoid any confusion, we have replaced every occurrence of "corrected backscatter" by "true backscatter" in the revised version.

12) "The vertical resolution is 30 m for the CT25K." This was my concern for the abstract, can you claim to be able to detect 50 m thin layer when you average over 30 m? I am sceptic here.

R2.12: As discussed in R2.5, we agree with this comment and increased the minimum geometrical thickness of a hydrometeor layer to 90 m.

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13) For answering the question of multiple scattering effects you should take to account not only the laser beam divergence but also the size of the field of view of the two instruments. And you did not specify this number for neither of the two in struments. I reckon, you should give a comment on that.

R2.13: We have included this information in Table 1 in the revised version. The field-of-view of both instruments is relatively small, justifying the approximation of no multiple scattering that we have made (Page 8, lines 2-3: *Due to the low beam divergence and small field-of-view of the CT25K ceilometer (Table 1), the effect of multiple scattering is small*).

14) "This includes ice particles and supercooled liquid droplets as well as any form of precipitation." Why? Precipitation is NOT a cloud? Is your algorithm counting it as a cloud? Please clarify this.

R2.14: As related to R1.2, the PT algorithm is indeed triggered by precipitation. While the conventional algorithms try to locate the source cloud of the precipitation, the PT algorithm will place the base height of the hydrometeor layer at the bottom of the detected precipitation. A motivation for this is now given in the revised manuscript under Section 3 as follows:

We do not attempt to distinguish between clouds and precipitation, since our broad definition of a cloud and its importance for the energy and mass budget includes precipitation as well. This is different from the conventional algorithms that try to identify the base of the cloud above the precipitation layer given that the latter does not entirely attenuate the signal.

15) "As the physical variability of the background signal obtained for clear polar air is low." I assume this physical, atmospheric variability is what you are talking about, because of course the variability in the polar clear air signal is very high due to the high noise and measurement at a detection limit.

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R2.15: We agree and indeed talk about physical, atmospheric variability. As now further clarified in Section 3, the background signal in clear polar air is very low. Because of this, the variability in this background signal is very low as well. The approach by Platt et al. (1994) can therefore not be used as such.

16) The range correction does not worsen the signal, as after the range correction some features in the signal can be seen more clearly. However, it increases the noise level in the signal.

R2.16: We agree with the reviewer and reformulated the sentence as:
The fast decrease of signal with range and its range correction (evident from the lidar equation in e.g. Munkel et al., 2006) leads to increasing noise levels higher in the profile.

17) Please rewrite Eq. 1

R2.17: We agree that Eq. 1 should be formulated in a more mathematical way. We propose the following changes in the revised version:

The SNR was calculated for every separate height range bin at time step i and range bin j as:

$$\mathit{SNR}_{i,j} = \frac{\overline{\beta}_{i,j}}{\sqrt{\frac{1}{2M}\sum_{k=-M}^{+M}\left(\beta_{i+k,j}-\overline{\beta}_{i,j}\right)^2}},$$

which is the ratio of the temporal mean $\overline{\beta}_{i,j}$ and standard deviation of the attenuated backscatter over $\pm M$ time steps around time step i and range bin j. Provided that the temporal resolution of the individual profiles is 15 s, M is equal to 20 profiles for a time interval of 10 min.

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18) "The atmospheric fluctuations in this interval are small compared to the instrument noise such that the standard deviation over the interval mainly contains internal noise from the instrument." That should depend on height you are taking to account. I would say, at low altitudes the atmospheric variability is much higher than the noise, isn't it?

R2.18: Indeed, at low altitudes the atmospheric variability is much higher than the noise, whereas at high altitudes it will be the opposite (i.e., noise > atmospheric variability). However, as discussed in R1.5, our experience is that this is not a problem in practice. If the atmospheric variability near the surface is high, this is related to hydrometeors. As a consequence, the mean backscatter will be high as well, meaning that these periods are not flagged as noise in this procedure.

19) "The SNR threshold was set to 1 as was also done by Heese et al. (2010), and pixels with a lower SNR were removed." Does that mean that if you obtain low, positive mean backscatter from this averaging you must still remove it?

R2.19: Related to the previous question, this will depend on the standard deviation in the 10 min. Low, positive mean backscatter will be removed if it is lower than the standard deviation (when SNR threshold is 1), whereas it will be retained when it is higher. Consequently, the removal (or not) depends more on the persistency of the signal compared to noise than on the value of the backscatter. As also discussed in R1.2, we have assessed the impact of our approach on the results by allowing the SNR threshold to vary between 0.5 and 1.5, of which the results are indicated by the shaded areas in Figures 4, 6, 10 and 11.

20) "In a second step, the noise-reduced data were smoothed by applying a running mean over an interval of 2.5 min." So for SNR calculation and pixel removal you average over 10 min and then what is left over you average to final profile of 2.5 min? I feel this may be not clear enough.

R2.20: The reviewer has correctly interpreted what we mean in the text. To avoid possible confusion, we have further clarified it as: *For the final analyses, the noise-reduced data were smoothed by applying a running mean over an interval of 2.5 min, determining the final temporal resolution of the data.*

21) "The PT algorithm processes every vertical profile." You mean every 2,5 min avarage profile ?

R2.21: The algorithm processes indeed the final 2.5 min averaged profiles. We clarified this in the revised version (Page 12, line 21: *The PT algorithm processes every vertical 2.5 min averaged and SNR-processed profile separately*).

22) "The CBH detection is triggered if the attenuated backscatter at a certain height in the vertical profile exceeds the threshold." Maybe you could give the theshold precisely also here?

R2.22: The actual backscatter threshold that is used by the algorithm is determined only in Section 3.3. We therefore think that it would be inappropriate to mention this threshold already in the general method.

"After the trigger, the algorithm also considers the mean attenuated backscatter 50 m above the trigger point (60 m for the Summit ceilometer). If the backscatter value at this elevated height also exceeds the threshold, the height of the trigger point is set as the CBH. This ensures a certain amount of robustness of the signal at the detected CBH, meaning that a hydrometeor layer should have a minimum geometrical thickness to be detectable by the algorithm." Why not checking all points at that range? Then you could, at least for the PE station, try to see thinner than 50 m layers?

R2.23: The formulation as it was in the original manuscript caused some confusion. The algorithm does not only look at one range bin a certain altitude above the trigger point, but also at all range bins in between. Moreover, as described in R2.5, we increased the minimum thickness of a cloud to 90 m, instead of the original 50 m. Al-

though in theory a geometrically thinner clouds should be detectable by at least the PE ceilometer, we chose for consistency between the different ceilometers, thereby also decreasing the chance of false triggers. For clarification, we therefore propose following changes in the revised version of the manuscript:
After the trigger, the algorithm also considers the mean attenuated backscatter over the minimum cloud thickness distance (set to be 90 m for both systems) above the trigger point. If the backscatter value over this elevated height also exceeds the threshold, the height of the trigger point is set as the CBH. This ensures a certain amount of robustness of the signal at the detected CBH, meaning that a hydrometeor layer should have a minimum geometrical thickness of 90 m to be detectable by the algorithm.

24) "This approach was found to perform best in identifying the base of optically thin hydrometeor layers compared to other algorithms." The other algorithms were not designed for the same purpose, they are not even comparable. I reckon the beauty of your approach is that you did optimize it for the detection of thin polar clouds, and that is the only one at the moment for serving this purpose. This comparison is not appropriate.

R2.24: We agree with the reviewer and refer to R2.2.

25) "The optimal threshold is one that allows the detection of hydrometeor layers with a low optical depth while not triggering the algorithm in clear sky conditions." Well, it may be also difficult to distinguish it from the aerosol layers present within the boundary layer, or you do not detect any aerosol?

R2.25: As we describe in Section 4.3, we acknowledge that such sensitive algorithm will inevitably be triggered sometimes in case of elevated aerosol layers:
The drawback of the high sensitivity of the algorithm (detection of features with $\tau = 0.01$) is that CBH detection can sometimes be triggered by layers of elevated aerosol contents. This only rarely happens over the Antarctic ice sheet due to its remote location and clean air (e.g., Hov et al., 2007). This is not the case for Greenland,

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which is much closer to industrialized countries. In the events of elevated aerosol contents, some aerosol layers will inherently be identified falsely as cloud (Shupe et al., 2011), an issue that occurs in other parts of the Arctic as well, for instance in Svalbard (Lampert et al., 2012).

26) "For example, increasing the threshold from $3 \times 10^{-4}\,\mathrm{km}^{-1}\,\mathrm{sr}^{-1}$ to $30 \times 10^{-4}\,\mathrm{km}^{-1}\,\mathrm{sr}^{-1}$ at Summit decreases the amount of detections by 10 % and increases the mean CBH by 70 m, while at PE the amount of detections is decreased by only 2 %, though the mean CBH increases by 190 m." Does that mean that at PE there are more optically thin clouds which are span over the larger altitude range in the troposphere? Is that what you expect? Or is that an artifact due to e.g. different height resolution of PS and Summit instruments?

R2.26: Our results indicate that there are more optically thin clouds at Summit that are no longer detected when you slightly increase the threshold, whereas these clouds are relatively thicker at PE. However, the altitude range at which they occur is less variable at Summit compared to PE. We believe this is not related to artifacts but represents true results. Nevertheless, we report these numbers primarily to motivate our backscatter threshold choice.

27) "The Summit ceilometer data in Fig. 7b indicate that precipitation reaches the surface after 14 h. Since the first two range bins of the profile were excluded, the CBH is located at 60 m in such conditions." But in the case when there is precipitation, than the CBH is at the level of clouds, isn't it? How does your algorithm deal with that?

R2.27: For addressing this question, we refer to R2.14. The high sensitivity of the PT algorithm triggers cloud base detection at the first detectable concentration of hydrometeors in the profile. Since precipitation is equally important for surface mass and energy budget, while it is not feasible to try to locate the base of the source cloud with the PT algorithm, CBH is reported at the base of the precipitation layer in such events.

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28) In both cases, the PT CBH is significantly lower compared to the Vaisala and THT CBH. At both study sites, the Vaisala CBH is situated much higher." Well, not always, e.g. Fig. 7a at about 4-6 UTC it is not? "In the case of optically thin features with only low backscatter values, Vaisala sometimes reports the profile as being clear sky." You mean e.g. Fig.7a from 0-4 UTC?

R2.28: We agree that Vaisala does not always report the CBH much higher, only most of the time. Moreover, the clearest example of clear sky reports from Vaisala, while PT detects a cloud, is Fig. 7b from 0-12 UTC. We propose clarification in the text as follows:
In both cases, the mean PT CBH is significantly lower compared to the Vaisala and THT CBH. At both study sites, the Vaisala CBH is mostly situated much higher in the cloud, where backscatter values are peaking. This is to be expected since the primary goal of the Vaisala algorithm is to detect visibility changes for pilots. In the case of optically thin features with only low backscatter values, Vaisala sometimes reports the profile as being clear sky (e.g. Fig. 7b from 0-12 UTC).

29) Atmospheric sounding by radiosondes has been used in the past for cloud detection validation in polar regions, where higher values of RH are associated with clouds (Gettelman et al., 2006; Minnis et al., 2005; Tapakis and Charalambides, 2012). The RH at the level of the detected CBH should in general be high, assuming the actual presence of hydrometeors at this height. An example case with ceilometer attenuated backscatter measurements and the radiosonde-derived RHice is shown in Fig. 8, which shows that the RHice increases significantly at the cloud base." Higher meaning what % of RH? Please give an exact definition because not always higher RH is a clear indication of cloud. Significantly meaning of how much? RH beyond 100 % ? And at what range? Heights between 0,5-1,4 km? Where in this range is the CBH in your opinion and what is found by the algorithm? Note, that also at about 2 km there is a significant increase of RH but in Ceilometer data I see no clouds.

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R2.29: We agree that our formulation was unclear. We have reformulated this section and we added extra information for clarification:
Atmospheric sounding by radiosondes has been used in the past for cloud detection validation in polar regions, where clouds are in general characterized by high RH_{ice} *values (Gettelman et al., 2006; Minnis et al., 2005; Tapakis and Charalambides, 2012). Since our primary goal is the detection of optically thin ice clouds, cloud bases will not always be characterized by significant ice-supersaturations, as is the case in the liquid-containing portion of the clouds. Hence, we do not apply radiosonde cloud detection methods such as proposed by Jin et al (2007). Instead, radiosonde-derived statistical* RH_{ice} *distributions are used to assess the performance of the PT algorithm. The* RH_{ice} *at the level of the detected CBH should in general be high, assuming the actual presence of hydrometeors at this height, while this is not necessarily the case for clear-sky* RH_{ice}*. Statistically, clear-sky* RH_{ice} *values should therefore be lower than cloudy* RH_{ice} *values. An example case with ceilometer attenuated backscatter measurements and the radiosonde-derived* RH_{ice} *is shown in Fig. 8. Visual cloud base determination based on our definition of a cloud indicates a CBH around 500* m*. The radiosonde data show that the* RH_{ice} *increases significantly (by 45 % at this cloud base, although its absolute value does not indicate ice-supersaturation).*

30) "The test indicates that the cloud base RHice values are indeed significantly higher than the clear sky RHice values (p *value* < 0.01)*, suggesting that the PT algorithm performs well. Could you explain, I see no connection.*

R2.30: We have further explained what we mean in the revised version:
We used a one-sided nonparametric two-sample Kolmogorov–Smirnov test to determine if the RH_{ice} *measurements of cloud bases were significantly higher compared to clear sky* RH_{ice} *values (Hájek et al., 1967). The test indicates that the cloud base* RH_{ice} *values are indeed significantly higher than the clear sky* RH_{ice} *values* (p *value* < 0.01)*. If the PT algorithm would often be triggered in clear sky, both distributions would not statistically differ significantly which suggests that the PT algorithm performs*

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well.

31) There is a paper by Lampert et al., 2012 about the humid layers detection at Svalbard. I am wondering how much your results at Summit compare with their paper.

R2.31: We thank the reviewer for mentioning this study. We have read this paper with interest and included the reference at the point where we mention episodes of elevated aerosol layers at Summit (Page 19, lines 11-12). The sensitive PT algorithm could be triggered in such events. However, comparison of the results by Lampert et al, 2012 with the results we found at Summit is not straightforward. The climatology of Ny-Alesund is very different from Summit, being much more maritime. Since it is extremely difficult to assess the performance of our work at a maritime site, direct comparison is outside the scope of our study.

32) "As our measurements include a variety of atmospheric conditions from ice to supercooled liquid, we assume an average ratio of S = 20 sr for a rough estimation of the extinction coefficient." Yes, this is very rough assumption and you do not show that you actually can make that assumption. Thus, I think it would be very helpfull if you made an example calculation/estimation of what range of results you would get if you would use the 16 sr and 25 sr, just to give a reader the feeling of how much you risk by taking this average. It will be also a kind of error estimate due to this assumption.

R2.32: We agree with the reviewer that an uncertainty estimate was lacking. We have conducted the analyses with the lower and upper estimates of the lidar ratio (16 sr and 25 sr) and we found an uncertainty on the derived optical depths of 25 %, which is added in the revised version (Section 4.3) as:

We assessed the degree of uncertainty due to the lidar ratio approximation, by varrying this ratio S between $16\,\mathrm{sr} < S < 25\,\mathrm{sr}$. The resulting optical depth uncertainty was 25 %. This agrees well to similar studies with ceilometer by e.g. Wiegner and Geiß (2012).

33) "This study indicates that using an adapted algorithm for cloud base height detec-

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tion, the robust and relatively low-cost ceilometer can be successfully used to extract information on a wide range of hydrometeor types." You did not convince me that you can distinguish type of pareticles, nor their size. Thus, please rewrite the sentence to stress our what you mean.

R2.33: We agree with the reviewer that we are not able to distinguish between hydrometeor types. We have clarified this in the revised version as:
This study indicates that using an adapted algorithm for cloud base height detection, the robust and relatively low-cost ceilometer can be successfully used to extract information from various hydrometeor layers over the ice sheets, including the frequently occurring optically thin ice layers.

34) I would add two references: Lampert et al. (2012) and Wiegner and Geiß (2012)

R2.34: We thank the reviewer for pointing us to this relevant literature and have included these references in the revised version.

35) Fig.5: Why the average profile on the right hand site is below zero from about 2,5 km, i.e. after the liquid cloud? Are the data not noise corrected here? The PT algorithm aims at detecting lower most layer, would it be very difficult that it would detect also liquid layer at the same time?

R2.35: We thank the reviewer for pointing at this issue and we discovered a processing problem during the plotting of this figure. We have solved this issue for the revised version. However, the case that was used for Figure 5 was not very clear anymore. We have therefore found another case that shows more clearly what an example profile looks like. We show on purpose a profile without noise correction to emphasize what the original profile looks like and where the final CBH is placed by the different algorithms.
Regarding the reviewer's second question, it could be possible in theory to detect the base of liquid layers at the same time. The algorithm would therefore be run in two

cycles with a different sensitivity (as was also done in Section 4.4). However, this is not the primary scope of this study, and including the detection of liquid layers in the core algorithm would require numerous additional sensitivity analyses to assess the impact of the liquid backscatter threshold choice on the results. We have therefore excluded this option in the main PT algorithm.

36) Fig.7 Why PT algorithm does not detect any cloud from 0-3 UTC on the upper subfigure? There are no clouds there? What does detect then the THT algorithm?"

R2.36: Referring to R2.35, we have discovered a processing issue for this case. This problem has been solved, making the case that was shown in Fig. 7 less suitable for clarifying our point. We have found another case (14 March, 2011), which is more representative for the PE observations. In the current Figure 7, no false detections by the THT algorithm are reported anymore.

3.3 Specific comments

Specific comments such as technical corrections and suggestions provided by Referee 2 throughout the text have been inserted into the revised manuscript.

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