amt-2022-10

” Detection of supercooled liquid water clouds with ceilometers: Development and evaluation of deterministic and data-driven retrievals”

Authors’ response to comments from reviewer #1

2 May 2022

We would like to thank the editor and the two reviewers for their very constructive comments on our manuscript. We received genuine insights, which have significantly contributed to increasing the manuscript quality and potential impact. To improve the clarity in our responses we have numbered the reviewers’ comments: for example, the comment 1 from reviewer 1 is listed as R1C1 and will refer to these comments as such in the following.

Based on some of the comments from the reviewers and late feedback from our co-authors, we also made small additional changes to the original manuscript:

(1) Following internal discussion between the co-authors, we realised that our initial definition of Supercooled liquid clouds (SLW) was misleading as it included both mixed-phase and SLW clouds. With our technique, and the technique from T19, we can detect clouds containing SLW, e.g., both SLW clouds and mixed phase clouds, as we cannot distinguish between the two. We therefore decided to replace instances where we referred to both SLW and mixed phase as “Supercooled Liquid water Containing Clouds” or SLCC, including in the title.

(2) We have incorporated late feedback from one of our co-authors; these were mostly typo and added precisions in the manuscript and figures.

(3) We have applied our two retrievals to additional data from the same instrument (the ceilometer) that covers a full annual cycle including austral winter months. We have included a new section 3.4 in the manuscript and a new Figure 10. We think that this will add value to the paper, showing how this new retrieval can produce climatology of supercooled liquid water containing clouds in regions such as Antarctica where observations are scarce. We also added a few sentences putting these results into perspective and in the light of other observations in Antarctica and elsewhere.

(4) We made some cosmetic modifications in the Figures: In Figure 1a, the arrow was wrongly labelled “North”, we removed the label to Progress 3 and removed the bathymetry. In Figure 5, we changed the x-axis units. Figures 7 and 8 saw the unit of the axis changes to km instead of m to simplify the axis unit labels.

Reviewer #1

Overall comment

New algorithm has been developed to detect supercooled liquid water (SLW) by just using ceilometers. New method is compared with the existing method to observe SLW with more extensive observational data set and existing algorithms to identify SLW with only ceilometer. Authors have developed new method, utilising machine learning, that perform better compared to the existing algorithm for simple instrumentation. Manuscript is well written and mainly clear - some specific clarifications are requested in the followed section in detailed comments. I am suggesting this manuscript will be accepted after minor revision (mainly clarifications).

Detailed comments

How does authors see the potential of using this method elsewhere i.e would it require location-specific training set to detect SLW? Would be interesting to see test results for other location with / without specific training set. In addition, two models - trained with location specific data - could give different results on the same attenuated backscatter profile, right? Could authors elaborate on these aspect a bit in the manuscript. Would this cause some problem in some applications? Is this something to be accounted for?

R1C1: Thanks, this is indeed a very important aspect, and we may have overlooked this in our discussion. We have now added this paragraph to the discussion section (lines 973-980):

"One important aspect of our approach is that each locally trained model will provide a given cloud phase retrieval, and various training sets will give various cloud phase retrievals. We labelled our model G22-Davis and following that logic, we can imagine for example, G22-Casey as another model trained on data collected at Casey station. It will be important in future work to evaluate the difference between model retrievals based on various training sets for the same applied dataset. Given these constraints, our other approach proposed in this study, using empirically defined thresholds on peak characteristics could provide a benchmark cloud phase model to refer to, to evaluate each of the G22 trained models. "

Work is currently underway utilising observations in NZ to evaluate how the current approach and model could transfer to these datasets.

line 49: Please add reference for: "Typically, a depolarization ratio below 10% is characteristic of SLW clouds, while higher values are produced by ice particles"

R1C2: The reference Ricaud et al. (2020) (already listed in the references) has been added to that sentence.

line 147: it is stated: "(2) remove noise by applying a noise removal algorithm and subsampling the data to 5 min, 50 bins;"
Could you please describe what kind of noise removal is applied – how it is done and same for bin sampling. More information is needed.

R1C3: Thanks, we added more details as follows (lines 159 to 153):

" "(2) remove noise by applying a noise removal algorithm and subsampling the data to 5 min, 50 bins; The noise removal is done by estimating the distribution of noise at the highest available range and subtracting the mean of the distribution (scaled by the square of range) from all bins in the column. In the cloud masking, the standard deviation of noise is considered when determining if a bin is cloudy. By default, five standard deviations are subtracted from the value before the cloud mask threshold is applied. This is done to prevent false positives with sufficient probability; Subsampling is mostly done to improve signal-to-noise ratio. The cloud masking usually benefits from subsampling to 5 min intervals and 50 m vertical resolution, because it decreases the number of misclassified bins;"

line 148: "(3) calibrate the attenuated backscatter using the approach of Hopkin et al. (2019)"
Please clarify if this is done after subsampling and if subsampling is generating some effect to the method?

R1C4: This is done after subsampling and we have clarified this in the text, it now reads as:

"(3) After noise removal, then subsampling, ALCF performs a calibration of the attenuated backscatter using the approach of Hopkin et al. (2019). In addition to the absolute calibration, the instrument built-in software applies overlap calibration internally. The final pre-processed products were daily netCDF files including the total attenuated volume backscattering coefficient (ß, m^{-1} sr^{-1}) at a resolution of 5 min and bin vertical resolution of 50 m."

line185: Please clarify how ice virga is defined?

R1C5: We presume you refer to line 135. We added this sentence:

"… perpendicular channel. Ice is defined to be virga rather than cloud when it exists beneath a SLW layer. It also…"

line 257: It is stated that "the width of the peak must be < 4,"
It remains unclear, what units are in case of 4? Range gates (4*50m)? Please clarify, for example stating ", corresponding to X meters".

R1C6: The sentence now reads: "the width of the peak must be < 4 bins (corresponding to 200 m)". See also same comment from R#2 at R2C11.

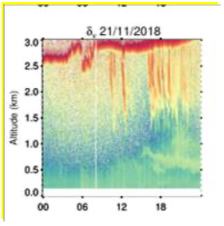
line 249: "extinction other than molecular in the lower levels, cannot be directly compared in terms of backscatter values to peak"
Can you please clarify the terminology usage, why "extinction" and not "attenuation"? It is also stated that there is no other extinction than molecular – how about effect of aerosols?

R1C7: Thanks, we indeed made a mistake here in using extinction where it should have been attenuation. 'Attenuation' refers to the loss of (lidar) signal as the lidar signal propagates up through the atmosphere, for example: 'a SLW cloud layer usually fully attenuates a lidar beam, resulting in no signal return'.

In contrast, the extinction is, more formally, the 'particulate extinction co-efficient' which we calculate from solving the lidar equation. Extinction (along with backscatter and lidar ratio) are the properties of the aerosols / clouds themselves.

The presence of aerosols along coastal Antarctica has been demonstrated (as measured by ship campaigns (Humphries et al. 2021, https://doi.org/10.5194/acp-21-12757-2021). Near the coast, there is much less sea salt aerosol than over the warm Southern Ocean, instead aerosols are mainly biological in origin.

However, the extinction by clouds far exceeds extinction from aerosols. For the whole PLATO campaign (3 months of Summer), we only recorded one nice clear aerosol example (as per the below figure that shows depolarisation ratio). The aerosols can be seen at 500-1000m from about 1500 UTC onward. The detection of aerosols during PLATO is limited by the continuous daylight (local midnight is at about 1930UTC), and thus high SNR for most of the summer.



Line 258: Does this "multiple peaks" -group consist all cases where number of peaks > 1? Did you check if there is any difference between beta value in cases of 2nd peak and 3rd peak? Is "multiple peaks" usually only 2 peaks. Will there rise some implications in case of 3 peaks?

R1C8: Thanks, this is a very relevant question, and we indeed failed to address this in the manuscript. This was also raised by Reviewer #2 in R2C12 and R2C13. In the paper, the "multiple peaks" group include peaks within a profile that are either second (in terms of altitude, so after a first peak has been identified), or third or fourth. While the "single peaks" group include all first peaks identified, whether there are multiple peaks in the profile. We now realise this could be confusing, so we propose to change the terminology to:

"Multiple peaks" → changed to → "Secondary peaks"

"Single peaks" → changed to "Primary peaks"

This is changed in the text and figures.

In addition, we also add information about the cases where peak numbers equal to 3 and 4 have been found. The caption of Figure 3 now reads:

"Figure 3: Distributions and Kernel Density Estimates of values of attenuated backscatter for identified peaks. Primary peaks are labelled in blue (3,727 datapoints), while profiles including secondary peaks (peak numbers equal to 2, 3 or 4) are shown in orange (570 datapoints, including 539 datapoints with a peak number = 2, 26 datapoints with a peak number = 3 and 5 datapoints with a peak number = 4). Vertical dashed red lines indicate the median values of primary and secondary peak distributions. Adjusted secondary peaks (secondary peak attenuated backscatter values + offset) are shown in green."

The occurrence of cases with a third peak (26 cases) and a fourth peak (5 cases) are very rare, i.e., 0.6% and 0.1% of the cases, and therefore the number of datapoints is not sufficient to provide a statistically meaningful distribution to rely on to use the same approach as for the cases with second peaks. We have therefore decided to group all 2nd, 3rd and 4th peak cases into the same group labelled "secondary peaks" and use the properties of that group to adjust the peak values for 2d, 3rd and 4th peaks. This is imperfect, but the only solution we found acceptable given the data constraints.

The paragraph covering this has been modified as follows:

"We call the first group of peaks that don't see lower-level attenuation "primary peaks" and the group of peaks that are higher in altitude above primary peaks "secondary peaks". For primary peaks, data for which SLCC were identified were selected based on the Boolean condition defined using the radar-lidar cloud mask. For secondary peaks, an empirically based set of conditions must be defined to extract only potential SLCC peaks from the secondary peaks. These conditions were based on the observed statistical distribution of peak properties and were empirically set as: the width of the peak must be < 4 bins (corresponding to 200 m), the peak width height must be > 40 x 10^{-6} m^{-1} sr^{-1}, and the peak prominence must be > 60 x 10^{-6} m^{-1} sr^{-1}. The secondary peaks group also include the cases of third and fourth peaks in elevation above the primary peaks when found. Third and fourth peaks were only found in 26 cases (third peak) and 5 cases (fourth peak), representing 0.6% and 0.1% of the cases from all identified peaks. This very small sample size did not allow to use the approach proposed for second peaks and therefore 2nd, 3rd and 4th peaks were all included in the same "secondary peak" group."

Lines 265-270 "The difference between the median value of the single peak distribution and the multiple peak distribution can be calculated and is equal to 4.20 x 10-5 m-1 sr-1 ."
It remains unclear how this was calculated, please clarify. Is there difference between 2nd and 3rd peaks (see previous comment)?

R1C9: This sentence (new lines 303 to 304) now reads as:

"The absolute difference between the median value of the primary peaks distribution ($Q2_{prime}$) and the secondary peaks distribution ($Q2_{second}$) can be calculated as $|Q2_{prime} - Q2_{second}|$ and is equal to 4.20 x 10-5 m^{-1} sr^{-1} ."

The 25 cases of third peaks, and 5 cases of four peaks both have a wide distribution of peak values, and it is not possible to draw conclusions, given these small sample sizes.

Section 2.3 Enhanced data-driven ceilometer cloud phase mask
More information is needed in this section. Please clarify and describe:
- line 282: what are these "learners"? Please clarify. - How are the training and validation data sets selected? How long training set? How long validation data?
- Can you please tie these ML world terms into observations / data used. Many aspects remains unclear for the people unfamiliar with machine learning and it would be impossible for people to reproduce this algorithm. Any comment on this aspect?

R1C10: Thanks, we are addressing this comment as three points as per below:

(1) "learners": we modified that sentence, and it now reads: "The principle of this algorithm relies on a "boosting" strategy, where predictions of "weak" learners (here, "learners" are decision trees) are combined to produce a "strong" learner by utilising additive training strategies."

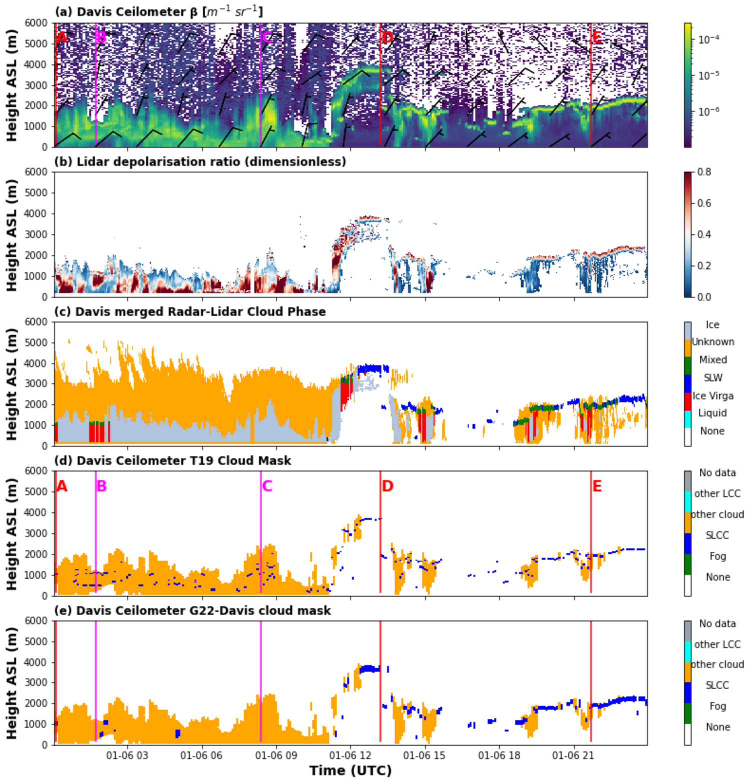
(2) “How are the training and validation data sets selected? How long training set? How long validation data?”. See also response to R2C19. The split between test and train was implied in the use of the 3 K-fold cross validation approach. This is now explicit, and a new sentence has been added at line 345: “With the 3 k-fold cross validation, two third of the data are allocated to training, while the remaining one third of the data is used for testing.”

(3) “Can you please tie these ML world terms into observations / data used. Many aspects remain unclear for the people unfamiliar with machine learning and it would be impossible for people to reproduce this algorithm. Any comment on this aspect?”: We added this sentence below in new lines 352-355 to provide information on the implementation of the ML approach. It was already mentioned in the acknowledgements, but we think it is needed here as well, following your comment.

“Data preparation including splitting the data into training and testing for cross validation was implemented using the python library scikit-learn (Pedregosa et al., 2011) and the model XGBoost was implemented using its python library XGBoost (Chen et al., 2015).”

figure 5: would it be possible to show the depolarization values in this plot? Would be interesting to see the data especially in the first half of the data period when T19 method gives lots of SLW detection and reference not.

R1C11: We have now included in Figure 5 a subpanel showing the depolarisation ratio from the Lidar. This is the new subpanel (b) as shown below.



line 435: How good is the reference in the first place? This reference method could be described in more detail as it remain unclear how the reference combination algorithm is working. Is this method seen as reliable reference? Please offer some references.

R1C12: We added a new paragraph to highlight the robustness of our lidar-radar mask at new lines 198 to 205:

"It is necessary to determine the uncertainty in the Raman lidar liquid phase product before quantifying the performance of the ceilometer liquid cloud algorithm. To this end, we performed a Monte Carlo simulation using a random population of N = 1,000 samples from a normally distributed population (Alexander et al., 2021). For each integrated cloud attenuated backscatter βint and integrated depolarization ratio δint point, we have associated uncertainties Δβint and Δδint, which we set to be twice the standard deviation of the normal distribution. We then determined the cloud phase for each of these 1,000 realizations. These simulations indicated that we misclassified only around 0.3% of the SLW as ice with the Raman lidar during the three months of observations, showing the robustness of our radar-lidar cloud phase product."

line 538: "raw" means different things for different people. I suggest to clarify, do you mean the data in output files or "attenuated backscatter" - meaning the data after calibration. Is it necessary to calibrate the signal before use?

R1C13: We replaced "raw" by "attenuated". As discussed in the methods section of the paper, we did some calibration as part of the ALCF pipeline, using the attenuated backscatter from the instrument (self-calibration). We do not think it is necessary to go into these details here again in the discussion.

line 539: Just out of curiosity: how many ceilometers there are in Antarctica where no other potential devices are installed?

R1C14: We have sent emails out to the whole of the SCAR Antarctic Clouds and Aerosols Action Group with the aim of collating all the ceilometer data collected around Antarctica. This includes emails to around 25 distinct groups, but the number of sites with only ceilometers deployed will be less than half of this number at present. However, several sites have longer term datasets than those presented in this study so there is some useful spatio-temporal analysis possible potentially.

line 561: ”The new ceilometer algorithm described herein has been developed at the Bureau of Meteorology and is not publicly available.”
For what use this algorithm is developed if it is not publicly available and cannot be reproduced based on this manuscript?

R1C15: See also R2C27. This was the status at the time of the submission, but our approach has changed since, and we are preparing the algorithms to be included in the ALCF developed by co-author Peter Kuma. It will therefore be available and applicable easily by future users. The text of the manuscript has been changed to:

“The new G22-Davis ceilometer algorithm described herein as well as the original T1 algorithms are in the process of being included in ALCF and will therefore be open-source and publicly available.”