Review of: ”Optical and microphysical properties of ice crystals in Arctic clouds from lidar observations” by Chazette and Raut.

General remarks

The manuscript introduces an approach combining different lidar observed parameters to retrieve ice cloud properties. It makes use of Raman and depolarisation measurements to obtain lidar ratio and thus extinction profiles which are then inverted into ice water content and ice crystal effective radius. The method is applied to a case study of arctic ice clouds and analyses the vertical profiles of the retrieved cloud microphysical properties. As observations in the Arctic, where we can expect a significant amount of pure ice clouds, are still rare, observations and retrievals as presented in the manuscript are of high value. Therefore, the study is well suited for publication in AMT. However, the manuscript lacks in several major issues and, therefore, does not reach its full potential. These major issues have to be reassessed in detail before I a can recommend publishing the manuscript.

First, the manuscript fails in presenting and quantifying the uncertainties of the retrieval. Some issues are named but not quantified. Due to the lack of in situ validations of the retrieved cloud properties, this is a major limitation of the manuscript and does not promote the future application of the method. Second the interpretation of the clouds observed in the case study is partly misleading because the authors misinterpreted the cloud type as far as I can conclude from lidar plot. This leads to an unprecise analysis of the clouds in the discussion.
Below, I compiled a list of comments which have to be considered in a revised version of the paper. There might be some contradictory statements which result from my misinterpretation of the text when first reading the manuscript. I am sure the authors will know how to weight in such cases and how to improve the text to avoid misinterpretations by other readers.

Major comments

Presentation of the retrieval algorithm and uncertainties

The objective of the AMT journal is to present new methods and elaborate their potential and uncertainties. This was only partly achieved by the authors. First, it is not clear to me if the presented retrieval approach is new or if methods well documented in literature are used? It needs to clearly stated, what are the new aspects of the presented approach and how does it compare to other commonly used cirrus lidar retrieval?

The second objective should be to demonstrate and quantify the uncertainties of the retrieved cloud properties. Only some uncertainty sources are mentioned, but no final quantification is given. In the end, each profile of extinction, IWC and R_{eff} needs to be presented with an uncertainty range. This is of special importance as there are is no in situ validation available. Here I may summarize some of my thoughts. More details are given in the specific comments below.

- How calibration uncertainties of the lidar lead to uncertainties of the retrieved cloud properties?
- How uncertainties of LR transfer into the retrieved COT and R_{eff}?
- How the assumption of spherical particles affects quantitatively the retrieved COT and R_{eff}?
- How the contamination of measurements by liquid droplets lead to biases in the retrieval? The lidar backscatter plot suggest, that liquid droplets are present.
- How large is the ”little” influence of multiple scattering?

Definition and interpretation of the cloud types

The use of cloud types which are analyzed in the study and for which the retrieval approach works is not well specified. This addresses two aspects:

a) Starting with the title, it is unclear, what type of Arctic clouds can be analyzed by the presented method. Step by step, the limitations of the retrieval approach reveal. Only ice clouds and only cloud with $\tau < 2$, while in the introduction often stratiform and mixed-phase clouds are discussed. This limitation of the analysis should be made more clear from the beginning, starting with the title.

b) To my point of view, the clouds observed in the case study are not correctly classified, which leads to some misleading interpretation of the data. I only see a few stratiform clouds in the lidar backscatter plot. Most clouds extend through a wide range of altitudes and are rather inhomogeneous. The clouds are also not formed in the boundary layer as indirectly suggested by analyzing the UAV measurements. To me, it rather looks like ice clouds which form in high altitude and which then sediment downward. Some liquid cloud tops (in this case maybe stratiform), where the lidar is immediately extinct, are present in a range of 2 km altitude on 16 May and 2-3km altitude on 17 May. I assume, that the sedimentation of ice crystals from the higher ice clouds leads to a glaciation of most of the lower liquid cloud layers (seeder-feeder effect). Only few mixed-phase clouds can sustain. This makes the cloud situation quite complex. This misclassification has two implication. First, the comparison with cloud properties reported in literature can become misleading when different cloud types are compared. Second, the retrieval may suffer from the existence of liquid clouds. This impact needs to be discussed.

List of specific comments

P1, title: The title is partly misleading. It reads very general and to some point suggests to provide a more general statistical evaluation of ice cloud properties. To avoid raising wrong expectation, I suggest to add, that the manuscript a) focuses on demonstrating a method to derive ice clouds properties from lidar and b) that only one case study is analysed.

P1, L20-22: Something is missing in this sentence. ”large”/”low” optical thickness?

P1, Abstract: The abstract does not include what is the major new contribution to the scientific discussion. Is the presented lidar retrieval a ”new” technique which is worth to be publish? Or are there major conclusions on the relevance/impact of the cirrus properties? Why should I read this paper?

P1, L26: The radiative impact of ice clouds was not calcualted and discussed in the study. I also doubt, that single location lidar observation can provide a ”large-scale” estimate of the cloud radiative impact. Additionally I do not understand, what ”experimental resources” will be reduced? To what are you comparing? And is this conclusion justified?

P2, L30: The 1 km averaging only holds for satellite lidar. Airborne lidar have a much higher spatial resolution.

P3, L6: If $\tau < 2$ is the constrain of the study, then this should be highlighted from the beginning including the title.

P3, L13: PASCAL was in parallel to ACLOUD in 2017.

P3, L33: As the manuscript aims to be a technical paper, at least the major uncertainties documented in the provided literature should be repeated here.

P4, L3: Averaging of 1 min is also rather long. Assuming a $10\,\mathrm{m\,s}{-1}$ wind speed, you end up to a horizontal resolution of 600 m. This is similar to what you stated in the motivation for satellite observations and for this you concluded that is is not sufficient to study cloud structures.

P4, L9: Should it be rather ”cloud-free atmosphere”? I understood, that you have multiple scattering if there are cloud, especially liquid clouds.

P4, Equation 1: I recommend to avoid these horizontal brackets in your equations. I first thought this is a normalization. Rather add additional single line equations defining these properties.

P5, L5: Can you give a statement, why this assumption (aerosols is negligible) is needed?

P5, L19: Does A_C close to 0 mean, you can simplify Eq. 2? If yes, I would show this simplification.

P6, Equation 6: Why ”A” appears here is without index? If it is A_C and you assume $A_C = 1$, then you end up with Equation 4. Is this intended?

P7, L2: This AOT value certainly does not hold in general. Do you refer to Chazette et al. (2018) because it analyses the same measurements/cases? Then please highlight, that the AOT value only holds for this case.

P7, L9: In what respect do you use ”effective” here? Effective cloud properties merging liquid and ice particles?

P8, L1: Does this second method provide identical results? What method did you use in your analysis?

P8, L6: This reads strange. I'm not sure, how it is similar to aerosol when there still can be aerosol. As I understand, you substract the molecular DR from the VDR to derive only the contribution of ice crystals. In principle, you end up here with a DR, which characterizes aerosol particles and clouds? Only that you assume, that there is now aerosol particles.

P8, L11: add: ”Monte Carlo radiative transfer model”

P8, L24: The references of the justification using spherical particles is not well chosen. These references describe microphysical schemes in numerical cloud modelling. Assuming spherical shapes in microphysical schemes is something different to assuming spherical particles in radiative transfer models, where the radiative properties matter. This brings me to the question if you can discuss and quantify the uncertainty of your retrieval results with respect to ice crystal shape.

P9, Equations 15&16: Why these two parameterisations are given? Which one is applied in your study?

P9, L18: Can you show how/where in Eq. 15&16 you derive the link between v_{eff} and μ?

P9, L20: ”shape” parameter: Can be misleading, as you discuss on the one hand the shape of the ice crystals and on the other hand the shape of the size distribution. Try to be more precise here to avoid misunderstandings.

P9, L24: Why LR is retrieved? I thought, that R_{eff} will be retrieved from the lidar observations. Similar, it was hard to follow the entire approach and processing chain (e.g., where the Mie calculations are used?). This leads me to suggest adding some kind of overview/flow chart of the entire algorithm to the manuscript. This overview may summarize the most important processing steps, measured parameters and retrieved parameters, and equations/models applied in the retrieval.

P10, Figure 1: The sensitivity of LR with R_{eff} is rather low for large R_{eff}, which are typical for ice clouds. Can you estimate the uncertainty of retrieved R_{eff} based on the measurement uncertainty of LR?

P11, L16: Having negative temperatures does not guaranty the formation of ice clouds. Most likely, super-cooled liquid clouds will from in these conditions. Maybe mixed-phase, if some ice crystals are formed. But still ice formation at temperatures close to $0 \circ$C is not efficient.

As shown in Fig. 3, the clouds do not form in the boundary layer. It rather looks like high ice clouds, which sediment downward. In the altitude range of the UAV, I can not observe any cloud formation. Some liquid cloud tops, where the lidar is immediately extinct, are more in a range of 2 km altitude on 16 May and 2-3km altitude on 17 May.

P11, L32: Your horizontal spatial resolution does not resolve such small scales. What I can identify are stratiform liquid layers. However, the ice cloud is not directly connected to the liquid clouds. I assume, that the sedimentation of ice crystals from higher ice clouds leads to a glaciation of the lower cloud layer in most of the time (seeder-feeder effect). So this is something different to the ice/liquid pockets which are described in the cited literature for stratiform mixed-phase clouds.

P12, L3-9: These lines should be moved to somewhere before you discuss the coexistence of liquid and ice. Maybe move to line 28 on page 11.

P13, Figure 3: Add day/month/year in figure caption.

P13, Section 4.2: I would have liked the synoptic situation discussed before or while introducing your case study. Switch with Section 4.1.? Or merge with a general description of the case in Section 4.1?

P14, Figure 4: Indicate where your measurement site is located. Otherwise, the map does not help a lot. As you are investigating clouds, a second panel row with cloud cover, ice water content or humidity might help. This would help to understand how the larger area cloud field looked like. A satellite image might also do it.

P15, L1: "write "as indicated in the time series of Fig. 3."

P15, L2: This once more violates your motivation of ground-based lidar measurement having a high spatial resolution. With averaging you end up with horizontal distances far beyond kilometers. Why averaging is needed? How the results would look, if no averaging is applied in order to analyse the detailed horizontal structure/dynamics?

P15, L3: In case 2, the lidar reflectivity suggest, that some liquid clouds are present and included in the averaging. How does this affect your retrieved ice cloud properties?

P15, L14: What a priori assumption of LR did you assume here? Didn't you had a measured LR?

P15, L20: How these uncertainties of LR transfer into the retrieved COT?

P15, L30: What about liquid droplets contaminating your retrieval?

P15, L32: What size distribution do you retrieve from your method?

P16, L3: Can you quantify "little" influence?

P16, Table 1: The Klett method was not explained in the methods section. What is different to your approach? What are the known systematic differences?

P17, Figure 5: Figure is of poor image quality. I suggest to add a legend for the different lines. I also suggest to check for color-blindness and maybe vary line stile to differentiate the lines.

P17, Figure 5: Can you add uncertainty estimates to the extinction profile?

P18, L12: This shows only the sensitivity for changing v_{eff}. But how about the uncertainty of retrieved R_{eff} with respect to uncertainties in LR?

P18, L15 - P19, L5: Mioche et al. (2017) characterized low-level mixed-phase clouds. In most cases stratiform mixed-phase clouds in the boundary layer. I don't think, this cloud type can be compared to the ice cloud cases you present here. They are of different nature. This should be emphasized in this comparison.

P19, L7-10: You did provide a synoptic analysis of the cloud case. It should be easy to identify the origin of the air mass and potential aerosol particles/pollution sources. Does pollution manifests in your estimates of aerosol particles? I thought, aerosol particle concentration is assumed to be low and neglected in the retrieval.

P19, L23: I don't see a stratiform cloud in your case study. These comparisons need to be done with more care.

P20, L10: I obviously missed this part in your description of the method. How the scattering of liquid cloud parts is removes/extracted from the ice cloud backscattering in the analysis?

P20, L15: This is somehow strange to read. Mie calculations always assume spherical particles. Do I need to understand this conclusion in a way that you would not have had an agreement with Mie calculations, when non-pherical particles are present?

P21, L1: Provide uncertainty ranges of your retrieved cloud properties in the conclusion.

P21, L6: The lidar retrieval was not validated against in situ observations. I would be hesitant to replace valuable in situ observations by remote sensing observations. In addition, in situ observations do provide much more than only IWC and Reff (e.g., crystal shape, size distribution, roughness,...).

P21, L7: It was not shown how the lidar retrieval of the study would complement common lidar/radar retrieval.

P21, L11-14: To extend you method to non-spherical ice crystals you would need an assumption on the ice crystals shape. Only knowing that the particles are non-spherical, would not help. What shape/phase function would you assume? How ice crystal size is then translated into IWC (volume-size ratio,...). This conclusion is very speculative and certainly not ”easy”.