Review of 'Ice crystal number concentration from measurements of lidar, cloud radar and radar wind profiler' by J. Bühl et al.

The manuscript describes a methodology that aims at estimating the ice crystal number concentration (N_i) and ice crystal number flux F_i (N_i multipled by the terminal fall velocity v_t) based on combined measurements from a ground-based cloud radar (CR) and a radar wind profiler (RWP) or lidar. More specifically, the authors present here two retrieval methods that can independently be used depending on instrumental availability. A first method uses the CR reflectivity (Z) and spectral width (w) and the RWP v_t to constrain parameters of a particle size distribution (PSD) and infer N_i and F_i. Alternatively, in the absence of RWP measurements, the lidar extinction (E) is instead used in combination with Z. The authors describe the theoretical basis for these methods, which are based on rather sophisticated LUT approach, and propose a brief uncertainty analyses. Finally, a case study representative of a mixed-phase cloud is discussed.

There is high scientific interest for this work, as only few methods are today dedicated to remote-sensing retrievals of N_i, despite the importance of this parameter to better understand ice clouds and represent them in models. A particular novelty of the approach proposed in this manuscript is its strong focus on radar measurements and the use of a RWP. Indeed, other existing methods for N_i retrievals strongly depend on lidar and/or thermal infrared measurements and therefore perform poorly in optically thick ice clouds. The idea presented here is thus very interesting, but I still have a some major concerns regarding some aspects of the retrieval method, detailed in the comments below. Also, I would have liked to see more analyses of the retrievals, especially since the authors indicate in Sec. 2 that measurements are available during a 4-month period and later in the conclusion that the retrieval method is very fast. But this paper already constitutes a first step and the manuscript is well within the scope of AMT. The manuscript is well written, although can be a bit difficult to follow in its most technical sections. Overall, I recommend publication of the manuscript after major revision, provided appropriate response to the comments below.

General comments:

1. My first general comment concerns the use of a LUT approach, which appears as a strong limitation here considering the great instrumental synergy that could be obtained here. I was wondering why this choice, until the very last paragraph of the conclusion where it is finally justified. There are clearly advantages of going for a LUT approach (keeping a clearer view of the physical aspects, retrieval speed), which are mentioned by the authors, but I can't help but think so much more could be done with a variational method: all measurements could be used simultaneously, and a proper sensitivity / error estimates study could be performed (see my following two comments). Obviously, the authors shouldn't switch to a variational method for this study, but such discussion must be addressed earlier in the manuscript, and I strongly encourage the authors to migrate to a more flexible method in the future.

2. I have issues with the error analysis done in section 3.5 and summarized in table 2. Uncertainties on retrievals should have two origins: first, the propagation of errors due to non-retrieved parameters (here p, T, s, particle shape, parameters of M(D) and A(D) relations, ...) and

instrumental accuracy (here v_t, Z/E, w), and second the sensitivity of these measurements to the parameters to be retrieved. It seems that only the first aspect is addressed here? And, if so, why not include the errors due to the choice of a particle shape in Table 2? Since this is also a non-retrieved parameter, it should appear there. It would also be good to attribute uncertainties on the assumed parameters of the mass-diameter and area-diameter relations. Finally, showing directly the relative uncertainties on N_i and F_i in table 2 would be clearer for the reader.

3. Why are the CR, RWP and lidar not all used together if they are available simultaneously. The authors seem to hint at a redundancy between the information in v_t and Z/E, but the case study shows that should be different type of information. And it is also expected that the lidar (E) carries information on a different part of the PSD (small particles) compared to the CR and RWP. The authors indeed show in the case study that there is almost no difference between the mean N_i and F_i retrieved by both methods if the "side-planes" shape is used whereas very large differences between the two methods appear for "plate-like" particles. Doesn't this indicate that there is information on the particle shape that could be further constrained by simultaneously using all available measurements? In any case, the discrepancies between the results from both methods should be further discussed.

4. Finally, it would be useful to explicit more clearly in the abstract or conclusion the conditions under which the retrieval methods can be applied. For instance, p 8 l 6: "only small pristine particles are considered in the context of this work" (and what does "small" mean?). Same p 4 l 17. Also, do these retrieval methods only apply to thick and relatively warm mixed-phase clouds as illustrated in the case study?

Specific comments:

5. The study by Delanoë et al. [2005] is cited as a reference to the shape of the PSD used in this study (gamma-modified, Eq. 1), which is the most central element of the retrieval method. The authors also indicate, p 3 l 9-10, that "this kind of distribution was described by Delanoë et al. [2005] as being universally applicable for ice crystal populations". First, a main conclusion by Delanoë et al. [2005] (or, more recently, Delanoë et al. [2014]) is that, **when properly normalized**, PSDs tend to fold into a unique universal modified-gamma distribution shape. By normalized it is meant that the concentration and size axes of the PSD are normalized in order to remove dependency on parameters that are important to the shape (in the case of Delanoë et al. [2005], it is IWC and D_m that become constant after normalization). I do not see mention of this central aspect in the text. It seems from Eq. 1 that D is indeed normalized by Dm and that there is a normalization coefficient for the concentration (C), but the latter isn't discussed anymore. Please comment. Second, the PSD shape in Eq. 1 does not correspond to a gamma-modified PSD, but it does correspond to a gamma-μ indeed mentioned in Delanoë et al. [2005] but that was found to be a less accurate representation of in situ PSDs than the gamma-modified shape. Please clarify.

6. p 2 l 11, and onward in the paragraph - there are many mentions of "crystal size", please define more clearly what you mean by that (maximum size, effective size?). Or do you mean the PSD? This confusion happens often in the text. For instance, p 9 l 19, "ice particle size D

is the most crucial intensive parameter for the retrieval of N" - is it D or N(D) that is crucial to N (I'd agree on the latter)? Similarly, in p 3 l 8 there is mention of a median diameter D_m, could you explicit what you mean by this? As Delanoë et al. [2005] is mentioned, is it the mean volume-weighted diameter (ratio of 4th to 3rd moment of the PSD)?

7. p2 l13: Ceccaldi et al. [2013] describes a method to infer a cloud classification (phase) from lidar-radar measurements, this is not an appropriate reference here (again p 16 l 3). If you refer to the DARDAR-CLOUD algorithm, then perhaps Cazenave et al. [2019] or Delanoë and Hogan [2010] would be appropriate, but none of these papers discuss of N_i retrievals either. As far as I know, only Mitchell et al. [2018] and Sourdeval et al. [2018] describe satellite products of N_i. The former paper uses thermal infrared and lidar measurements, the latter uses lidar-radar measurements. Additionally, Sourdeval et al. [2018] showed that N_i retrievals using lidar measurements only are possible for cold ice clouds, so the statement p 2 l 14 is not absolutely correct. However, it indeed seem that their satellite retrievals are indeed poor when the lidar is missing.

8. Appendix B lacks references or details. Some are given p9 l21-22, and that seems sufficient to justify Eq. 9, so perhaps Appendix B is not necessary?

9. Table A1 lists m(D) and A(D) coefficients for all sorts of shapes, many of which aren't used. Is it useful to indicate them all?

10. n_L and i_L are inconsistent between the equations p 10 and Fig. 6. Also, in Fig 6b, what are the space coordinates x_i?

<u>Technical comments:</u>

11. p 6 l 2 - typo, "cloumn-like"

12. p 6 l 4 - "particle shapes" rather than "particle species"?

13. p 9 l 19 - type, "parameter"

14. p 9 l 28 - wrong citation format

15. p 10 l 19 - D or Dm?

16. p 13 l 2 - bold m (vector) or m_i (element)? There are other similar inconsistencies in this section.

17. p 13 l 1 - can you define "precisely"? What are exactly the elements of m? Are p and T included?

18. p 13 l 15 - does "the desired vector r" corresponds to the retrieved properties? (understood from Fig. 6 but not detailed in the text)

19. Fig. 5 - is it D or Dm on the y-axis?

References:

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