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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Relation between ice and liquid water mass in mixed-phase cloud layers measured with Cloudnet" *by* Johannes Bühl et al.

Anonymous Referee #3

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The authors present an analysis of a long-term dataset of mid-latitude mixed-phase cloud properties observed by a combination of Ka-band cloud radar, lidar and microwave radiometer. The Cloudnet categorization is used in combination with additional algorithms to estimate liquid and ice water content and ice particle motion to derive ice mass flux.

In general, I find the study interesting and certainly worth for publication in ACP. Unfortunately, the manuscript does not adequately discuss previous work in this field. I also had the feeling that many statements and conclusions given miss proper discussion. I therefore recommend publication after the comments and corrections listed below are addressed.

General comments:

Printer-friendly version

Discussion paper

Lack of references and discussion of previous work:This manuscript puts a “special focus on mixed-phase cloud layers” and aims at characterizing heterogeneous ice formation within them with a combination of ground-based remote sensors. However, while reading through the manuscript, I found the given references and discussion of the results of this study with former work to be rather insufficient. Mixed-phase clouds either in the arctic or in mid-latitudes have been a focus topic during the last decade of many institutions and observing programs (for example the ARM program). Nevertheless, I hardly could find any citation of the important work which has been done in this field in this manuscript. I especially miss a proper citation in the introduction but also in the discussion of the results. In my opinion, a proper discussion of the work which has done in this field would also help to put your results into perspective and would actually strengthen your study. Just to give one example: I did just a half an hour literature review on this topic and found for example a very similar comparison between LWP and cloud top temperature as you show it in Fig. 10a in a recent paper by Zhang et al., “Ice Concentration Retrieval in Stratiform Mixed-Phase Clouds Using Cloud Radar Reflectivity Measurements and 1D Ice Growth Model Simulations”, JAS, 2014 (their Fig. 7). Although their paper is about arctic mixed-phase clouds, such work should be discussed in your paper. In fact, I could imagine that the similarities or differences that one finds for arctic and continental mixed-phase clouds would actually be very interesting for both modellers and observationalists. Therefore, I can only recommend publication of this manuscript when the discussion of former literature related to this study is properly included and discussed.

Pristine Particles: During recent years I realized that people mean different things when they talk about pristine particles. I would like to know more exactly what you mean when you talk about pristine particles. Just single ice particles? Is a single dendrite with some rimed droplets or a broken dendrite still pristine according to your definition? If one reads articles from the in-situ community (e.g. Korolev et al., GRL, 1999) they find even for Arctic clouds in a wide temperature range from 0 to -45°C that only 3% of the observed particles can be classified as being pristine. Considering that one

can expect the conditions for pristine particle growth to be much better in Arctic clouds than in mid-latitude clouds, I wonder how you can be so sure that your remote sensing observations are really related to pristine particles and not to for example polycrystals?

Fall velocity: Throughout the paper you use the term “fall velocity” while I suppose you actually show and talk about the measured radar Doppler velocity. I think you should more carefully distinguish between “terminal fall velocity”, “vertical air velocity” and “vertical Doppler velocity”. The latter one needs to be first corrected for vertical air motion which is not trivial in ice clouds and I could not find that you applied such a correction. For example, in L. 235 you use “vertical velocity” and actually mean the vertical Doppler velocity. Please be more accurate with these terms throughout the text and the Figure labeling. I am also not sure whether the vertical air motion in mixed-phase clouds is really equally distributed in such a way that long-time averaging of the Doppler velocity would results in the terminal fall velocity of the particles. Do you have any indication for this, maybe from other studies? Because I think this is the basic implicit assumption you are doing here.

Specific comments:

L. 171: I would add to rain attenuation also cloud liquid water in general. Typical attenuation values at Ka-band for cloud liquid water are around (depending on temperature) 1.5 (dB/km) / (g/m^3) (see for example Hogan et al., JTECH, 2005). In your case where the liquid water is at cloud top this should not be a big issue but one certainly has to account for it when looking for example at multi-layer mixed-phase clouds like your first example in Fig. 3. Also if the mixed-cloud is at larger heights and the atmosphere is humid, water vapor attenuation cannot be completely neglected at Ka band. In the next sentence you say “strong attenuation is avoided” but what do you consider as “strong”? In fact, any attenuation will introduce a bias in your IWC estimate and hence has to be discussed as a potential source of error. You also state that the 3 dB calibration uncertainty transfers into 30% uncertainty in IWC. However, given the power law in Hogan et al., 2006 for IWC and Z, the IWC error depends on the value of Z. The IWC error for

-10 dBZ due to 3dB uncertainty is certainly larger compared to 0 dBZ.

L. 173: “Radar calibration is estimated to be accurate to 3dB for the LACROS cloud radar”. A 3 dB absolute calibration accuracy for a cloud radar certainly needs comprehensive calibration efforts (e.g. external target calibration, long-term calibration monitoring, etc.) which have to go beyond standard manufacturer calibration. I know that programs like ARM invest a lot of money and man power to reach a 3 dB calibration accuracy for their radars so I and probably many readers would be curious to know more details about the calibration efforts you performed to reach this level of radar calibration accuracy.

L. 179-184: From Fig. 6 I find the majority of points being above -23 dB SNR. I can only see that the SNR in general decreases towards higher temperatures. Maybe I missed it, but can you explain this behavior? It is quite unfortunate to use almost the same color (grey) for the data points of LDR between 0 and -14 dB and for the points were a reliable LDR estimate is not possible. Please change.

L. 187: I think I understand what you try to say with your definition of your Z-threshold but for the broader audience I think you should explain a bit more: Why 5000, why -45 dBZ, etc.

L. 215: I can’t see why aggregation is the main reason for the low LDRs. Aircraft in-situ data often show that the particles show simply irregular shapes. Also the tendency of the particles to form aggregates in general decreases with lower temperatures.

L. 243: I think this statement is a bit oversimplifying since it is only true if all particles within the volume are perfect Rayleigh scatterers. Particularly close to the dendritic growth region you can hardly be sure that no aggregates are present. Also, if the relation between particle mass and reflectivity would be so straight forward as your statement seems to imply then Z-IWC relations for Ka-band wouldn’t show such large errors.

L. 261-268: I see the problem of using the MWR LWP for thin clouds. However, I wonder how well mixed-phase clouds can be approximated with your adiabatic approach given that especially at the top of mixed-phase clouds a lot of mixing due to radiative cooling is taking place and therefore entrainment processes might introduce deviations from the adiabatic LWP estimate. Did you proof that the adiabatic approach is superior to the MWR by plotting for example a scatter plot of MWR LWP vs. adiabatic LWP? For the upper part of your LWP distribution with LWP up to 100-200 g/m^2 the MWR estimates should be quite reliable. Are the 20 g/m^2 uncertainty referring to the relative or absolute LWP uncertainty? I would assume the relative uncertainties of LWP should be smaller?

L. 278: When you say "strongest peak" does this mean you also consider multimodal spectra? Or do you rather mean "maximum of the radar Doppler spectrum"? I think a statement like "actually makes sense" is not very precise nor scientific, please rephrase. Overall, I do not understand why you are not taking full use of the radar Doppler spectrum itself? You can easily derive IWC for each spectral bin, multiply with the Doppler velocity of the bin and finally integrate the resulting flux. At least you should provide a proof that simply multiplying the moments is a similar good approximation.

L. 291-294: One of your colleagues at IfT just published a study (Kalesse et al., ACP, 2016) that shows the importance to analyze processes and probably also fluxes not along straight vertical profiles but along the fall streaks which can also been seen in your data (e.g. Fig. 3). Have you tried such an attempt?

L. 297: You leave the reader quite alone with this statement. Is this value realistic? Is it consistent with former studies?

Style and Typos:

L. 19-21: I would remove the "and" between "aspects" and "involved" and put another comma instead.

L. 21-22: “Laboratory measurements have already delivered a lot of useful information” sounds quite vague to me. Please be more specific.

L. 23-25: Please provide references to the reader by whom this has been measured.

L. 47: In an “only if” construction it should probably be “is there a direct”. Please check.

L. 69-71: Again, please provide the references. Especially in the introduction you should help the reader to find the work you are referring to.

L. 81: “such clouds and may”

L. 94: Please also provide a reference for the radiometer.

L. 179: “an SNR” -> “a SNR”

Caption of Fig. 6: Description is insufficient. Specify what are the single points, the black dots and the error bars.

L. 196: There is no red line in Fig. 7d

L. 230: What do you mean with “raw values”?

L. 208: I think you mean “cloud thickness” here?

L. 255: were investigated

Caption Fig. 10: “averaged for”

L. 279: Comma after “however” missing; “the order of magnitude” of what? The entire sentence is not very clear to me and should be rephrased.

L. 348: Comma before “respectively” missing

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