Reply to 2nd Review of "Relation between ice and liquid water mass in mixed-phase cloud layers measured with Cloudnet" by Johannes Bühl, Patric Seifert, Alexander Myagkov, and Albert Ansmann

We thank Reviewer#2 very much for scrutinizing the manuscript again. We believe that this made the manuscript considerably better and regret that we did not meet all critical points before. In our response below we try again to correct the errors and clarify the issues raised.

I thank the authors for enhancing the manuscript. However, I still have concerns regarding three of the major issues I raised. Remarkably, the authors rejected in particular comments which involve more effort than changing some lines in the manuscript.

Major comments

Vertical air motion: The authors have to provide more details in L 153/154: How much averaging is required to get rid of vertical air motion? Are there any references supporting that? Isn't that in opposition to their statement in the introduction: "Vertical motions keep mixed-phase clouds alive by activating aerosol particles to cloud droplets…".

Influence of turbulent motion induced by the cloud layer itself averages out during an observation time of 15 minutes. The Doppler velocity shown in Fig. 8b and d is, however, still susceptible to large-scale motions longer than 15 minutes. We believe that a lot of the scatter in Fig. 8b and d is resulting from such influences.

A comment about that issue was added at Line 168.

Why did the authors not apply the Shupe et al 2008 algorithm for detecting vertical air motion? The authors seem to know the method since the article is in their references. At least the authors should mention the issue more clearly when analyzing the Doppler velocity and introducing the static ice flux.

The algorithm of Shupe 2008 separates particle fall velocity from the turbulent properties of a cloud-top layer with the help of a Fourier analysis. In the process, the velocity is "mean-removed" and "detrended". Hence, the method can only provide information about vertical air motions with frequencies smaller than the cloud observation time. It could therefore not be used to remove the influence of large-scale air motion from the average Doppler velocity observed within falling particles. For that, an independent and direct measurement of vertical air motion at cloud top or directly within the falling particles (see Bühl et. al., AMT, 2015) would be needed.

Furthermore, the authors often use the term 'fall velocity' when they actually mean 'Doppler velocity' (e.g. Fig 8b and corresponding discussion). Please go through the manuscript in order to use them correctly.

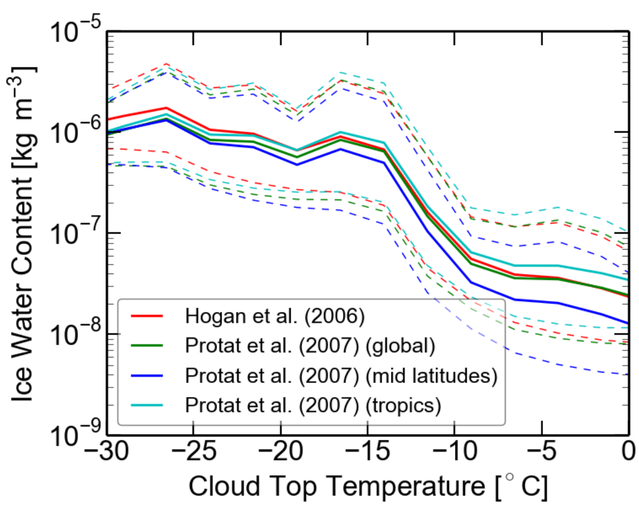
We agree and changed "fall velocity" to "Doppler velocity" wherever we talk about measurements.

Z-IWC relation: The authors responded: "We consider the Hogan retrieval as the best that is currently available." but do not point out why they think Hogan's method is best. Hogan et al (2006) assumed the mass-size relation of Brown and Francis (1995) which is based on a study by Locatelli and Hobbs (1974). They developed the mass-size relation for "aggregates of unrimed bullets, columns and side-planes". Since this study is targeting pristine ice particles, how can a mass size relation for aggregates be used? Moreover, Hogan assumed the median volume diameter of the particle size distribution to be between

0.2 and 3 mm. Is that in agreement with the expected size of the pristine particles? I would recommend to apply 2-3 other Z-IWC relations in order to investigate whether this changes the overall results. I expect this won't change the results, but give a better feeling for the uncertainties.

We believe that the temperature dependence actually implicitly takes into account particles shape in the retrieval. The relationships have been elaborately validated by Heymsfield, JAMC, 2007 (DOI: 10.1175/2007JAMC1606.1) and Protat, JAMC, 2007 (DOI: 10.1175/JAM2488.1). In the following graph we retrieved the Ice Water Content on the basis of the values of CTT and the mean values of Z shown in Figure 7b in the paper (thick lines). We compare 4 different Z-T parameterizations of Hogan and Protat. We conclude that the spread is about a factor of two for the temperature interval -30 to -15°C and about a factor of 5 in the interval -15 to 0°C.

We also now give this information in the paper (Line 207).



LWP retrieval: I still think that a radiometer retrieval would be better suited to get LWP if it is offset corrected during clear sky periods. This would correct not only for water vapor, but also for potential calibration offsets. Since a radiometer is part of the standard Cloudnet set up I do not see why this should limit the applicability of the approach to other data sets. Did the authors actually compare LWP values of the radiometer and the adiabatic approach to each other?

We compared the values and the results are plausible in a statistical sense. However, the unknown bias for an individual measurement is about 20g/m². For individual cases, we found that the background correction algorithm sometimes created implausible negative values of LWP or values that are much too high. The current retrieval would therefore only introduce larger uncertainties and strongly increase the scatter of the measurements. Also the negative LWP measurements would introduce a dangerous bias. The adiabatic retrieval has the geometrical cloud thickness and the adiabaticity factor as an uncertainty, but usually yields plausible values.

Minor comments

Cloud misclassification: The authors responded that they estimate the accuracy of their cloud classification to be around 15% (I hope that this is actually the inaccuracy!). I would suggest to mention this in the manuscript.

We mention this now in Line 198.

Pristine particles: I would suggest that the authors point out in section 4.2 more clearly why the data shows that the observed particles are pristine. In their response to my corresponding comment, the authors points this out more clearly than in section 4.2.

We added a description, trying to clarify the usage of the term "pristine" starting from Line 293.

Static lifetime index: 'static' is not used consistently in the manuscript. e.g. Section 4.4 and Figure 10.

Replaced

- "static approaches" by "approaches" (Line 306)
- "Steady conditions" → "static conditions" (Line 359)
- "Static lifetime parameter index" → "static lifetime index" (Line 389)

L 147: I do not understand this statement, how is 'quality' quantified?

In this case, we actually talk about the "homogeneity" of the cloud cases. By demanding a larger separation between the clouds, the number of cloud cases will go down, but the clouds will be less influenced, e.g. from gravity waves originating from other clouds nearby.

We added this explanation to the manuscript (Line 148).

L204: This calibration is 1) component based (i.e. theoretical) and 2) for a different radar.

2: The manufacturer of the radar (METEK company) does the calibration method described in Görsdorf and Bauer (2015) in the same way for all of its systems.

1: This calibration is indeed component based, so some unknowns remain. An advantage of the LACROS radar is, e.g., that the antenna is characterized experimentally because the radar is equipped with a scanner unit.

We try to explain this better in the manuscript at Line 220.

L 221: I'm surprised that the authors use such a static formula. Don't the authors use the standard MIRA moments which finds the hydrometeor peak in the Doppler spectrum?

The threshold formula only relates to the sensitivity of the detector. In principle, it gives the radar constant.

L255 fall velocities are decreased in 8b, but enhanced in 8d for the highest temperature bin?

The increase at the highest temperature bin is an interesting feature because it is also predicted by the measurements of Fukuta and Takahashi. However, in our measurements the significance of this increase is not large enough.

We added an explanation to the text: "Actually, the increase of Doppler velocities in the temperature interval between -5 and 0°C bin is also found in Fukuta (1999). However, the uncertainty of the measurements in this temperature interval is too large for a definite identification of this phenomenon."

Figure 8: Is it possible that different temperature bins have been used in Figure 8a and c (b and d)?

All temperature bins are 2.5°C and we indeed found that they were slightly shifted (by about 1K). We recalculated the values with the correct intervals, but significant changes in the mean values are not visible. There is also a basic difference between the two values: The white bars indicate the maxima of the histograms for each temperature bin and the lower ones show the mean values.

L 292/293: A 'future' approach published in 2010? Please rephrase.

Replaced "future" by "alternative"

L 298: If the IWC uncertainty is one magnitude, how can the uncertainty of IWC/LWC be one magnitude as well?

We agree that this formulation is wrong. We try to explain the errors of the corresponding measurements in more detail in Lines 329.

L 295 & 304: Isn't the statement in line 295 "Assuming that particles directly below the mixed-phase layer have the same properties as within the layer…" (i.e. the authors assume static conditions) in opposition to Line 304 "ice formation is a dynamic process"?

We agree that the term "dynamic process" creates some misunderstanding at this point and leave it out.

L 301: Do the authors really mean 'error'? maybe 'variability'?

We changed to "variability".

L 308: v -> v_CB?

Indeed, v and v_CB have been used in the manuscript inconsistently. We updated the manuscript accordingly.

Autors' response to reviewer #3: I noticed that the authors mentioned a minimum time span for a cloud case of 15 min, but according to the manuscript (L 155) it is 20 minutes.

I'm very sorry for this mistake: The selection criterion actually is 15 minutes. It has been mentioned in the manuscript several times, but one time wrongly. We checked the manuscript again for this error.

Other changes:

Fig. 9: "ILCR ratio" → "ILCR"

Line 307 → added "water content" to definition of ILCR