

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 #2

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The authors of the manuscript used the Cloudnet algorithm to analyze mixed phase clouds observed in Leipzig. I find the topic interesting and the article is in general worth to be published. However, I have some major comments which should be addressed. In particular, the authors have to address uncertainties more carefully and summarize their findings better so that they not oversell their results.

1 Major comments

Abstract: In the abstract, the sensitivity of space borne radars is discussed. However, this issue is mentioned only in Figure 7 and the last(!) sentence of the summary. When mentioning this in the abstract, the reader expects a much deeper discussion of that

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issue. How is this topic related to the key questions of the paper? I think the authors should either discuss that topic in greater detail in the paper or remove it from the abstract.

Section 4.1: My major concern is that Cloudnet's IWC estimations are used uncritically. Even though an uncertainty estimate is presented in L 167, the IWC results are likely biased and the authors should make that clear. How does that impact the authors' results and conclusions? What happens if other Z-IWC relations are used? From the spread of results when using other Z-IWC relations, is it possible to say something about the robustness of the key results of the study? Moreover, the authors should discuss in more detail why is there this drop around -10°C ? How can the authors distinguish between impact of particle type and number concentration? Can the authors exclude the possibility of cloud misclassification (i.e. the clouds are actually liquid)? Why is there no significant decrease for very cold temperatures when I would expect smaller particles as well?

Section 4.2: I cannot see that Figure 8 shows the 'necessity to select thin clouds'. I see that IWC is higher for lower temperatures, but how do the authors know that this is due to more non-pristine particles? I would actually recommend to omit Figure 8 (or explain better why its interesting), because the authors motivated the use of a maximum thickness of 350 m already well with references to other studies.

Section 4.3: What is the influence of vertical air motion? Did the authors correct for that?

Section 4.3: One of the key conclusions of the authors is that 'ice crystals formed in cloud layers with a geometrical thickness of less than 350m are mostly pristine when they fall out of the cloud' which they support by agreement of observations to literature studies. I think this is not supported by the study (except maybe the 0 to -10°C range). Even though they show indications for the presence of pristine particles, but they show not evidence that other particles are not present. How would non-pristine particles

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appear in the data with respect to LDR and fall velocity?

Section 4.4: How do the authors know that the median of LWP was around 20 gm⁻²? From the radiometer or from the adiabatic profiles? Are there differences in the statistic? Even though the general uncertainty of LWP retrievals is around 20 gm⁻², the uncertainty is actually way less if environmental conditions (mainly water vapor) are known. As far as I know, LWP estimation is not standardized in Cloudnet. But if LWP is bias corrected before the cloud (i.e constrain the water vapor), I expect the radiometer to be a better source for LWP than the adiabatic profiles. The adiabatic profiles are more an upper threshold for LWP. Moreover, your analysis of ILCR depends on uncertainties and biases of both LWC and IWC.

Section 4.5: As the authors note by themselves, this analysis is only for static conditions. I would propose that this can be seen from the nomenclature as well. I would suggest to call the lifetime index e.g. static lifetime index or potential lifetime index etc.

Summary:

L 303-305: I guess the authors refer to Figure 10? This is interpretation should have been discussed earlier in the manuscript. Moreover, it does not hold given the uncertainties of both LWP and IWC and that the authors show no quantitative evidence that IWC can be really estimated from LWP and CTT.

L 315: See comment to section 4.3

L 340-342: See above, I did not find any relation established and evaluated in this manuscript.

L 346-349: See also comment to abstract: In general, authors should avoid raising totally new issues in the summary as it is done here. Maybe, this part can be moved to the discussion of Figure 7? Is that actually a new, relevant result that pristine ice crystals have low backscattering cross sections and might be missed if the radar sensitivity is not sufficient?

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2 Minor comments

Title: I find the title of the article misleading. Even though a ratio between LWC and IWC is introduced, it is not discussed in detail.

Figure 6 and other: If the authors plot the lines on top of the dots, the figure would be much easier to read.

Figure 7a and other: What is indicated by the white boxes?

L. 93: Add Hatpro Reference

L. 98: Because the analysis depends on model temperature, what is the impact of this change on your analysis? Is there a bias between the models?

L. 118: add 'in Cloudnet below 0°C'

L. 120: I found this part hard to understand. Please motivate more clearly why you need this algorithm on top of Cloudnet and what it does

L. 124-128: What is the motivation for these thresholds?

L. 125: What is meant by 'driving microphysics'?

L. 139: I found it puzzling that CB refers to the cloud base of the mixed phase cloud and not of the complete cloud. What is actually the author's definition of a cloud? Are pristine ice particles already precipitation and not considered a cloud anymore? The same holds for Figure 2: 'signal below cloud layer' makes little sense if it is not clear that the mixed cloud layer is meant.

L. 151: At a typical radar range, what Ze value does -10 dB SNR correspond to?

L. 160: Please motivate thresholds already here.

L. 187: Is this equation valid for all radars? Where is IWC_thr in the plot?

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L. 192: Replace dramatically by e.g. significantly

L. 194: The threshold is black instead of red. In general, such descriptions should be indicated in the caption instead of the text.

Figure 1: My printer did not print the red ice part of the cloud which makes the figure very hard to understand.

Figure 4: Mention why other clouds are not considered.

L 222: Replace 'one has to keep in mind' with 'note'

L 224: Are the particles investigated here within this interval?

L 250: Isn't the fall velocity increased because these particles are more compact?

L 273: ')' missing

L 275: Remove actually

L 278: too colloquial: 'actually makes sense'

L 302: replace 'seem to be' with 'apparently'. 'seems to be' indicates something that is not true in reality.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-25, 2016.