Review of

Lidar-radar synergistic method to retrieve ice, supercooled and mixed-phase clouds properties

by Aubry et al.

General

In this study, a new, advanced algorithm is developed to classify mixed-phase clouds into liquid, mixed and fully glaciated clouds from remote sensing measurements. Further, microphysical and optical properties of the different phases can be retrieved from the measurements. This is an important step towards large-scale, detailed analysis of mixed-phase clouds, which have been difficult to detect but play an crucial role in cloud feedback to the climate.
I cannot express the innovation and importance of this work any better than Referee 3 and 2 have already done, so I like to say here only that I fully agree with them.

I also find the manuscript very well structured, fluently written and easy to understand. I am not an expert in remote sensing retrieval algorithms, but I was able to follow the explanations of the method and the innovations in it - but without being able to judge it well. Regarding the figures, I have some suggestions to make them easier to understand (see below the specific comments on the Figures).

There is only one more important point about which I have a question (see point 11 of the specific comments): the presented case study shows a cloud of about 1 km thickness. The information from lidar and radar together is only available in the upper half of the cloud, for the lower part there is no information from lidar.
Can satellite-borne lidar instruments generally only penetrate approx. 500 m deep into mixed phase clouds or is this determined by the thickness of the cloud in the upper part? Or, could thicker liquid clouds still be detected in the lower part, i.e. is only the lidar signal too weak in the present case?

All other points are minor and are listed in the specific comments. Overall, I recommend the manuscript for publication in AMT after minor revisions.

Specific comments

1) Page 11, line 219: *‚Whereas, the coefficient applied to the liquid water is different and set to 10, since the thickness of the detected liquid layer is smaller than ice layer*.‘

Is it generally the case that the thickness of the liquid layer is smaller than that of the ice layer?

2) Page 12, line 248ff: *‘For this study we use the following log-normal relationship defined by Frisch et al. (1995).’*

Why you use the oldest of the three available parameterizations?

3) Page 13, line 278: *‘Indeed, the radar is not used to retrieved the supercooled water neither in pure liquid clouds nor in mixed-phase clouds, ...’*

Typo.

4) Page 14, line 306f: *‘On the other hand, where there is no radar signal and a strong lidar backscatter, it is categorized as "supercooled water*" … ‘

What is meant with ‘ where there is no radar signal ‘ ? I think that means that the radar could in principle measure but there is no signal? But what if the conditions are such that the radar cannot measure but there would be a signal ? Is such a cloud misclassified ? Does this happen?

5) Page 16, line 343f: ‘...*note that the base of the supercooled liquid layer within the mixed-phased cloud cannot be determined unequivocally.’*

From Figure 2 c, it is also visible that from comparison with the in situ measurements the lowest part of the cloud is not detetcted with the radar – or is this an uncertainty caused by the unperfact match between in situ and satellite observation ?

6) Page 17, line 354f: ‘*Consequently, the CPI gives information about the ice particles and the FSSP about liquid droplets.*‘

The particles in the FSSP can also be 'secondary ice particles', which cannot be distinguished with the FSSP (see e.g. Costa et al. (2017). This should be mentionened here.

7) Page 17, line 355f: '.. *we take the ... ice water content* IWC_{CPI} *from the CPI, ...*'

What mass-dimension relationship have you used to calculate IWC_{CPI} ?
I found it a few lines later (line 361 - HC mass-size relationship), but would find it more appropriate here. And, can you explain why you used this one ?

8) Page 18, line 371f: 'T*able 7 presents the mean values in all selected pixels of all retrieved properties*.'

Why not include the in-situ mean values in the table, at least for the time periods where both in-situ and remote sensing measurements are available ? I think that would be useful.

9) Page 18, line 372ff: '*The extinction of liquid droplets is stronger than ice crystals by a factor of 7. The same trends is observed between LWC and IWC with average values 30 % larger for LWC. The ice crystals are larger than liquid droplets by a factor of 5 for the mean values. The liquid number concentration is much higher than ice number concentration by a factor* 10^3 .'

Should one see that from the figures? This would only be possible if you use the same color code in all panels (which is difficult, but not impossible), or at least the same limits in the color code scale (see also the comment b) on Figures 4, 5, right panels).

10) Page 20, line 381ff: see comment 5).

11) Page 20, line 392f: '*In these regions the FSSP detects liquid droplets while CALIOP signal cannot be used because of the attenuation (extinguished). This can explain why* α_{VarPy} *is lower than* $\alpha_{CPI+FSSP}$.'

I think that this effect deserves to be discussed in a little more detail, because this sounds as if liquid droplets in lower cloud layers are generally not detected. This raises the question of the limitations of the method in relation to the vertical extent of the cloud (see also genaral comment)?

However, the FSSP signal in this area is much weaker than in the mixed phase clouds above, so there are far fewer drops present. So a question is whether it would be possible to detect liquid drops with a concentration as high as in the mixed phase layer in the lower part of the cloud with the lidar?

Furthermore, in the article by Costa et al. (2017) – who classified mixed phase clouds based on airborne in situ measurements – it is shown in their Figure 8 that small cloud particles (up to 50 um, detetcted with a CAS instrument, which is similar to an FSSP) are still present even in completely glaciated clouds. This is also listed in their Table 6. It is not clear where these cloud particles come from, but the clouds are still classified as glaciated because the number of liquid droplets is so small (<

~0.1 cm^{-3}) that they cannot be considered a liquid cloud.. This could be discussed here to show that the new classification method is applicable.
By the way, it would be also interesting to see N_{liq} and N_{ice} from the in situ observations - then one could see whether the number of droplets is so small that they can hardly be called a cloud.

12) Would it be an idea to look for other in situ cases for comparison ? The data base of Costa et al. (2017) might provide the in situ observations. Maybe not for this paper, but for future work ?

Figures:

a) Figures 4, 5, 6: I recommend to change the order of the panels, liquid at the top and ice below, just like in the atmosphere – this is more intuitive and thus easier for the reader.

b) Figures 4, 5, right panels: I recommend using the same y-axis scales for all three panels, so that the differences between the panels (phases) are better visible.

References:

Costa, A., Meyer, J., Afchine, A., Luebke, A., Günther, G., Dorsey, J. R., Gallagher, M. W., Ehrlich, A., Wendisch, M., Baumgardner, D., Wex, H., and Krämer, M.: Classification of Arctic, midlatitude and tropical clouds in the mixed-phase temperature regime,
Atmos. Chem. Phys., 17, 12219–12238, https://doi.org/10.5194/acp-17-12219-2017, 2017.