Reply to Anonymous Referee #1

The authors like to thank anonymous referee #1 for his critical and constructive remarks that helped to improve our manuscript.

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Full title: An algorithm to retrieve ice water content profiles in cirrus clouds from the synergy of ground-based lidar and thermal infrared radiometer measurements

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This paper demonstrates an algorithm for ice water content profile retrievals from ground-based lidar and thermal infrared radiometer measurements on an optimal estimation framework. The bulk optical properties rely on a parameterization that links the optical properties with the ice water content and temperature. At first, the shortcoming of the retrieval method based on lidar alone measurements focusing on the uncertainty in the lidar ratio is demonstrated. Then, the authors show that the combined retrieval method based on lidar and thermal infrared measurements benefits the ice water content profile retrievals by reducing the uncertainty due to the lidar ratio. The computed optical thickness from retrieved ice water content profile generally similar for optimally moderately thick clouds and is underestimated compared to the counterparts based on the two-way transmissivity approach.

Overall, this paper is well written and well organized. The methods are sound. The topic presented in this paper is suitable to Atmospheric Measurement Techniques. I recommend the paper for publication, once the several points below have been taken care of.

General comments

The current manuscript contains several grammatical errors and redundant descriptions. I recommend the authors to proofread the manuscript again and encourage to make the redundant descriptions shorter. These treatments may help readers understand the contents.

Efforts have been made to correct grammatical errors and shorten redundant descriptions.

Minor comments

1. Pages 4–5: The authors should specify the following the instrumental characteristics: (1) The lidar pointing zenith angle; and (2) the FOV of the thermal infrared radiometer. If the FOV is not small enough, does the inhomogeneity of the response function along with viewing zenith angles affects the uncertainty in thermal infrared radiometric signals?

In this study, the lidar was pointing directly vertical with a zenith angle of 0°. The FOV of the thermal infrared radiometer is 3.5°. This information has been added to the text.
Hence, the FOV of the radiometer is larger than the FOV of the lidar (55 µrad). Therefore, the two instruments do not see exactly the same cloud area. This difference also depends on the altitude of the cloud. As in almost all remote sensing algorithms, we have assumed a homogeneous cloud in the instrument FOV and did not take into account any uncertainty due to sub-pixel heterogeneity. This assumption has been added to the text.
Furthermore, the information about the ground-based version of the TIR radiometer has been removed since it is irrelevant for this study.

2. Pages 7–8 “These two parameters are obtained for each cloud…”: These two paragraphs are confusing. The first paragraph mentions that the single scattering albedo and the phase function are obtained from the ice model introduced by Baran and Labonnote (2007). However, the second paragraph mentions that Vidot et al. (2015) parameterization is used to link the ice water content with several parameters including the extinction coefficient, single scattering albedo, and asymmetry parameter based on Baran et al. (2001), which is inconsistent with Baran and Labonnote (2007). I'm not sure if I understand these two paragraphs correctly. The inconsistent optical properties may arise an uncertainty, and the authors should clarify this. In addition, please make the first paragraph shorter. In the paragraph, although the authors introduce many parameterizations regarding ice properties, the paper only use the BV2015 parameterization.

Thank you for this remark. We tried to explain the microphysical in detail which might have resulted in a confusing description. Indeed, the optical properties are obtained from the Vidot et al (2015) parametrization. However, this parametrization is based on the microphysical model of Baran and Labonnote (2007) so there is no inconsistency here. Efforts have been made to shorten this paragraph and give the important information more directly to help the reader to understand the underlying microphysical assumptions of our methodology.

3. Page 9, Equation 8: Should “<<” be “≤” or “<”?

The symbol “<<” was chosen to indicate that we stop the iteration when the left-hand side of Eq. 8 is much smaller than the size of the measurement vector. However, it has been changed to “<” in the manuscript.

4. Page 10: Please add the descriptions about the Jacobian K if it is the case that opaque cirrus clouds (lidar signals cannot reach to cloud top) are present. You may not have $\partial F_{j\_top}/\partial IWC_{j\_top}$.

If the lidar signal is completely attenuated, the size of the measurement vector and consequently the size of the state vector are reduced. In this case, only the altitudes until full attenuation of the lidar signal are considered. Thus, the Jacobian is also reduced and contains

N_att lines (and columns) where N_att is the number of levels until the level of full attenuation. This information has been included in Sect. 3.2.
However, for the case study presented in this paper this case is less relevant since the lidar signal reached the cloud top for almost all treated profiles. Only in the end of the considered period around 19.8 UTC the signal was completely attenuated. For the according profiles, the lidar only algorithm as well as the synergy algorithm do not converge. This could be related to the reduced Jacobian but we believe that this non-convergence is more likely due to physical reasons since the cloud base at this time was located in low altitudes (around 6 km) and the temperature was rather warm for a cirrus cloud (between 245 and 250 K). In this temperature range, the presence of supercooled liquid droplets is possible which is not included in the BV2015 microphysical model. Hence, this model does probably not represent the optical properties of this cloud accurately enough. In order to draw more sophisticated conclusions, more cases have to be analyzed where the lidar profile was completely attenuated.

5. Page 19, Line 4 “the COT decreases and with it the simulated radiances”: Does this include a typo? Could you rephrase it?

What we wanted to say is that the COT decreases which causes the simulated radiances to decrease as well. The sentence has been rephrased.

6. Page 27 Figure 12 (e): Could you please reconsider the colors for the measurements? It is hard to recognize these plots.

The colors for the measurements in plot 12e have been changed in order to increase the readability of this plot.

7. Page 28, Lines 7–19: It is unfair to compare different qualities (i.e., COT and an effective COT). Since the author assumes the multiple scattering factor to be 0.75 for ice clouds throughout the paper, you can compare COT from the combined method with COT converted from an effective COT. In Figure 12c, although the effective COT is smaller than actual COT by 33% (if the multiple scattering factor = 0.75), I notice that the uncertainty due to multiple scattering factor cannot fully explain the underestimated COT from the combined method, particularly during UTC 16–17. The estimated lidar ratios are in the reasonable range (i.e., 20–40 sr) during the period. However, the effective COT from the two-way method is larger than the COT from the combined method by a factor of 2–3, and the multiple scattering factor of 0.3–0.5, which would compensate for the large effective COT, is unrealistic for ground-based lidar measurements. Therefore, I suggest the authors to add discussions in the paragraph regarding other potential sources that cause underestimated COT from the combined method. It may be good to mention a potential bias in the thermal infrared radiometer due to temperature.

Thank you for this remark. In fact, the COT from the transmission method plotted in Fig. 12c has already been corrected by the multiple scattering factor as mentioned in Sect. 4.2 (p. 28, lines 10-12 (page and line numbers refer to the discussion paper): "… the COT from the transmission method reported in Fig. 12c has been divided by the assumed multiple scattering factor for ice clouds of $\eta = 0.75$ in order to be consistent with the retrievals from the synergy algorithm which have been performed for $\eta = 0.75$ as well."). This phrase has been reformulated in order to help the reader to better understand. However, your remark that the assumed multiple scattering factor alone cannot explain the discrepancy between our synergy algorithm and the transmissivity approach is totally justified. Hence, we included a further discussion of this point:

"However, the multiple scattering factor alone cannot explain the inconsistency between the COT retrieved with the synergy algorithm and the COT derived from the transmission method. Another possible reason for this discrepancy may arise from the uncertainty in the transmission method itself because it depends on a good characterization of the molecular signal above the cloud and a good estimation of the cloud top altitude. These parameters are related with rather large uncertainties due to the quite noisy micro-pulse lidar signal in the high altitudes of cirrus clouds. Furthermore, the discrepancy between the two COTs could also originate from a potential bias in the TIR radiometer measurements due to an inaccurate temperature correction as mentioned in Sect. 4.1, or from a potential bias in the TIR forward model due to an inaccurate description of the atmospheric water vapor profile since the TIR radiometer measurements are very sensitive to water vapor (Dubuisson et al., 2008). Finally, the difference in the COTs from the synergy algorithm and the transmission method could also originate from the microphysical model which might not be perfect. The extinction at the lidar wavelength, which is calculated based on the IWP constrained by the TIR radiometer measurements, could be slightly underestimated. Figure 12c shows that if the IWP is larger, the difference between the COTs from the two methods becomes smaller. This can be explained by the fact that the contribution of the water vapor in the TIR radiometer measurements is more important for thin clouds than for thick clouds leading to an underestimation of the IWP and consequently an underestimation of the extinction, especially in case of thin cirrus clouds."