Reply to Anonymous Referee #2

The authors like to thank anonymous referee #2 for his detailed remarks and corrections of the manuscript that helped to improve the readability of our paper.

General Comments

To be brief, I agree with Reviewer 1's assessment. In particular, with their last point 7. To this point, I would further note that I could not find the fov of the lidar used nor the laser divergence. Such parameters should be listed as they directly impact the magnitude of the multiple-scattering effects in the lidar signals.

To play devil's advocate, it could be supposed that the important results presented in this paper all depend on the accuracy of the microphysical model used in the optimal estimation retrieval. Specifically, if the extinction at the lidar wavelength for a given IWC predicted by the model is inaccurate then the inversion results will be inaccurate, leading to wrong values of the retrieved lidar-ratio. However, the authors should point out that this is not quite the case. What is important is the combined lidar + radiometer retrieval is the relationship between the TIR absorption optical depth profile and the lidar extinction profile implied by the microphysical model. One of Platt's earlier papers discussed this important relationship. This relationship is expected to be robust for a large range of particle shapes and sizes. So I believe the results presented in this paper are likely at least robust with respect to inaccuracies in the microphysical model (at least with respect to the relationship between the extinction, IWC and temperature). This point should be covered in the discussion section of the paper and likely mentioned in the conclusions.

Thank you for this comment. We agree that the discussion of the microphysical model could have been more detailed in our discussion paper. Hence, we added the following paragraph to the conclusion section (see also the paragraph below that has been added to Sect 3.2 dealing with the error characterization of the microphysical model which is related to this point):

"It is important to note that the same microphysical model has been used to compute the bulk ice optical properties (i.e. the scattering and absorption coefficients as well as the asymmetry parameter and the phase function) for all wavelengths considered in this study. The consistency of this microphysical model over a large portion of the electromagnetic spectrum ranging from the visible to the infrared has been tested in numerous studies. Nevertheless, the parametrization of these optical properties as a function of IWC and temperature may introduce some uncertainty. However, a personal communication from A. J. Baran suggests that the error introduced by such a parametrization is rather small (smaller than 5%). Thus, we believe that the results presented in this paper are robust and mainly point out the misrepresentation of the phase function in the exact backscattering direction, which is a key result of this study."

Specific Comments (page and line numbers refer to the discussion paper)

p. 2, lines 28-29:
"Nowadays, it is common to operate lidar systems from space as well as from the ground and our method should be applicable to all types of lidar."
Somewhat vague sentence. Please re-write. It would be better to be more direct. e.g. The algorithm presented here should be applicable to combined TIR and simple backscatter lidar measurements, included space-based observations.

Thank you for this suggestion, the sentence has been re-written:
"Our method should be applicable to combined TIR and simple backscatter lidar measurements from ground-based as well as space-based observations."

p. 4, line 2:
"dispose of" means "get rid of". I do not think you want to say this here.

What we wanted to say is "possess", it has been corrected.

p. 4, lines 27-32:
Since you later consider multiple-scattering effect, it would be useful to state the laser divergence and the fov of the lidar receiver somewhere in this section.

You are right, this information has been missing and was added to the paragraph:
"The divergence of the laser beam as well as the field of view (FOV) of the receiver are both 55 μrad."

p. 9, line 7:

What do you do if noise causes the power to be zero or negative?

First of all, we apply a binomial filter to the measured lidar profiles to smooth the signal. If the power still becomes zero or negative, the profile is treated until the altitude just below the first occurrence of a negative power. In general, this only occurs above the cloud top but if this is happening in lower levels, the data is considered to be of insufficient quality and the profiles are excluded (this was never the case for the considered time period in this study). In any case, we stop the retrieval at the latest 500 m above the retrieved cloud top as mentioned on p. 14, lines 7-8.

p. 11, line 20:

Here you are calculating Se assuming that the extinction-vs-IWC relationship from the microphysical model is perfect. i.e. you are NOT taking into account this component of the model error. This is OK. However, you should make it clear in the discussion that you are neglecting this component and why you are doing this (too complex ?, errors in microphysical model not known etc.. ?)

Indeed, we are currently not taking into account an error related to the microphysical ice cloud model. This error is important but it is also difficult to quantify. We plan to evaluate it for future versions of our algorithm. For this paper, the following paragraph has been added to the discussion of the variance-covariance matrix:

"It should be noted that the characterization of the errors related to the BV2015 parametrization is very challenging. Thus, no error for the microphysical model is currently taken into account. However, this issue needs to be addressed in future studies and an evaluation of the uncertainty arising from the Vidot et al. (2015) parametrization, which has to be integrated in future versions of our algorithm, is planned. This evaluation could be performed by comparing the single scattering properties calculated directly from the ensemble model of Baran and Labonnote (2007) with the results from the Vidot et al. (2015) parametrization. Unfortunately, this is very costly to realize for all couples of IWC and temperature, particularly since the parametrization has not been developed by us. Furthermore, the uncertainty arising from this parametrization is assumed to be smaller than 5% (A. J. Baran, personal communication). Consequently, the present study neglects an error related to the microphysical model."

p. 17, line 13:

Is this not the other way around. That the retrieved integrated IWC depends strongly on the backscatter-to-extinction ratio?

Yes, you are right. It is indeed the other way around and has been corrected in the text.

p. 19, line 3:
"which is related to a correction factor for the phase function in backscattering direction"
Unnecessary. You have explained this numerous times before.

Okay, this half-sentence has been removed.