Response to D. P. Donovan (Referee #2)

The reviewer's comments are in black and our answers are in blue. Snippets of text from the submitted manuscript are in italics while modifications of the manuscript are shown in bold italics. The pages and lines reported here correspond to the submitted manuscript.

This paper describes a novel method to determine (thin) ice cloud optical depth as well as some limited mean particle size information using a combination of lidar data, IR radiometry and atmospheric thermodynamic state information. The technique appears robust and appears reasonably easy to implement. It could also be a candidate for network deployment.
The paper is, in the main, clear and well-written and worthy of publication. Another reviewer has already noted a number of issues. I have noted a further few aspects that should be improved before final acceptance.

We would like to thank Dave Donovan for his pertinent and informative comments. We provide below a point-by-point reply to his comments.

P1: line 1: What type of profile information? Please be specific here.

We have completed the missing information. P1 L1: *Multi-band downwelling thermal measurements of zenith sky radiance, along with* ***cloud boundary heights*** *~~height profile information~~, ...*

P11: Lines 25-28: The authors should expand the lidar multiple-scattering discussion. It is not quite satisfying/convincing to me. I agree that the application of Eloranta's formalism is appropriate, (it would be useful if they specified the equation number of the formula they used) however, they must follow through to discuss the errors in terms of optical depth and not leave the discussion solely in terms of Pt/P1. Also, the discussion must be made much clearer.

For example, (looking at Eq. 4) it is clear that the important factor in determining the COD is the ratio of the signals at cloud base and cloud top. Thus, the relevant quantity for determining the effect of MS is the Pt/P1 ratio at cloud top (since at cloud-base Pt=0). However, curiously, the worst-case cloud-base height is defined as 5.5 km but the cloud top altitude is not specified.

Assuming that the worst-case Pt/P1 ratio value of 60% quoted by the authors is indeed the value at cloud-top. The error in COD induced by MS effects for the worst-case scenario can be easily calculated. Using Eq.4 it is easy to show that the effect of MS will be to lower the retrieved extinction by an amount given by
dCOD_ms = -0.5 log (1.0+Pt/P1)
and Pt=0.6 P1 implies that dCOD_ms= 0.23 which is about a -10% bias.

Moving on, it is not clear what the authors mean by the "overall average value for the Pt/P1 upper limit". Is that the altitude averaged value for the just described "worst-case" scenario or the average ratio at cloud-top over the investigedcases ?If the former, then it is not a useful quantity. If the latter then that would correspond to a COD retrieval bias of -0.05 which could be significant for many of the results presented in this paper (e.g. see Fig .6 before about 19:00). Indeed accounting for the MS effects may bring the IR radiometer and lidar results more into line with each other values.

The authors have enough information to define the COD, cloud geometry and particle size ranges they are dealing with. Using this information and Eloranta's model I think with not too much effort they could define and apply a mean eta factor as was done in the paper by Platt which they reference.

Multiple-scattering (MS) is an important question and we agree that this part could be improved. The former approach (using the formula (16) in Eloranta (1998)) has been improved by using his model for all the cases (Figure 1). As Dr. Donovan correctly wrote, the MS is maximum at cloud top height (or at least, very close to top height depending on the backscatter profile) and we changed it in the text. We also pursued this study to estimate the MS coefficient, as defined in Platt (1973), to take account of the MS effects. We hope that the additional statements will help the readers to understand the real but limited (less than 10 %) impact of MS in the present cloud study.

Due to a very small angular field-of-view of the AHSRL receiver (45 µrad) it is common to assume the molecular backscatter cross section is not affected by multiple scattered photons (Eloranta et al., 2007). ***However to better quantify the effect of multiple scattering, we applied a practical model for the calculation of multiply scattered lidar returns, developed by Eloranta (1998) to the 150 cloud cases (not shown here). The inputs included molecular and particular backscatters, effective diameters (inferred from AHSRL+MMCR technique) and cloud height boundaries. The practical model computed the signal for all orders of multiple scattering but we were limited to the 4th order due to computation time. The impact of MS can then be evaluated in term of COD, as the multiple scattering lowers the retrieved extinction. From Equation (4), we define ΔCOD_ms as :***

$\Delta COD\_ms = -0.5 \log (1.0+Pt/P1)$

where Pt/P1 is the signal for all orders of multiple scattering over the single scattering return, and is an output of the MS model. Lastly, this term can be used to correct the retrieved optical depth by inserting a multiplicative factor η to model the reduction of the extinction coefficient, as used in Platt (1973). The parameter η is not constant (Bissonnette, 2005) and Platt argued that it should vary between 0.5 and 1. A linear fit would lead to the conclusion that in this present study, η equals 0.95. From here, this factor has been used to correct COD retrieved by lidar in the rest of the manuscript.

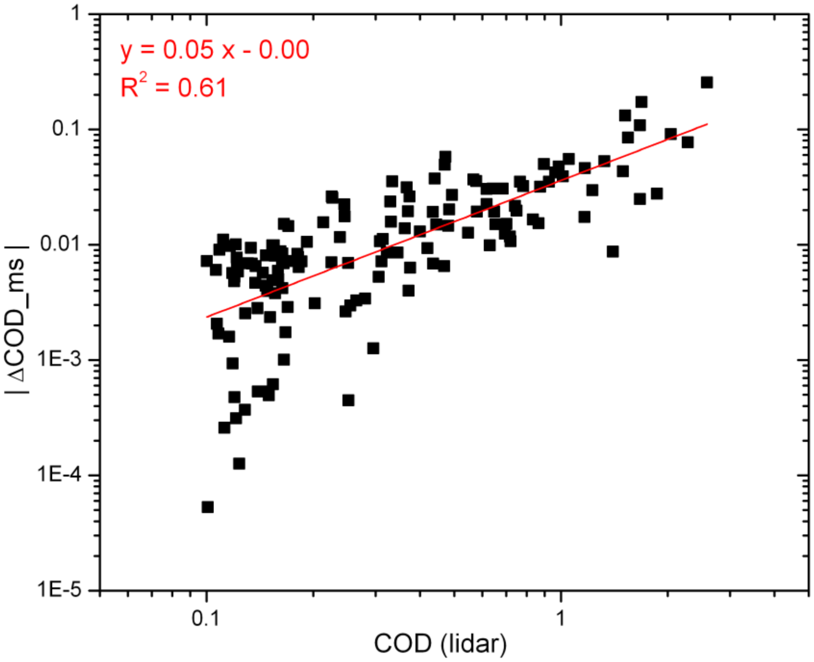

Figure 1: Difference in COD due to multiple scattering, according to the calculations of Eloranta's model (1998). The black squares represent the 150 cases selected in this study.

Page 20: Line 5-10: Can the authors please comment on how realistic it is to assume that all the measurement errors on the IR BTs are indeed independent?

From an error analysis of a very similar instrument (we used a newer version with 2 additional channels), in Legrand et al. (2000), we cannot state that the measurement errors are wavelength-independent. On the contrary, it seems that the global uncertainties for each channel depend on the detector temperature (Legrand et al, 2000; Brogniez et al., 2003).
We understand that our choice of assuming uncorrelated noise is simplistic but this assumption is done in numerous studies (e.g. Daniel et al., 2003; Turner, 2003; Sourdeval et al., 2013; Köhler et al., 2015; Sourdeval et al., 2016). We assume that the correlated errors would be reduced when the cloud dominates the measured signal (as the measured Tb is strongly linked to COD).
Some corrections can also be done to reduce noise level and therefore to minimize the off-diagonal elements of the covariance matrix. For example, a noise filter using principal component analysis was applied to P-AERI data in this study to reduce uncorrelated error (Turner et al., 2006).

Page 20: Line 14: sigma/sqrt(1000). What is the significance of the sqrtterm ?

The goal of applying the sqrt term was to have the same order of magnitude in the total error covariance matrix. By applying this term, we ensure to keep the spectral variation of the forward model errors.

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