I would like to thank Dr. Tesche for his thorough review of my initially submitted manuscript. The paper is immeasurably better because of his effort. I have addressed each of the suggestions in the manuscript and I provided written responses below.

Major comments:

- **Structure**: I suggest to restructure the manuscript to make it easier for readers to follow the process of development, application, and evaluation. For instance, I suggest to introduce all considered measurements as well as the data comparison strategy before the new method is described. At least for me, knowing what data will or might be used really supports the thought process. It also enables a much more straightforward presentation of the findings later in the manuscript. Here is a potential structure:

Reply: I have taken the reviewers comments seriously and restructured the manuscript largely in accordance with this Reviewers suggestion although I did not follow it verbatim. I did add an introductory paragraph as suggested that will hopefully allow the readers to better know what instruments and data are being used and how we will combine them and why. I have also restructured the results section as suggested. I think that this new organization makes good sense and I appreciate this Reviewer's suggestion.

- **Figures:** Rather than using figure and panel titles, I suggest to provide a full description of a figure in the figure caption. This includes what's shown in the different panels (and in which colour), the time and location of measurements, and the source if measurements are shown.

Reply: We have reworked the figure captions as suggested.

- **Derivation of Eq. (9):**

Reply: I am very appreciative of the time this Reviewer took in going through the math. I've made all the changes suggested after finding my transposition errors from my handwritten notes. I have numbered all relevant equations. Instead of including an appendix, I decided to just include the detailed derivation of what was equation 9 in the body of the text. I think this makes more sense and allows a reader to follow the derivation easily.

- **Measurement uncertainty:** I am trying to wrap my head around the reasoning in the lidar range-gate spacing and the subsequent discussion of Figure 3. I understand that finer range resolution allows for a better quantification of rmax as it provides to better resolve where exactly the lidar signal becomes saturated. If I have a common range bin of 15m and my nominal height is at the bin centre, wouldn't it mean that my range uncertainty is 7.5m rather than the full 15m? I wonder if measurements capabilities are actually a factor 2 better than currently accounted for? After the discussion of Figure 3, it is concluded that the rmax as inferable from the lidar measurements is insufficient to retrieve Nd and re and that the use of additional information as in the optimal inversion algorithm could compensate for this lack. Later, all presented results are inferred by the OE algorithm. I

wonder what the results would look like if the author had used the lidar-derived rmax. Could you please comment or provide an example?

Reply: The discussion presented by the reviewer is correct. In calculating the uncertainty that led to equation 3 I used a Gaussian PDF that had 1 sigma values of 1, 5, 15 and 30. That would correspond to range bin spacing of twicer the 1 sigma values. I have corrected the notation and the discussion throughout the text. While the results are more optimisitc, the magnitude of the uncertainty for range bins spacing of 10-20 m increases from 55% to 130%. Most operational lidars would have factor of two uncertainties in Nd which is marginally informative. Regarding the second half of this comment, I did run the direct calculation of Nd when I ran the OE. The results were noiser but seemed statistically similar. This is expected since Rmax is the primary source of information. Note also that I've changed the notation for range in the revision. I now use a capital R to differentiate it from effective radius.

- **Verification:** I suggest to restructure the verification section of the paper. The airborne in-situ measurements allow for a more direct comparison then the MODIS observations. I would therefore discuss those (they would already be introduced in Section 2) first in the detailed case study (currently lines 374 - 386), continue with the addition of MODIS observations during the case study period (currently lines 358 - 373), and conclude with the comparison to the other coinciding MODIS overpasses (currently lines 387 onward).

Reply: I did restructure the Results section of the paper – perhaps not precisely as suggested here but I took the Reviewer's comment seriously and moved the case study discussion from its own section into the section on the case study comparison/validation.

- **Bigger picture**: I find this to be an excellent paper that could be even better if the author could sort the new method into the bigger picture of cloud remote sensing. You outline what kind of measurements are needed. Maybe you could also provide a check list on ranges of cloud properties for which you would assume the method to be valid? Is it just shallow liquid water clouds that don't precipitate too heavily? Are there other regions or established measurement sites for which the method could be applied? Would such an application require a revision of the OE algorithm? Which knowledge gaps could be closed by a widespread application of the new method?

Reply: I added a paragraph to the conclusions section addressing many of these questions. I avoided a few of them such as the last one about wide-spread application. Obviously as the developer, I'd like to see the method applied widely since I developed it to be easily used. However, before I start pushing this idea, I'd like to mature the algorithm more and see if it holds up against wider tests. It is certainly not a panacea given the limited information we have in the measurements to retrieve Nd. I honestly think this type of method is about the best we can do but only more critical testing will tell if it is sufficient.

Specific Comments:

- line 174: please give all normalised standard deviations in either percent or without units

Reply: Addressed

- line 252: to wit?

Reply: I think my usage is correct. The phrase to wit is followed by an example of something that has already been said – according to the online sources I looked at. Here I'm providing the regression that I described in the previous part of the sentence. Admittedly, I'm not an expert writer but I think this is ok.

- equation for Kb: second row, third column should be $\partial\sigma/\partial\eta$

Reply: Fixed

- line 275: I suggest to start the section on the evaluation of the OE algorithm here as the text before is still on the functioning of the algorithm

Reply: Agree. Done.

- Tables 2 and 3: Is it possible to combine those table, maybe in a transposed form? I also suggest to include cases 2, 4, and 6 in Table 2 as this is what's stated in the text. Please also check the referencing between the tables if kept separated.

Reply: I combined Tables 2 and 3 into a single table as suggested.

- line 194: not clear how this is shown in Table 2

Reply: Table 1 shows the correlation coefficients among the off diagonal elements. We use that correlation coefficient and the variance of the quantities to calculate the covariance. Text is clarified.

- Figure 6: Maybe show plots for radar and lidar to 2 km height only?

Reply: Done

- Figure 7 and in the text: integers should suffice for Nd

Reply: Noted.

- line 397: please quantify what is meant with good correlation

Reply: correlation coefficients are now included as an inset in Figure 7.

- Table 4 could be moved to the appendix

Reply: I suppose so, but I think I'll choose to just keep it in the text.

- Figure 9: It's hard to be certain but I have the impression that there are more points in the plots than overpasses listed in Table 4...?

Reply: Thanks for catching. I did omit 2 overpasses from the table. They are now included. 14 points in the table and figures.