Dear Editor!

First of all, we thank reviewer #1 and reviewer #2 (and thus the editor) for taking the time to read the paper carefully and to provide us with a list of very valuable comments which helped us to improve the paper significantly. These remarks were necessary to open our eyes regarding the status of the submitted manuscript. We are convinced that the improvement was successful.

The paper is almost completely rewritten to meet all the points of the reviewers. We improved the introduction section and provide now an extended discussion on previous work on lidar-based INC retrievals. We discuss what the important and difficult points of the retrieval are, and describe why we are now motivated to present a straight forward method for the retrieval of INC profiles from lidar data. We kept the question in mind: what is new !

The method section (Sect. 3) is better organized. Together with the introduction, it should now be clear, what the different steps (part 1, part 2 , part 3) of the entire INC retrieval scheme are, and what we are going to present in the different detailed subsections 3.1 (part 1), 3.2 (part 2), and 3.3 (part 3) of the method section 3.

As a very new point, we include a second APC-to-INC parameterization (Eq.(3) in Sect.3.3) in our study. This parameterization is explicitely applicable to desert dust (DeMott et al., ACP, 2015). Now we can even present comparisons of results obtained with the aerosol-type-independent parameterization (DeMott et al., 2010) and obtained with the (new) dust-related parameterization (DeMott et al., 2015) in Sect. 4. INC estimation errors (standard deviations) are within a factor of 2 when using the dust-related INC parameterization scheme. So, at least, we can state that the uncertainties are no longer within a factor of 10 (one order of magnitude).

We reduced the number of figures of the result section, and now we use the best available temperature and pressure profiles (GDAS for Limassol, GMAO for CALIPSO flight track sites) in the analysis of ground-based and spaceborne lidar data, i.e, we use atmospheric profiles from numerical weather prediction models rather than sparse radiosonde profiles or Standard Atmosphere profiles of temperature and pressure.

So we revised all parts of the paper in deep detail. This is the main reason that we needed two months for all the improvements.

In the following, we go step by step through the detailed comments of the reviews. Our answers are given in bold.

Reviewer #1:

Abstract: Most of the article actually deals with the demonstration of the methodology using the ground-based lidar and only a small part with its application on CALIOP and its potential. Therefore I would suggest organizing the abstract accordingly to reflect this.

The abstract has been rearranged accordingly.

Pages 25752-25753. The introductory paragraph on the methodology needs rewriting, since as it is written does not help the reader to follow the general concept of the methodology, which is crucial to understand the subsequent sections 3.1 to 3.4. What is missing is a description of the concept before summarizing this in a table. In other words the authors should present clearly what is the concept,

what data are required to convert this to an applicable methodology, what data are available (e.g. lidar, sunphotometer, data from campaigns with pure dust) and how these have to be pre-processed. All this information is more or less included in sections 3.1 to 3.4. But these sections are rather lengthy discussing in detail the selected profiles and as consequence the general concept gets lost.

We rewrote the method section (Sect. 3), provide a brief step-by-step description of the INC retrieval scheme in the beginning of Sect. 3 (here we briefly explain part 1, part 2, part 3 of the retrieval) and provide then a straight forward description in Sect. 3.1 (part 1), Sect. 3.2 (part 2), and Sect. 3.3 (part 3). Furthermore the introduction section is extended and the way how to get INC from basic lidar observations is explained already here by going through the literature on lidar INC retrieval. We discuss the problems that have to be solved, and why we present this INC retrieval scheme now (what is new in this article). We hope the first part of the entire paper (Sects. 1 to 3) is no longer confusing and now easy to read.

Page 25753, lines 20-22. The authors should emphasize here that they investigate a relationship valid explicitly for dust and that's why they use data from certain campaigns. As written it might be misleading and the reader can conclude that such a relationship is valid for any aerosol type.

We kept this point always in mind when doing the revision of the entire article. That should be very clear now.

Page 25754, lines 20-28. Concerning AOT for Limassol in figure3 the light blue symbols correspond to coarse mode AOT and the dark blue to total AOT for the same period. Why these are not coincident and the latter has less and different points?

To avoid confusion, Fig.3 in the submitted version is now Fig.2 in the revised version. The numbers of points are equal, but we agree it is not easy to find all the points, one has always to look 'horizontally', i.e., for a given APC (y axis) there are always two AOT values (x axis). And some points are behind other bigger symbols. Sorry!

Page 25755, lins 1-7. This paragraph is confusing and needs some justification. What the authors suggest here is that the dust AOT is well mixed within the DLH and thus they can estimate from this a mean extinction coefficient and mean layer APC ? If this is the case, can this be verified by most of the measurements used?

Yes the dust layers were usually well mixed, and we mention that. But we think this not so important. The correlation between AOT and column APC(r>280nm) is of fundamental importance. The correlation in terms of dust extinction coefficient and APC(280) is presented in Fig.4 to provide numbers in terms of parameters which can be measured with lidar, and also to show that we covered a large range of the possible dust extinction values, and this at very different sites. All in all, the correlation between dust extinction and APC(280) in Fig.4 or between the respective column values in Fig. 2is always the same and thus robust and excellent.

In numbers: Disregarding the units and different orders of magnitudes, the slopes of the regression lines in Fig.2 (0.685, AERONET, column values) and in Fig.4 (0.673, extinction values, APC) are practically the same. This is now mentioned in the text. The small difference is caused by the fact that we included Cyprus data in the regression in Fig.2, and considered only Morocco, Cape Verde, and Barbados data in Fig.4.

Page 25756, lines 7-14. The authors mention that there are more appropriate parameterizations for INC concerning mineral dust (which is the case here) but they use the more general one from DeMott et al. What is the reason behind that? Do these methods require input data not available? A better justification should be provided. The authors should also make a comment on what has to be done to minimize this large uncertainty (an order of magnitude) and if estimates with such uncertainty are still useful for the modelers.

Please have a look into Sect. 3.3. This section is now completely new or better significantly updated. We include the latest state of INC parameterization published by DeMott et. al. (2015) just a month ago. Now, we can make use of two parameterization approaches: the global (aerosol-type-independent) IN parameterization (Eq.(2), DeMott et al., 2010, we used this Eq.(2) already in the submitted version of the paper) and the new mineral-dust-related parameterization (Eq.(3), DeMott et al., 2015). We show comparisons of the two parameterizations in the results section (Sect.4).

We (the authors) met Paul DeMott in Zurich in January (during an EU project workshop, BACCHUS) and discussed with him our results and findings, and our way to present these new results. And he agreed with our results and conclusions. So, Sect. 3 as well as Sect. 4 (results) are now largely improved, and even approved by Paul DeMott.

In the DeMott et al. paper of 2010 it is stated that the modeling community needs to know the IN concentrations within an order of magnitude. This is obviously sufficient for modelling. This is now stated in Sect. 3.2 (in the second paragraph after Eq.(3)). Just a comment here: If we keep the huge influence of temperature in mind (INC increases by one order of magnitude when the temperature increases by 5 C) we can imagine that an order of magnitude uncertainty in INC profiling is sufficient. What the modelers really need are rather accurate temperature fields and temperature changes at cloud level when clouds form and become radiatively active….

Page 25756, lines 23-26. The authors present an error budget and provide some values. They should provide some reference on that to justify them.

We try to provide error information and respective references in Sect.3. Good news is that the INC uncertainty range is now within a factor of 2 (standard error) when using the new INC scheme for dust after DeMott et al. (2015). So, the overall uncertainty is no longer a factor of 10.

Page 25757, lines 22-27. Figure 5 already highlights the impact of temperature for the same extinction coefficient. Figure 6 however can be misleading, since it suggests that it is only a matter of constant bias versus height, which is of course not true, since temperature is not constant with height. I would suggest removing this figure.

We removed Fig. 6. We rewrote the entire result section, and removed several figures. By the way, Fig.5 now shows APC and INC curves (for fixed temperatures) for both parameterizations.

Page 25758, discussion of Figure7. Why don't the authors use temperature profiles from radiosondes or a model valid for the day of measurements? Their methodology cannot be independent from the knowledge of the temperature profile. Otherwise the uncertainties would be always so large that the corresponding estimated INC would not be useful.

Now we switched completely from standard atmospheric assumptions or radiosonde data to GDAS data (numerical weather prediction data, for Limassol) and GMAO data (atmospheric modeling approach for CALIOP, all space lidar profiles are given together with GMAO temperature and pressure profiles). So this part is largely improved.

Page 25760. Discussion on Figure 12. Same comment as for Figure 6.

We removed Fig.12.

Section 4.2. Concerning the methodology and its applicability what is the value of presenting another example here? Again the message is that the temperature profile is the dominant driver

We simply want to show two case studies, one unique case with dust up to 10 km, and one for typical dust layering up to 5 km. The discussion of the second case is now strongly shortened. Only three figures are left (Fig.11: CALIPSO color plot, Fig.12: trajectory plot, Fig.13: APC, INC profiles).

We also want to show a Saharan dust outbreak and a Middle East dust outbreak. Another important point is that we can discuss the atmospheric (meteorological) consequences for heterogeneous ice formation better and in a complementary way by these two case studies. The first case study allows us to discuss convective cloud towers (up to great heights) and the large impact of changing temperatures when air parcels are moved vertically. The second case shows us that for typical dust layers (up to 4-5 km height) there is almost no height region where ice formation can take place (it is too warm, and only at the top of the dust layer ice formation may take place…). But if such dust layers are moved to the north (horizontal transport of air masses), and get colder and get lifted during the transport then they can develop a powerful strength regarding cloud glaciation.

Reviewer #2 (editor's comments):

This paper describes a methodology for estimating ice nuclei concentrations from polarisation lidar data. While on first reading this sounds like a revolutionary achievement it is not quite what it seems. The lidar data are used to estimate the number concentration of dust particles greater then 280 nm, and then a parameterisation from the literature is used to convert this to the concentration of ice nuclei. Of course, this method can only measure mineral dust ice nuclei, and the results are very uncertain indeed – around 60% on the dust particle concentration and a further order of magnitude in the conversion to ice nuclei. The title needs to reflect this uncertainty or people unfamiliar with lidar will believe that the global distribution of ice nucleus concentration can now be measured with reasonable accuracy (indeed, a cloud physics colleague of mine, on seeing the ACPD paper, rushed into my room to tell me exactly this). So a title like 'Estimated dust-related ….' would be a better reflection of the content of the paper. The first paragraph of the conclusions also needs toning down in view of the uncertainties in the INC parameterisation.

We improved the title and the first paragraph of the conclusion section.

To our opinion the paper has indeed a 'revolutionary' touch (at least for us, the authors, who work on this topic now for more than one year). And sure…, we want to attract people, to read the paper by choosing an attractive title.

This is the first time that such an INC retrieval attempt is shown in all detail (from basic lidar observations to final INC profiles). The applied INC parameterizations (after DeMott et al., 2010, 2015) are state-of-the-art. We applied our method to CALIOP data, and we show that CALIPSO can be used to estimate INC profile, and we show this for unique scenarios. From the atmospheric modeling point of view, INC profiles are strongly requested , so our research is relevant. Mineral dust is the most important ice nuclei reservoir around the globe because it is almost omnipresent (at least in the northern hemisphere). All these facts together make this paper to an exciting story, at least for us, the authors.

The introduction makes no reference to previous studies deriving INC from lidar data, yet in 3.4 we are told that the concept originated in Ansmann et al (2008) and was further discussed by Seifert et al (2011). A proper discussion of previous work should be provided in the introduction, and the advances presented in this paper identified and put in context. What's new here?

A proper discussion of previous work is now given in the introduction section (Sect. 1). In addition, in the first paragraph of Sect. 3 (method section) we again state the most important contribution of lidar in the INC retrieval procedure (estimation of APC(280) from dust extinction coefficient). The first three sections are largely improved and should be easy to read now. Confusing statements and bad arrangements of text pieces are hopefully completely removed.

Although the writing and diagrams are clear, and the argument is straightforward, I found the paper surprisingly difficult to follow. The first paragraph of 3.1 assumes that the reader has read the previous paper by Mamouri and Ansmann, but in fact there is enough information scattered about this paper to work out what is going on. The paper would be easier to read if this information were presented coherently at the beginning of 3.1. Crucial information (like the fact that assumed lidar ratios are used to convert backscatter to extinction) are contained in a figure caption; this should be part of the text. Results (fig 1) are presented before the method used to calculate them (the one-step and two-step methods). I don't really understand the significance of the two-step method – the results seems to agree well with the (conceptually-straightforward) one-step method so are they needed? A sentence or two should be included at the end of 3.1 explaining why the two-step method is needed for this paper (if indeed it is).

As mentioned before we spent considerable time and effort to improve the text (better structure, better and more clear explanations of the INC retrieval scheme, clear separation of the three main steps (part 1, part 2, part 3) etc. We start already in Sect. 1 to provide an overview of the different steps of the retrieval. We continue with this in Sect. 3 (first, introducing paragraph). We provide lidar ratio information in the text, we explain why we find and use 55 sr (Saharan dust, ground-based lidar) and 40 sr for Middle East dust, and that CALIOP use a lidar-ratio look up table, and this table only contains one lidar ratio for dust (40 sr).

We introduce Fig.1 in Sect.3.1 (when we need this figure, not earlier) and discuss the methods how we get the different parameters shown in Fig.1. We have to keep this discussion short! We have to say: please read the referenced literature, if you want more details. The retrieval of the backscatter and extinction coefficients and depolarization ratio is well known, and clearly not the topic of this paper.

Concerning the use of the one-step and the two-step method for dust and non-dust separation (part 1 of the INC retrieval, Sect. 3.1): Ok, this is a new aspect, nevertheless we have to keep the

discussion on the retrieval of optical properties short again, especially on the use of the one-step and two-step methods. However, we use both methods (one step, two step) because we want to stress that we concentrate on desert dust cases (Fig 1, 29 Sep 2011) and not on other dust types like fine-mode soil dust (Fig.1, 27 Sep 2011). In the simple case of desert dust, the results obtained with both methods agree. But this is not the case for fine-mode dust. So desert dust is more simple to analyze and the most important dust type. Afterwards (after Fig.1), in the residual parts of the paper, we only use the well-known one-step method now. In the submitted version, we used the two-step method in Sect.4. Now we completely changed to one-step results for the dust extinction profiles in Sect.4.

Section 3.2 is perhaps the heart of this paper, in that it converts the lidar extinction profiles to APC_{280}, which is a product of the sunphotometer retrieval that can be related to INC. The paper does not explain how the column-integrated APC_{280} is derived, nor does it properly discuss the uncertainty in this derivation, leaving the reader struggling to grasp the significance of this calculation. (At the end of 3.2 a value of 20% is quoted for the conversion of AOT to APC_{280} but this is not supported by an argument or by a reference to previous work). A linear relationship between the integrated lidar extinction and the column APC_{280} is not surprising – very crudely they are both measurements of the amount of desert dust – but the closeness of the relationship is remarkable. Fig 3 also illustrates nicely the value of polarisation lidar and the separation it allows of spherical and non-spherical backscatter. However, the slope depends on the assumptions behind the retrieval leading to fig.2, which should be better explained.

Again (to avoid confusion) first of all: Fig. 2 is now Fig. 3, and Fig. 3 is now extended..Fig. 3a shows the volume size distributions, Fig.3b shows the respective number size distribution. Fig. 3b is introduced to facilitate the explanations how we got the APC values.

We explain now how we calculate APC(280) in much more detail. Based on this Fig. 3b and the related discussion in Sect. 3.2 (part 2) it should be clear now how we get APC(280). We discuss the errors in more detail, including uncertainties introduced by using the AERONET inversion scheme to get the size distribution from the basic measurements of spectral optical thickness and sky radiance. We cite SAMUM papers which allow us to estimate the potential uncertainties in the AERONET inversion results (for the size distribution and the APC(280) value). The AERONET retrieval errors are believed to be in the range of 20% or less. However, we also state that there is no alternative to AERONET. No other approach (e.g., airborne insitu observations of microphysical and optical properties) would be trustworthy enough. This is simply the main message of all FALCON flights during the SAMUM campaigns and the SALTRACE field studies. We need simultaneous observations of microphysical and optical properties for undisturbed ambient conditions, and this is obviously only possible by means of remote sensing.

Maybe it's just me, but I couldn't understand why plotting AOT and APC_{280} normalised with dust layer heights (fig 4) gave a different result to fig 3, nor why fig 4 doesn't show any of the Limassol data. Perhaps this could be better explained.

As already discussed above: Disregarding the units and different orders of magnitudes , the slopes of the regression lines in the old Fig.3, now Fig.2 (0.685, AERONET, column values) and in Fig.4 (0.673, extinction values, APC) are practically equal. This is now mentioned in the text. The small difference is caused by the fact that we included Cyprus data in the regression in Fig.2, and considered only Morocco, Cape Verde, and Barbados data in Fig.4.

We only show Morocco, Cape Verde, and Barbados data in Fig.4 because only for these field campaigns we have dense lidar data sets and almost pure dust conditions. At Cyprus, there are always complex aerosol conditions.

On line 15 of p.25756 is a very important paragraph stating what this paper is really about. This paragraph needs to be at the beginning of the paper so the reader understands where it is going.

Yes, we emphasis this point (APC(280) retrieval from dust extinction coefficient) already in the introduction (Sect. 1) now when we discuss previous work in the field of lidar-based INC retrieval.

Fig 10 – why have you applied a different lidar ratio to the two datasets?

We extended the discussion on the used lidar ratio selection at several places in the paper (not only in the figure captions) and provide references . The point is here that the lidar ratio for Saharan dust (as measured during the SAMUM campaigns, Morocco, Cape Verde, Barbados) is 50-60 sr (for illite-rich dust particles from the western part of the Sahara). A high illite fraction causes a comparably low real part of the refractive index which finally leads to comparably high lidar ratios. This context is explained in detail by Schuster et al. (2012), and also given in Mamouri et al. (2013). For the eastern Sahara and Middle East deserts, the illite amount of the dust is rather low (less than 10%), the real part of the refractive index is comparably high so that at the end the dust lidar ratios are between 35-45 sr for Middle East dust (Schuster et al., 2012, Mamouri et al., 2013).

Finally, a couple of small typos:

p.25754 l. 7 – radii (not radiis)

We improved that, but changed the text part in general…

p.25763 l.20 – extent (not extend)

Improved.