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Interactive comment on "Dust-related ice nuclei profiles from polarization lidar: methodology and case studies" *by* R. E. Mamouri and A. Ansmann

Anonymous Referee #1

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Review of the paper by Mamouri and Ansmann,

The paper demonstrates a methodology to estimate profiles of ice-nuclei related to dust, using the synergy of a ground-based polarization lidar and a sunphotometer. Then the authors examine the applicability of the methodology on CALIOP data. The methodology applied is actually an adaption and optimization of existing techniques to lidar data, utilizing in parallel the output from earlier dedicated lidar field campaigns, which aimed in the characterization of dust particles. The methodology presented, if applied on a global scale, will be valuable to improve our understanding on heterogeneous ice formation in the presence of dust and thus will assist the modeling community

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to describe better these processes in atmospheric circulation models. Therefore the paper is suitable for ACP and should be considered for publication, after considering my comments in a revised version.

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.

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.

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.

Page 25754, lines 20-28. Concerning AOT for Limassol in figure 3 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?

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

ACPD

14, C9686–C9688, 2014

is the case, can this be verified by most of the measurements used?

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.

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.

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.

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.

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25747, 2014.