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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Injection of mineral dust into the free troposphere during fire events observed with polarization lidar at Limassol, Cyprus" *by* A. Nisantzi et al.

Anonymous Referee #2

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This paper deals with the soil dust content, in lofted fire smoke plumes advected from Turkey and observed with a polarization lidar in Cyprus (Limassol), member of EARLINET. The paper rightly acknowledges previously reported statistical studies, and the methodology based on the sun/sky photometer-lidar synergy, for retrieving advanced aerosol properties (mass concentration, lidar ratio etc.). The paper is well written, with high scientific interest, worth being published in Atmospheric Chemistry and Physics, scientific journal and in order to be improved, I would suggest to the authors to take into consideration the following comments.

Minor Comments: 1. In Figure 4, for the identified lofted aerosol layer, some mean

values of the aerosol optical depth and fine mode fraction are given, with a standard deviation of zero. Please correct this. The authors are also suggested to use the same precision (concerning the same aerosol properties) for the values reported in Figures 2, 4, 10 and 11. In addition, it is also recommended to use standard deviation values for all the atmospheric properties that are retrieved/calculated, and declare (maybe with a color) the ones used as constants or assumed (for example SFT/PBL).

2. In Figure 6 an overview of the geometrical properties of the aerosol layers is demonstrated. I wonder if the authors during their research observed any seasonal pattern concerning the detection of smoke and smoke free aerosol layers reaching Limassol from greater Turkish area. If so, this could be also demonstrated, integrating their climatological study.

3. In Figure 7 the time series of the derived layer mean PDR values is given. It would be clearer to the reader if the corresponding standard deviation values were shown here. Without those error bars the "threshold" line on this figure seems totally arbitrary. The authors are suggested to make some comments on the 4 high values (PDR higher than 10%) observed at the smoke free cluster. The "smoke free" cases (blue circles) have larger variability than the corresponding smoke cases. Is there any sufficient explanation on this fact?

4. In Figure 9 the derived layer mean PDR values (for smoke and smoke free cases) are given in respect to the air mass travel time (estimated from HYSPLIT), along with values already reported in the literature. The author is mentioning that the 10 of the 21 smoke free cases are actually cases influenced by smoke generated in areas north of the Black Sea, the smoke free annotation in the figure's legend have to be deleted. Thus, for reasons of clarity, it is recommended to the authors to use one color for their observational values, since all of them are cases that air masses are influenced by fires. In addition, an exponential fit to the whole data set can be performed in order to have a first approximation of the corresponding PDR values that are going to be reported in the future.

5. Page 5 line 413-415: The authors are giving the following explanation for the scattered data of Figure 9, “Finally the nearby deserts .... dust load”. To the reviewer, it was not clear through the manuscript, that those cases (cases of free tropospheric dust load contribution, from Middle East and North Africa desert areas) where not excluded with the backward trajectory analysis, performed by the authors. This should be clarified better in the text.

6. In Figures 10 and 11 the aerosol backscatter coefficient is given until ground (without taking a constant value from the overlap height and downwards) in contrast to the PDR which has constant value from approximately 300m. Since the overlap effect is strongly eliminated due to the signal ratio, how this would be possible? In addition, in Figures 1, 2, and 4, the aerosol backscatter and linear depolarization profiles are given from 300 m a.s.l.. Below this height range no values are demonstrated. It is suggested to the authors to keep the same pattern of demonstrating their scientific results concerning the vertical profiles below overlap height, for all the figures given out in this study.

7. In Figure 11, at the height range of 2 to 3 km, a mean PDR value of 10% is shown, indicating an aerosol layer, something that is not demonstrated from the corresponding aerosol backscatter and extinction profiles. If this aerosol layer is artificially observed, due to low signal to noise ratio lidar capability, and was not included in the analysis, is something that have to be denoted clearly.

8. The authors have used the required volume to extinction conversion factors from Ansmann et al., 2012, whose reported values concerns only coarse and fine aerosol modes, observed for different aerosol types at different locations and time periods. The corresponding values reported by the authors on Page 5, line 457, includes also fine dust particles, and with no clear evidence that those values were also observed (and/or found to be comparable) with the ones retrieved by the sun/sky photometer in Limassol. In any case, the authors are suggested to add some text on this part of the manuscript, for being more descriptive and accurate.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C5307/2014/acpd-14-C5307-2014-supplement.pdf

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