Dear ACP-Editor,

we thank the two reviewers for careful reading of the manuscript and their comments. We improved the paper along their suggestions. Our answers to the comments (in bold) are given, step by step.

First of all, we included the reference Amiridis et al. (Smoke injection heights from agricultural burning in eastern Europe as seen by CALIPSO, ACP, 2010) in the introduction, and later on, we compare the injection heights (smoke top heights) of Amiridis et al. with ours in Sect. 3. Good agreement is given..

REFEREE 1

MODIS hot spots are provided with a metric of confidence on their detection. A high value on this parameter (e.g. >80%) would enhance the confidence and facilitate the interpretation of the lidar measurements reported by the authors.

For the retrievals we have used the confidence level of 80%. We have excluded the cases where the confidence level is smaller than 80%, we used only the cases with confidence level greater than 81%. We state that now in Sect. 2.5

The trajectories in Figures 3 and 5: In Figure 3, the vertical pathway is plotted only for 2 height levels over Limassol, while in the 2D map, more trajectories are shown.

We improved both trajectory plots regarding colors and length (six-day backward trajectories now). Figure 3 contained already 6 height levels in the submitted version, but maybe there was a problem with the printout.

While this is considered as a smoke-free case, the trajectories indicate a possible advection from the Northern Black Sea region which may be observed in Limassol.

I would suggest that the authors should provide the complete information for the trajectories as well as the confidence level of the fire detection by MODIS in order to strengthen the characterization of each case. This should be done also for the second case (please revise also the trajectory Figure 5, there are 2 green lines that cannot be distinguished)

We mention the MODIS confidence levels now. We increased the lengths of the trajectories to six days back. We increased the MODIS fire integration time to seven days - in both figures. However, to our opinion, trajectories show the most probable pathways of air masses, but not the exact ones. And the MODIS fire spots indicate that there were fires during the specific period of interest, but they do not provide more detailed info. The trajectories may thus indicate that it is probable or less probable that air masses were influenced by fire smoke and dust. We avoid to discuss the trajectories in more detail. This concerns also the potential that other dust sources contributed (background dust, soil dust from arid regions, deserts further east of Turkey…).

Regarding the scatter in Figure 9: Winter/Summer cases may justify some outliers (winter cases are reported as shown in Figure 7). Soil dust is less likely to contribute to the measurements during winter due to most probably large values of soil moisture which decreases the dust emission.

Most of the northerly flows occur in summer. However, there are also some measurements in winter. We included in the discussion (Sect. 3) that soil dust emission is reduced in winter because of precipitation (wet deposition), wet soils, and snow cover (Turkish mountains, and north and east of the Black Sea).

Do the authors have an estimation of the depolarization uncertainty impact on their conclusions?

The overall uncertainty is <10%. The uncertainty of particle depolarization ratio PDR comes mainly from the systematic errors in the setup of lidar system. The main error sources originate from the depolarization calibration V* as well from the linear depolarization ratio of molecules δ^m. By using the derived error formula, the PDR values have a mean relative uncertainty at 532nm of 7.4%. Specifically for the four years measurements we have calculated the V* values (0.069±0.004) and by using the outliers of V*, the PDR relative uncertainty is 7.4%. Additionally the change of δ^m (0.013±0.002) by 15% yields a mean relative uncertainty 2%.

In Sect. 2.1, we mention the uncertainties in the volume depolarization ratio (<5%) and in the particle depolarization ratio (<10%).

REFEREE 2

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.

We improved this and give numbers with three decimal numbers.

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 3 decimal**. 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).**

We decided case by case. We do not like to provide 3 decimal numbers in general. Two numbers are usually sufficient (and this saves space in the plots). We give standard deviations where it is possible and useful. However, we checked all plots concerning the suggestions above. In several figure caption, we tried to be more clear concerning the input parameters and the retrieved parameters.

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.

Most observations (with northerly airflow) occur in summer.

In Sect. 3 we write: The variability in the depolarization ratio may reflect the influence of numerous dust sources around the eastern Mediterranean. A seasonal cycle in the depolarization ratio time series is not visible because most northerly flows occur during the summer season. In winter the release of soil dust may be generally reduced by enhanced precipitation (increased wash out), wet soils (prohibit dust emission), and the presence of snow covers (in the Turkish mountains and further north and east).

If so, this could be also demonstrated, integrating their climatological study.

The data set is not too large, we avoid to analyze and interpret the data in more detail than given in Sect. 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.

Included!

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?

Sure the threshold line is just arbitrarily chosen, but this line supports the eye to see the differences. We do not say, that this is a threshold line.

Regarding the variability…., as we mentioned in the answer to reviewer #1…: To our opinion, trajectories show the most probable pathways of air masses, and the MODIS fire spots indicate that there were fires during the specific period of interest. More precise information is not available. The trajectories may indicate that it is probable or less probable that air masses were influenced by fire smoke and dust or not. We can never be sure that smoke-free cases are really free of smoke (and dust). The trajectories are simply not good enough to trust them at all. Furthermore, they cannot tell us whether other dust sources contributed to the observed enhanced levels of depolarization or not. They just provide some information on air mass transport and the probability which of the numerous aerosol sources may have contributed.

We avoid to discuss the trajectories in too much detail. We also avoid to discuss the specifically 4 high values. We mention that there are many potential dust sources around Cyprus which cause the overall variability. So, we keep the discussion short (on a qualitative level in Sect.~3).

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.

We show the exponential fit now and state that the 1/e decay time is 4 days (Sect. 3). We leave the blue circles in the plot, to keep this color feature throughout the paper… However, the improved explanations should be sufficient now, that even these cases contain some smoke (and dust).

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.

See our discussion above! How can we exclude a contribution of any of the numerous dust sources? Nature is complex, trajectories are trivial and may just help us to identify the most probable sources. On the other hand, we have only a limited set of observations (for northerly flows). We cannot do more than to distinguish just smoke and smoke-free cases. We improved the discussion in Sect. 3 keeping the comment of the reviewer in mind.

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.

We improved the plots accordingly. The backscatter coefficient decreases when we have a strong source at ground, but the depolarization ratio may be roughly constant if the mixing of aerosols remain almost constant. Of course, the particle depolarization ratio should decrease to 5% and less if urban haze dominates close to the surface. In many cases with heavy dust, however, the depolarization ratio is almost constant down to the ground.

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.

We removed all these noisy and not trustworthy parts of the profiles (in Figures 10 and 11).

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 an

We improved the reference. In Mamouri and Ansmann (2014) this aspect is discussed (volume-to-extinction conversion factors for fine dust, coarse dust, non-dust)