

Interactive comment on “Extreme dust storm over the eastern Mediterranean in September 2015: Lidar vertical profiling of desert dust at Limassol, Cyprus” by R.-E. Mamouri et al.

Anonymous Referee #3

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Review of the paper “Extreme dust storm over the eastern Mediterranean in September 2015: Lidar vertical profiling of desert dust at Limassol, Cyprus” by Mamouri et al. submitted to Atmospheric Chemistry and Physics.

This short paper describes the case of an extreme dust plume occurred over Eastern Mediterranean lasting for few days and observed with ground based measurements at Cyprus. In addition satellite products from MODIS are used. The paper is within the scopes of Atmospheric Chemistry and Physics as it presents a rare (may be unique) dust event. However, in its current version needs major revision before it can be accepted for publication. The two main reasons is the lack of uncertainties throughout the paper and the presentation of speculative results (although not necessarily wrong)

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in the aim to characterize the event especially for the 8th September when lidar data were not available. For more details see the comments below.

Major comments

1) Cyprus has four WMO stations (<https://www.wmo.int/cpdb/dashboard/index/countryCode:C>) Why you do not present visibility results from them? At least 3 (Akrotiri, Larnaka and Paphos) of them take visibility observations, with Akrotiri being next to Limassol (~25 km). The data should be available at least through the national meteorological service of Cyprus. Although, the photographs in Figure 3 are indicative of the low visibility occurred during the noon of 8th and the contrast with 9th September, still they do not follow exactly the WMO guidelines especially when visibility is poor, as established in WMO Guide to Meteorological Instruments and Methods of Observation (http://library.wmo.int/pmb_ged/wmo_8_en-2012.pdf). The visibility results will permit also an assessment of the variability of the dust extinction-to-mass conversion factor given the observations of PM10 for the 3 sites providing both types of observations.

2) For a short paper like this presenting an extreme event, what I was hoping to find was uncertainties to all the observations and of course the factors used from previous studies. Just providing mean values is not enough in order to establish the importance and the uniqueness of this event, especially for the values mentioned in the abstract and conclusions. Although, the authors provide estimations in the legends of Figures 6 and 7, it would be better to introduce them in the manuscript and the figures. While there are not uncertainties presented regarding the results of 8th September.

3) In the abstract and the conclusions you present mass concentrations values estimated from visibility by using a typical dust extinction-to-mass conversion factor. Also, the AOD values of 6-10 are based on assumptions about the vertical distribution. Although, in both cases the values are logical (according to the analysis presented in Section 3), simply they are not measured, so I strongly suggest to present in the abstract and the conclusions only the observations by adding the phrase “with possibly

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higher values occurred the 8th September” or something similar. Especially, in the case of mass concentration there are measurements and PM10 (or PM2.5) is what you find in the literature. For this reason is more appropriate to present the PM10 measurements in the abstract/conclusions.

4) The Introduction needs rewriting, especially paragraphs 1 and 3. What’s the point to present the results of the study already in the Introduction section in a short paper like this? On the other hand, there is no reference at all about climatological and extreme event studies for the region. At least, there are studies covering the Eastern Mediterranean dealing with AOD, lidar measurements and PM10. Example of relevant papers (certainly a non exhaustive list) can be found below:

Basart, S., Pérez, C., Cuevas, E., Baldasano, J. M., and Gobbi, G. P.: Aerosol characterization in Northern Africa, Northeastern Atlantic, Mediterranean Basin and Middle East from direct-sun AERONET observations, *Atmos. Chem. Phys.*, 9, 8265-8282, doi:10.5194/acp-9-8265-2009, 2009.

Gkikas, A., Hatzianastassiou, N., and Mihalopoulos, N.: Aerosol events in the broader Mediterranean basin based on 7-year (2000–2007) MODIS C005 data, *Ann. Geophys.*, 27, 3509-3522, doi:10.5194/angeo-27-3509-2009, 2009.

Gkikas, A., Hatzianastassiou, N., Mihalopoulos, N., Katsoulis, V., Kazadzis, S., Pey, J., Querol, X., and Torres, O.: The regime of intense desert dust episodes in the Mediterranean based on contemporary satellite observations and ground measurements, *Atmos. Chem. Phys.*, 13, 12135-12154, doi:10.5194/acp-13-12135-2013, 2013.

Papayannis, A., et al. (2008), Systematic lidar observations of Saharan dust over Europe in the frame of EARLINET (2000 – 2002), *J. Geophys. Res.*, 113, D10204, doi:10.1029/2007JD009028.

Querol, X., Pey, J., Pandolfi, M., Alastuey, A., Cusack, M., Pérez, N., Moreno, T., Viana, N., Mihalopoulos, N., Kallos, G. And Kleanthous, S.: African dust contributions

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to mean ambient PM10 mass-levels across the Mediterranean basin, *Atmos. Environ.*, 43, 4266–4277, 2009.

The authors should review the literature in order to establish the extreme character of the event. More specific comments regarding the Introduction follow:

i) Page 1, Lines 11-14: “The visibility ... 10000 $\mu\text{g}/\text{m}^3$.” If I understand these are the results of the paper. If not provide the reference otherwise delete the sentence.

ii) Page 1, Lines 14-15: Some thing as previously if there is a reference, please provide it otherwise delete.

iii) Page 1, Lines 16-17: “Surprisingly ... models.” Please provide information about the models. E.g. <http://sds-was.aemet.es/>

iv) Page 1, Line 23 to Page 2, Line 7: How useful is to provide this breakdown of Section 3 in a short paper? The authors repeat themselves several times. I suggest deleting it and incorporating any additional information provided in this part (if any) to the relevant subsections.

5) Page 2, Lines 14-15: Although, there are not observations from the CUT-TEPAK AERONET sun-photometer during the event, there are observations (at least Level 1.5) from other sun-photometers in the region like AgiaMarina_Xyliatou and SEDE_BOKER. It is important to compare MODIS AOD retrievals with AERONET values in order to establish how good there are in the case of large AODs (>2). This is important, especially, from the moment that you are using MODIS AOD over Cyprus in Section 3.1.

Minor comments

6) Page 1, Lines 2-3: Which dust models, give details otherwise delete.

7) Page 2, Line 19: Add reference for MODIS AOD.

Levy, R. C., Mattoo, S., Munchak, L. A., Remer, L. A., Sayer, A. M., Patadia, F., and

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Hsu, N. C.: The Collection 6 MODIS aerosol products over land and ocean, *Atmos. Meas. Tech.*, 6, 2989-3034, doi:10.5194/amt-6-2989-2013, 2013.

8) Page 2, Lines 26-30 and Page 3, Lines 19-26: The authors present AOD and PM10 observations for several sites in Cyprus, but they do not discuss the differences between them, i.e. the spatial variability. Either discuss the spatial variability among the different sites or just present the data for Limassol. In the former case, I suggest to present the MODIS AOD maps (similar to Figure 1) instead of the AOD time series, probably zooming in Cyprus. Otherwise use the same time range and the same cities for Figures 2 and 4.

9) Page 3, Lines 9-11: Provide reference for the visibilities obtained from pilots.

10) Page 3, Line 23: "... and thus main contain errors." Ask the Air Quality Department of Cyprus for validated data. Otherwise, what's the point of presenting data in your manuscript which are of unknown quality?

11) Page 3, Line 30: Replace "Cyprus" with "Limassol", as the lidar observations are from Limassol and as you stated in Page 2, Lines 23-25 the Troodos Mountains were always visible during the dust storm, certainly not affected the same way with Limassol.

12) Page 3, Line 30: "A two-layer structure ... on 8 September," this is pure speculation. Either present observations or delete it.

13) Page 4, Lines 5-7: This is just a speculation, as you do not have any information about the vertical distribution of the dust. The authors should underline more this fact using a stronger word than assumption. The fact that the Troodos Mountains were not covered by dust means that possibly the dust layer was below 2 km, but it could be just 500 m (or lower) like the 9th September (Figure 5). On the other hand, Figure 6 shows through the backscatter coefficient that the layer was not homogeneous with the higher values at surface during the 9th September, so this could be the case for the 8th. At this point MODIS maps can give a hint about the area with the high AOD (even

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saturated at 5), while also the AOD observations from MODIS/Terra could be useful to check the temporal variability. Certainly, this result although plausible can not be one of the main conclusions of the paper as it is based on assumptions which can not be verified. Instead the authors could use the MODIS AOD.

14) Page 4, Line 19: The word frequently is in contrast with just one case study mentioned in the next sentence. Either rephrase or use another reference for the altitude of Saharan dust plumes over Cyprus.

15) Page 4, Line 24: Why you do not use the same time interval, e.g. 18-20 UTC for all profiles shown in Figures 6, 7? Although, I do not expect large changes in the results (if any) the comparison between the four days will become straightforward.

16) Page 5, Lines 1-4 and Figure 6: For all four days the depolarization ratio reduces significantly below about 0.5 km. This means that surface aerosols are always a mixture of dust with pollution or is just an instrument artefact e.g. overlap function?

17) Section 3.6 and Figure 8: I do not think that this section and figure add something in the paper. I suggest to totally removing both. Otherwise, the authors should justify their utility.

Technical comments 18) Page 1, Line 24 and Page 2, Line 18: Replace 'imaginary' by 'imagery'.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-354, 2016.

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