Interactive comment on "Middle East versus Saharan dust extinction-to-backscatter ratios" by A. Nisantzi et al.

Anonymous Referee #1

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The article by Nisantzi et al represents a continuation of the research described in Mamouri et al, GRL, 2013. The highlight of the latter paper was that dust originated in the Arabian peninsula has a lower lidar ratio (30-40) compared to Western Saharan dust (50-60). This difference is thought to have its origin in the smaller concentration of illite, and hence a decrease of the real part of the refractive index.

To obtain this result, elastic backscatter lidar returns had been combined with depolarisation measurements and sun-photometer-derived AOD. The method of constraining lidar retrievals to sun-photometer AOD measurements is not new (see e.g. Takamura et al, 1994). However, in the variant presented by the authors, account for the presence of several different aerosols in the atmosphere above the Limassol site is made: PBL aerosols, non-dust tropospheric aerosols (non-depolarising) and dust tro-

pospheric aerosols (depolarising). The method requires to assume a priori values of the lidar ratio (LR) for the PBL and the non-depolarising components, so as to derive the LR for the dust component. The lidar that the authors use has a Raman channel and could thus be capable of determining the LR directly; however, the GRL article was based on a single episode (28-30 September 2011) when the Raman signals were not available: hence the need to use the elastic lidar method instead.

In the new paper, a more systematic approach is used, and four years of observations (2010-2013) are analysed to reinforce the conclusions of the previous paper. Fourty-nine dust events are analysed systematically, of which 17 show a Middle Eastern (ME) origin and 32 a Saharan origin. The best cases are identified as having a dust backscatter fraction of 0.8 (or larger) and a PDR at least 0.25, or in other words, when the dust aerosol is not too contaminated with other aerosols: the number of cases fulfilling these criteria reduce to 12 (6 each for ME and Saharan dust). A case study (23 May 2013) is considered, where the LR could be derived both with the lidar-sun-photometer approach and with the Raman method, showing agreement in the result.

The research topic is certainly of high importance, the LR being an important parameter for aerosol remote sensing by lidar. Spaceborne sensors, such as CALIPSO, cannot measure the LR directly, and hence we need detailed information on different source regions. However, I believe that the paper could benefit from a radical revision of how the text is written, because at the moment it is quite difficult to follow the reasoning. Moreover, a more rigorous treatment of uncertainties and biases would make the results more persuasive.

I recommend a major revision.

MAJOR POINTS:

1. Despite treating an important topic, the way the paper is written is very confusing and does not help the reader have a clear flow of thoughts throughout the text. I

suggest that the authors critically revise the text as to make it straightforward to follow. Many of my comments are minor, but their number is far too large.

2. The results for the dust LR are dependent on the assumptions made for the other aerosols, i.e. the LR for the PBL and non-dust components and the depth of the PBL. I have the impression that this influence is quite marked in the data. It should be discussed better, and dust LRs for each individual case should always be accompanied with an estimate of the error that arises from varying all these assumptions. It is only if the estimated error is smaller than the difference between Saharan and ME episodes that the method proves useful.

3. Although the paper has the ambition to study cases where a mixed aerosol is found, I have the impression that only in cases where dust is purer results are truly reliable. The paper itself states it on P.5214 L.3-7. Maybe it is worth restating the paper's ambitions to more realistic ones.

4. All the assumed lidar ratios for the PBL (25-35) and the

   non-dust free troposphere (25-70) should really be better justified, and the arising uncertainties on the dust LR shoulde be evaluated.

5. How is the depth of the PBL evaluated? Do you keep it fixed, do you use a radiosonde for each case, or do you base it on the lidar data?

6. If backtrajectories can give an estimate of the airmass origin, they are not a measure of composition. Please discuss the difference between the two and insert the necessary caveats.

7. No mention of cloud-screening is made in the paper; however, I assume that this must be an important component of this algorithm.

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8. Abstract: it would be worth highlighting what is new compared to the 2013 paper, i.e. the examination of a large number of cases. At the moment one has to read the whole paper to understand what it adds to science.

9. Paper structure: It would make sense to place section 4.3 before 4.1. In this way the reader goes from reading about the method in section 3 directly to its application. Section 4.1 is an additional verification using Raman lidar and is quite useful, but its insertion before 4.3 is distracting. It is not clear to me what usefulness has section 4.2. Section 4.4 should not belong to the results, but to the conclusions.

OTHER POINTS:

10. Since your lidar has Raman capability, why don't you use the latter, which is more direct measurement of the LR and is less dependent on assumptions? I assume the reason may be technical; it is worth stating, anyways.

11. Abstract: state the average values of LR that have been found, together with their standard deviation, and rounded to integers, e.g. 53 +/- 6 and 41 +/- 4. Indicate the wavelength (532).

12. P.5205 L.21 "more studies are needed". Why? Expand concept.

13. P.5205 L.21 "photometers" –> "sun-photometers" (and same correction throughout the paper).

14. P.5205 L.29 "derived accurately" –> "distinguished"

15. P.5206 L.6 "the computation of the extinction and backscattering coefficients"

16. P.5206 L.8 "real part" –> "column-integrated real part derived with AERONET"; "column lidar ratio" –> "derived column lidar ratio"

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17. P.5206 L.10-11. This should be better highlighted to make readers appreciate the importance of this paper.

18. P.5208 L.4-5 "The laser transmits linearly polarised laser pulses at 532 and 1064 nm, and detects the parallel and cross-polarised signal components at 532 nm."

19. P.5208 L.12-13 This statement is untrue: in the paper you also make use of 1064 (Fig. 3) and Raman (section 4.1).

20. P.5208 L.4-13 Give integration time and vertical resolution.

21. P.5208 L.15 "retrievals of column-integrated particle size distributions..."

22. P.5208 L.14-21 Note that AERONET cuts retrieved column-integrated particle size-distributions at a 15 micron radius. This could be sufficient for long-range transport (as large particles get deposited) but it could prove insufficient near the source. Ryder et al (2013) have revealed particles as large as 200 micron in the Sahara (see their Fig. 3). This should be discussed as a potential limit of using AERONET for size-distributions.

23. Section 3 (data analysis procedure). It is unclear whether the procedure is applied systematically to all the data available, or only for selected cases when dust advection is detected. It is also unclear what integration times are used (I suppose several hours for each case, like in the examples shown?)

24. Section 3, Point 1. Mamouri et al (2013) gives three different approaches for combining lidar and sun-photometer, in equations (1), (2), and (4); they differ by the number of different aerosol types that are accounted for at once. Please clarify which one of these is applied at this first stage (I suppose it is equation 2). It would moreover benefit this paper to outline the principles of the method once again.

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25. Section 3, Point 3. Detail your approach to backtrajectories better, please. Are they run in a systematic and automated way for all data, or are they only run for cases where dust is detected thanks to depolarisation (in the latter case, point 4 should come before point 3). How is the end altitude of the trajectories at Limassol selected? (fixed, or based on data, etc.?). Have you considered using trajectory ensembles? Have you got objective criteria to interpret the trajectories, or is it by visual inspection on an individual basis? Do you take into account the height of the backtrajectories in each source location in relation with the local PBL height?

26. Section 3, Point 4. A PDR of 0.15 sounds like a dust mixture rather than pure dust. Why have you chosen such a low threshold? You later say that the good cases are only those with PDR > 0.25, so why not set this as a threshold directly at this stage?

27. Section 3, Point 5. Please use the term "base" instead of "bottom". In the first line eliminate "remaining". How is the PBL top determined? You mention a "high" PDR in the PBL; please quantify.

28. Section 3, Point 6. Replace "SD" with "standard deviation". Please clarify if mean and SD are computed over time (successive lidar profiles) or over the column.

29. Section 3, Point 7. Please explain how the thresholds at 0.05 and 0.31 have been determined.

30. Section 3, Point 8. 25-30 sounds like a large LR for clean marine aerosols; e.g. Omar et al (2009) give 20. L.19: yes, this would introduce uncertainties; therefore it would be a good idea to display this uncertainty next to each measured value of the dust LR. L.20 "it was found": where?

31. Section 3, Point 10. Please highlight that DF is different than Db.

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32. Section 3.1. This part is too qualitative. I suggest putting an uncertainty estimate next to each estimate of the dust LR.

33. P.5212 L.24. Detail information on how you prolong the lidar profile below overlap. This could be as simple as saying "in the same manner as in Mamouri et al (2013)".

34. P.5213 L.21. Fig. 4 shows the chart at 1400UTC as well, but this one is never mentioned in the text. Why?

35. P.5213 L.24. The statement "contained a mixture of..." is inaccurate, because you have no measurement of composition. Please restate as "travelled over X, Y, and Z and thus was potentially a mixture of A, B, and C". For some reason that I don't understand, this mixture becomes a pure "aged European haze" at L.9.

36. P.5214 L.7. AOD 0.38 is in contradiction with 0.45 given at L.15 and in the figure.

37. P.5214 L.16. "Fig. 6" –> "Fig. 5"

38. P.5214 L.17. Why is the extinction below 1 km computed from backscatter? Does the Raman channel have a larger overlap range?

39. P.5214 L.20. "total tropospheric" –> "free tropospheric"

40. Section 4.2. What is the purpose of this section? Could it be eliminated?

41. P.5216 L.3-4. Why at times 0700-1400UTC? Moreover, is this statement not in contradiction with Section 4.1, where data up to 2100 have been used?

42. P.5216 L.5. Microtops appears here as a surprise. It is not at all mentioned in section 2. Why is Microtops used instead of AERONET?

43. P.5216 L.14-15. "close to the ground" should be put in relation with the Saharan PBL depth for the time of day and season. It is known that the Saharan PBL can be as deep as 6 km in the late summer afternoon, and that at night dust remains in the residual layer up to similar altitudes.

44. P.5217 L.28. "we assumed low FT non-dust lidar ratios". Although the explanation given for this choice has its logic, it strikes that pushing low the non-dust LR would cause the dust LR to increase. The authors need to persuade the reader that such a bias would be negligible.

45. Tables 1 and 2: I am unsure they are useful. It would, instead, be useful to have a table listing all the 49 cases under study, and for each indicate: the assumptions on PBL LR, PBL height, and non-dust LR; the list of regions where the airmass has travelled; the PDR, Db and DF; and the estimated dust LR together with its uncertainty.

46. P.5218 L.12-13. Could the large Angstrom exponents be put in relation with a large atmospheric residence time, and hence loss of the coarse mode?

47. Section 4.4. What is presented here would better fit in the conclusions.

48. Fig.3 shows high intensity features at the top (red). Is it noise or cirrus? In case of cirrus, does it not contaminate your sun-photometer AOD?

49. Fig.5. Show uncertainty on LR curve (+/- 30%)

50. Why do the backscattering profiles in Fig. 5 and 6 look different? Should they not be the same?

51. Fig. 10 and 11. Show the uncertainty on the dust LR for each point. It looks like pulling the grey triangle (non-dust LR) up or down has the effect of pulling the dust LR the other way. This could look like a flaw, unless it is addressed and

potential biases are quantified. Note: it looks like PDR and DF are correlated (and this would in any case make sense).

52. Fig. 12: This figure is unclear to me. Does it relate data from the present study or from Schuster et al?