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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The vertical distribution of aerosols, Saharan dust and cirrus clouds at Rome (Italy) in the year 2001" *by* G. P. Gobbi and F. Barnaba

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Authors responses to Referee # 1 (referee questions are indicated by the symbol #). Page and line numbers refer to the PDF version of the ACPD on-line manuscript.

GENERAL COMMENTS

This paper presents analyses of one year of lidar measurements of tropospheric aerosols and cirrus clouds from a site near Rome, Italy, with particular emphasis on the influence of Saharan dust aerosols. The paper gives a clear description of the instrumentation and the data analysis procedures and is potentially a very useful contribution to our knowledge of atmospheric radiative forcing by aerosols and clouds.

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However, I don’t understand the authors’ choice of data groups (no cirrus, no dust, and Saharan dust), since there is some overlap between groups. I also question the usefulness and meaning of the "total" data grouping and the presentation of yearly averages of optical properties. The results would be more useful if restricted to physically meaningful groups and seasonally averaged quantities.

Since the latter questions are repeated in more detail in the reviewer's “Specific comments” below, the relevant answers will be provided in that section.

SPECIFIC COMMENTS

To more closely match the discussion in the paper, perhaps the word aerosols in the title should be changed to the phrase: planetary boundary layer aerosols.

Generally speaking the term aerosol includes all suspended particulate matter (including Saharan dust) in the whole atmosphere, not only the PBL. In this work we mainly compare Saharan dust to no-dust conditions, and Saharan dust is shown to often affect the PBL too (e.g., Fig. 3d). Therefore, even understanding the reviewer’s point, we feel it is better to keep the title as it is to avoid making it too long and/or possibly confusing.

2. Why was the lidar calibration done against a monthly standard atmosphere and not individual radiosondes more closely matched in time with the observations?

This is done because full-resolution radiosoundings are not freely available to us since they are done by the Italian (military) Weather Service. Furthermore, the closest radiosoundings are launched at a coastal site, 30 km away from our (rural-urban) site, and only twice per day. Since our average measuring rate is of 4.8 profiles per day of measurement, applicability of these radiosoundings for lidar calibration would be questionable as well. In any case, the impact of departures of the actual p, T profiles with respect to the climatological ones is considered in the error estimate (page 5, line 2) and it does not exceed 5%.

3. I found the data groupings used in the tables and plots to be confusing. I had to continually refer back to the part of the text where the various categories (NC, ND, SD) were defined. Can the categories be redefined to match those in the title, namely (PBL) aerosols, Saharan dust, and cirrus clouds?

Table 1 was specifically inserted in the original manuscript to provide data grouping information without having to refer to the text. Possibly, the reviewer missed it.In fact, there are several arguments (discussed in the manuscript) to demonstrate that the employed categories are physically meaningful (e.g. the reviewer’s general comments) and why their choice is reasonably good. In fact, lidars allow to measure profiles of all the optically thin particulate matter suspended in the atmosphere. It is the altitude and time-dependent displacement of this physically meaningful group (total suspended particulate matter) the subject of interest to climate modelers. The categories employed in the manuscript give the possibility of determining (by means of four curves only) the properties of: 1) the total suspended particulate matter (TOT profiles) and its most important subgroups: 2) the physically meaningful group of all the atmospheric aerosols (the one characterised by cirrus-less conditions) that is the no-cirrus (NC) profiles; 3) the physically meaningful group of total suspended particles in Saharan dust conditions, given by the Saharan dust (SD) profiles; and 4) the total particulate matter (i.e., aerosols+cirrus) in the absence of Saharan dust advection, given by the no-dust (ND) profiles. While providing a continuous view of the whole troposphere, this grouping into four curves allows to derive properties of the two aerosol-alone subgroups (PBL and Saharan dust) by simply ignoring the well detached cirrus contribution in the plotted profiles. Furthermore, presence of the cirrus trace in both dust affected and non-affected profiles allows to reveal the possible impact (in terms of amplitude and altitude) of dust on cirrus formation and to individuate the transition region between aerosols and cirrus. Conversely, following the reviewer suggestion, we would need to plot six curves: 1) PBL aerosols, 2) Saharan dust aerosols, 3) Total aerosols, 4) cirrus clouds in no-dust condition, 5) cirrus clouds in dust-conditions, and 6) total cirrus clouds, ending-up with a fragmented, less clear figure. Because of these rea-

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sons, we believe the employed categories are well suited to convey the information we wanted.

4. The profiles in Figures 2 and 3 labeled Total are not very meaningful to me. At first sight, it is confusing that the total can be smaller than one or more of the components making up the total. I think the Total profiles are actually averages of the individual components weighted by occurrence frequency.

As indicated in the original manuscript (page 6, line 18-19 and also Table 1) curves labeled TOT represent the average (not the sum) of all available profiles. It should not be surprising that transient events (as Saharan dust aerosols) show smaller amplitude when averaged in the TOT profile with respect to the average profiles (SD), representing the dust events only. It is an expected effect of seasonal or yearly averaging.

5. I also don’t think the yearly average statistics and profiles are very useful, and it seems to me they can be misleading. For example, a big point is made in the paper about the increase in cirrus cloud extinction and R between 6-10 km in the yearly average Saharan dust (SD) profiles in Figure 2. However, Figure 3(d) shows that the extinction enhancement between 6-10 km occurs only in the fall (SON), when the influence of Saharan dust at altitudes a few km above the PBL is smaller than in spring (MAM) and summer (JJA). Can the authors explain this, or is this an example of a misleading average? Could the enhanced extinction between 6 and 10 km in SON be a result of biomass smoke being transported from Africa or even South America? Can the authors differentiate between aerosols and optically thin cirrus if the two exist in the same altitude range?

As a matter of fact, extinction enhancements in the cirrus region during Saharan dust conditions are not only observed in Autumn. Figure 3 shows these were also observed in MAM (at 8.5 km) and in JJA between 8 and 11 km (here, similarly to SON, extinction is 2-3 times larger during dust conditions, but the small amplitude does not show it well in the figure). In discussing cirrus clouds, we decided to address yearly averages

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because the cirrus signal is very noisy due to the inherent short lifetime of these clouds. We do not make a big point about that (no mention in the abstract) we just believe this is an interesting point to notice, deserving further study. In order to respond to the reviewer objection, we added to the text (section 3.1) that the extinction (backscatter) increases were mostly generated in the June-November period.

Both yearly and seasonal averages are presented in the manuscript. Yearly averages are often used in climate modeling (e.g., Figure 5.2 and Figure 5.8 of IPCC 2001, Chapter 5: Aerosols, their direct and indirect effects). We definitely believe this information should be kept in the paper to allow for comparison with such important documents.

Finally, it is quite easy to differentiate dust from smoke signals: particle depolarization is over twice as large in the first case. Conversely, Saharan dust has depolarization levels similar to cirrus clouds, but backscatter ratios are one order-of-magnitude smaller and lifetimes one order-of-magnitude longer. As pointed out by the reviewer, this was not so clear in the manuscript. Therefore, we added to section 3 the following sentences:

1) There is no clear-cut way of discriminating between various aerosol and cloud types on the basis of lidar traces alone. However, some good inference can be done employing the polarization lidar data. Our classification has been performed via manual single-profile analysis, comparison with model aerosol forecasts and, when necessary, by back-trajectory analysis.

And

2) Saharan dust has depolarization levels similar to cirrus clouds, while backscatter ratios are mostly one order-of-magnitude smaller and lifetimes one order-of-magnitude larger. Together with a broader vertical extent of Saharan dust layers, all these features allow for a good discrimination between cirrus clouds and dust.

As indicated in the original manuscript (page 5, line 8), all Saharan dust detections were determined on the basis of the lidar trace and forecasted by the DREAM model.

However, the reviewer’s doubt (Could the enhanced extinction between 6 and 10 km in SON be a result of biomass smoke being transported from Africa or even South America?) gave us the opportunity to further prove the Saharan origin of the air masses classified as SD (Saharan dust) in our analysis. We therefore computed 1830 5-day backtrajectories for the dates affected by Saharan dust conditions. The relevant analysis has been inserted in section 3 of the revised manuscript together with an additional new Figure (Fig.2). These backtrajectories demonstrate the good reliability of our original classification. The person who carried-out the backtrajectory runs (L. Ammannato) has been included as co-author of the paper.

TECHNICAL CORRECTIONS

Comment

Abstract, line 1: Extinction is not observed by lidar; it is derived from backscatter.

We do not agree on this point. The monostatic lidar signal is certainly collected in backscatter configuration, but it is made by the product of backscattered and extinguished radiation. Both extinction and backscatter coefficients are unknowns in the single-wavelength lidar equation. Therefore, once determined the altitude-dependent lidar ratio, the lidar equation can be solved for either extinction or backscatter coefficients, it makes no difference.

Section 1, line 4: It is not certain that indirect aerosol forcing is larger than direct aerosol forcing, only that the range of estimated values is larger.

That is the message of the referenced paper by Ramanathan. In the revised manuscript instead of (larger) we now state (possibly larger).

Section 1, second line from bottom (and elsewhere): The lidar depolarization ratio does not indicate aerosol thermodynamic phase, only whether the aerosols are spherical or non-spherical.

In the manuscript (page 5, line 25) we state: (Depolarization provides a powerful too

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to infer particulate thermodynamic phase). We used the verb to infer since it means to deduce from evidence and reasoning. We believe this well describes what depolarization allows to do with respect to aerosol thermodynamic phase. In fact, the manuscript mostly reports depolarization as an indicator of the presence of non-spherical particles (e.g., page 5-line 12, page 7-line 25, page 7-line 28, page 9-line 11, page 9-line 14, etc.). In spite of these clear statements, we understand the general sentence (lidar-derived profiles of optical properties and thermodynamic phase of aerosols and cirrus clouds, Introduction, page 3, line 22) could still be misleading to somebody. It has then been changed into (lidar-derived profiles of optical and physical properties of aerosols and cirrus clouds).

Page 3, line 15: Change the word individuated to selected.

Done

Page 4, line 10: Change the word weighed to weighted.

Done

Section 3.1, first sentence: should read Figures 2a, 2b, 2c, and 2d, respectively.

Done

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5755, 2003.

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