

Interactive comment on “Airborne high spectral resolution lidar observation of pollution aerosol during EUCAARI-LONGREX” by S. Groß et al.

Anonymous Referee #2

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The paper “Airborne high spectral resolution lidar observation of pollution aerosol during EUCAARI-LONGREX” by Gross et al. Presents results from high quality data collected during an international measurements campaign. The data are undoubtedly valuable. The investigation about the intensive aerosol properties as lidar ratio and depolarization ratio, retrievable through an HSRL, is timely and appropriate as they are input parameters for the retrieval of optical properties by simply elastic backscatter lidar as CALIOP, the currently lidar on board CALIPSO satellite.

However, the collected data should be investigated in more details, discussion should be more appropriate and better referenced.

Authors refer to air masses satisfying the 3 criteria reported at page 26849 as “pollution” aerosol. This nomenclature is probably due to the assumption that everything is

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below a certain altitude (authors indicate 2.5 km asl, they should explain the choice of it or insert appropriate reference) is polluted. What about continental aerosols? The paper from Evans cited in this manuscripts reports also values for the continental aerosols and Ferrare et al., JGR 2001 reported lidar ratio values for continental aerosols, even simulation studies are present in literature (e.g. Ackermann 1998). “Pollution” aerosol should be identified not only for the location but also as results of pollution events/sources. However, I suppose during this valuable campaign, many data from other instruments could support this pollution identification. Authors should refer to these data for a correct identification of polluted cases.

One study case is reported as example of the applied methodology and results, but related observations are not discussed in details. Some (not exhaustive) points that should be discussed would be: - measured values of lidar ratio and depolarization - altitude dependence of these values - which are the layers identified as “pollution” aerosol layers according to the 1-2-3 criteria reported in the manuscript?

Figure 7 is not really readable. However, which are the “pollution” aerosol layers identified by the authors? Or the whole profile is considered for this study (for example fig 8).

Lidar ratio values reported by the authors for “pollution” aerosols are typically between 50 and 65 sr (Figure 9). Authors cited in the manuscript several papers about polluted aerosol values measured in different sites, values in fair agreement with the ones measured during this campaign. It is right mentioning that different aerosol types as dust, volcanic and continental aerosol also correspond to lidar ratio within this range of values. Lidar ratio therefore cannot be use alone for the typing of these types of aerosol. Authors should clearly state that lidar ratio at 1 wavelength is not sufficient for an accurate aerosol typing.

The reported particle depolarization ratio values (3-11%) are very low values, probably lower than the possible accuracy on it. So that, it is highly questionable a discussion

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in details about differences between 5% and 7%, but the only indication that these measurements could give about depolarization ratio is that for this “pollution” aerosol the depolarization ratio is low (typically below 10%).

The reported typical resolution of aerosol extinction and backscatter are significantly different. Are backscatter profiles correctly reported to the same resolution of aerosol extinction profiles for the determination of lidar ratio values? For the comparison/discussion, lidar ratio and depolarization value have the same resolution?

I agree with referee #1, authors stated that there is no dependence on place for lidar ratio and depol values, but they did not analyzed this aspect. The altitude dependence was not analyzed as well. These are relevant aspects that could be addressed with the valuable dataset collected by the HSRL during the campaign.

Finally, Conclusions are too strong and not well supported by the analysis reported in the manuscript. The no dependence on location and aerosol age are not well assessed; the “characteristic” values observed for this aerosol type are not compared to the other aerosol type. Authors should better address these points in the revised paper and correspondingly revise the conclusions section.

Specific comments

Abstract: “show virtually no variations” what does it mean virtually? However as referee 1 noted there are variations with the altitude case by case but this aspect is not investigated at all in the current version of the manuscript

Page 26845 line 26: typo Current Page 26845 “the retrieval of ...” not only extinction but also backscatter retrieval by simply elastic backscatter lidar needs for assumptions about lidar ratio. This should be better explained in the revised manuscript. Page 26846 please check the sentence”Both quantities only depends on the particle properties of the aerosol type...”

Page 26846-26847: statistical errors are reported regardless of the aerosol load.

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Please provide information about the altitudes and the typical aerosol load you are referring to.

Page 26849: “the observed air masses were transported from western directions...” if figure 2 reports backtrajectories it should be eastern directions, isn't?

Page 26849: about the case study see general comment above

Section 3.2: colour ratio is used in CALIPSO retrieval scheme, but the backscatter related Angstrom exponent could be more suitable for a wider community use. Moreover the Angstrom exponent has the advantage to be less dependent on the wavelengths pair.

Page 26853: which depolarization ratio value do you expect for marine aerosols? Add references

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26843, 2012.

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