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Atmospheric Measurement Techniques Discussions

Interactive comment on "Aerosol classification using airborne High Spectral Resolution Lidar measurements – methodology and examples" *by* S. P. Burton et al.

Anonymous Referee #4

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Comments to the AMTD paper of Burton et al., Aerosol classification using airborne HSRL measurements

General

The paper contains very interesting and original material, obtained with an excellent airborne HSRL. However, many points of the manuscript must be improved, are not just carefully written. For example, a better literature review concerning airborne aerosol HSRL is necessary. At the moment the reader gets the impression, only North American groups are active in this field. That is simply not true.

Furthermore, many observational findings are again set into the context of North American aerosol observations, widely ignoring the long list of papers and efforts regarding multiwavelength lidar and aerosol typing since the beginning of EARLINET or even earlier.

A statistical analysis of aerosol observations performed in the framework of 18 field campaigns is presented. That is unique! But, there is surprisingly no attempt to use any air mass transport analysis to distinguish between different aerosol types and characteristics, and, at least, to demonstrate, for some cases, shown in the figures, that the statistical approach makes really sense and is in agreement with the backward trajectories. This would then be convincing..

Some quantities in the paper are not explained. Sometimes AOT is used, sometimes AOD. Instead to use backscatter-related Angstrom exponents throughout the paper, the authors present: e.g., aerosol wavelength dependence, backscatter color ratio, backscatter spectral ratio, aerosol backscatter-related Angstrom exponent, backscattering Angstrom exponent. In the case of the wavelength dependence of the depolarization ratio: ratio of aerosol depolarization ratios, spectral depolarization ratio, depolarization spectral ratio, particle depolarization spectral ratio, aerosol depolarization spectral ratio, ...

Details:

p. 5633, lines 16-27: This is a good example of ignoring the non American papers on aerosol typing. Furthermore: Why do you concentrate here on simple backscatter lidars?

p. 5634, lines 7-23: Now the focus of the literature review is on AERONET. This is also not satisfactory. There are at least 30 lidar papers since 2000 (or even earlier, starting with the Ferrare papers, JGR 1998) dealing with high quality lidar observations with focus on aerosol typing.

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You may not know that the Cattrall paper is based on modelled lidar ratios. No measured ones are presented. There was later on one lidar paper (Muller 2007). In this paper the Cattrall results were compared with real world lidar ratios (from Raman lidar). And it was shown (if I remember well) that the Cattrall data are partly questionable (because they are modelled, rather then measured).

So, I suggest to perform a literature review on HSRL, and on aerosol typing with appropriate lidars (Raman lidar, HSRL,. . ..).

p. 5636, line 24. . ...: Please add Chinese HSRL observations (Liu, 2002), Esselborn (Tellus, JGR, Appl. Opt) on HSRL, Wandinger (JGR, 2002) showing airborne HSRL data. The list of papers dealing with aerosol HSRL is not that long, thus an exhausting review is justified here.

p. 5639, l. 25, please review not only the work of Japanese and American groups. It is almost impossible to measure desert dust depolarization ratios east of China. The dust is mixed and contaminated. Most depolarization ratios are then clearly below 30%. The best available depolarization ratio data set for desert dust is presented by Freudenthaler et al. (2009). They measured desert dust directly close to the source.

p. 5640, lines15-16: The lidar ratio of desert dust is around 50sr because of the irregular shape. Absorption does not play any role. Marine particles seem to be always spherical (because RH is always very high). The lidar ratio of marine particles is then determined by their large sizes (and thus close to 20sr). Concerning absorbing particles, please check the latest SAMUM special issue (Tellus 2011, Gross papers, Tesche Papers, Weinzierl paper, the latter includes HSRL observation!!!).

p. 5640, line 22: Lower lidar ratio and higher depolarization ratio . . . suggest higher concentration of dust? Please be more specific. . .! Is the reference level around 90sr? Then it would make sense.

p. 5641, first paragraph: discussion is confusing!

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p. 5641: aerosol classification: Why do you not use Angstrom exponents as most of the groups do, why do you still use color ratios.

p. 5642, line 4: scattering ratio. . .. Please define!

p. 5644: Another example of unsatisfactory literature review: The text suggest that Cattrall, 2005, and Omar 2005 seem to be basic, fundamental papers regarding aerosol typing.

Why do you permanently relate your findings to AERONET observations (now including Dubovik, and again Cattrall)? Photometers measure extinction effects, and height integrated as column values. So there is almost no chance to separate different layers, to obtain information of 'pure' aerosol types.

On the other hand, you present lidar results! You mainly deal with backscatter effects, profiles, why do you not use the huge number of Asian studies (Murayama, Noh, Liu, Franke, ...) and European findings (Amiridis, Papayannis, Mona, Mattis, Muller, Gross, and many others in JGR, ACP, Tellus. . ..) for comparison? There is already so much knowledge on aerosol types based on lidar work, that one does no longer need this Cattrall paper.

p. 5645: I would leave out such a chapter on ice particles. The review is again frustrating in view of the huge amount of cirrus lidar papers.

p. 5646, l12: It is interesting to notice that the Freudenthaler 2009 paper is referenced in the paper, but not here. Exactly here, a citation would be appropriate.

p.5646, l.20: Ice lidar ratios are measured since 1990 (Ansmann, Reichardt, Eloranta, Whiteman, Sakai . . ..) and are of the order of 20-30sr (in the case of off zenith pointing) and can be as low as 2 sr in the case of nadir pointing, and cirrus lidar ratios are thus lower by roughly a factor of 2 and more with respect to dust lidar ratios. That should be clearly stated.

p. 5647, l.5, again no reference to Freudenthaler et al.. Asian dust observations are

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always a problem. I believe that it is almost impossible to observed pure dust over Japan. And, if they measure almost 40%, I would start to think about there efforts to assure calibration quality.

p. 5647, l.12, Heese et al., 2009? There are so many better papers on this topic in that SAMUM special issue. Obviously the references are arbitrarily selected. According to Freudenthaler, more than 33% for pure dust at 532nm is not possible.

Section 4.2.3. See measurements of Gross et al. (SAMUM, Tellus 2011) Esselborn (SAMUM1, Tellus 2009). Separation of dust and smoke was also published in JGR (Tesche et al., 2009, based on SAMUM results).

Section 4.2.4. See Petzold et al. (SAMUM 1, Tellus 2009, SAMUM 2, Tellus 2011) on this topic.

Section 4.2.5. Isn't it possible that depolarizing mineral particles are injected too during hot fires? The discussion concerning fresh smoke is not confincing. Relatively low lidar ratios in the case of fresh smoke? That means they do not absorb much, or are fresh smoke particles large, and this causes the drop in the lidar ratio? Please provide an improved argumentation. How is fresh defined? Did you use trajectories to estimate transport time.

Section 4.2: General remark: How can one show an aerosol classification without any airmass transport study to identify the most likely aerosol sources, to study the transport ways (chances to add further aerosols from a variety of sources) and temporal periods (impact of aging on the aerosol properties)?

Figure 2 should include a clear indication of the Mexico City area (city boundaries, down town area)

Figure 2: Please provide backward trajectories for the interesting layers, around 4 km height, and the different aerosols below 3.5 km height before and after the black column.

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Figure 3: Two times Carribean?

Figure 4: In our polluted world measurements of pure dust features are almost impossible. Only the SAMUM team may have observed pure dust. Is that included in your analysis?

Figure 5: Without showing air mass backward trajectories, all the discussion around Figure 5 is just speculation, and thus not acceptable. Please show backward trajectories to convince me that you really measured dust over Alaska. How can you be sure. May be it is fresh volcanic ash from all the volcanoes in that area. By the way, volcanic aerosol is not considered in your classification scheme.

Figure 6: The same: A discussion that does not include an airmass transport analysis is useless.

Figure 8: I would leave out this case.

Other figures are fine.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5631, 2011.