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Atmospheric Measurement Techniques Discussions

Interactive comment on "Airborne multiwavelength High Spectral Resolution Lidar (HSRL-2) observations during TCAP 2012: vertical profiles of optical and microphysical properties of a smoke/urban haze plume over the northeastern coast of the US" *by* D. Müller et al.

Anonymous Referee #1

Received and published: 22 March 2014

General remarks

The paper presents very interesting aerosol observations with a new airborne High Spectral Resolution Lidar. Exciting first measurement results of a field campaign TCAP 2012 are presented to provide a quick impression of the potential of the new system.

However, the paper is too short. Many points are not outlined in sufficient detail. This

must be improved. Minor revisions are necessary.

Detailed comments:

Introduction:

Page 1062: A better introduction into multiwavelength aerosol lidar is needed. Why do we need this multiwavelength technique? Where is it realized? EARLINET!

Next, a better introduction in the development in multiwavelength HSRL is then need. What is the potential (compared to existing and well established Raman lidar networks) of such systems, advantage compared to Raman lidars? HSRL can be used on aircrafts, Raman lidar are only useful as ground-based systems.

What is the basic motivation to develop such a system? Explain, why NASA undertakes these effort to investigate the potential of future, next generation spaceborne lidars.

Then one needs to discuss the already on-going activities of ESA, ALADIN and EarthCARE HSRL and the potential to derive aerosol extinction profiles. Give references. There are papers from Ansmann and from Flamant in Appl Opt. and Tellus A.

Methodology:

The history of HSRL needs to be better introduced and discussed. Papers from She et al. from the 1990s, Hair et al., Appl. Opt, 2001, and earlier Eloranta papers (with Grund, with Piirionen) need to be cited and discussed. This should cover hardware developments as well as retrieval techniques. Eloranta (2005) is not an adequate reference, provides just a brief summary or introduction. This is not sufficient. And the new 355nm channel! To say, the novel channel at 355 nm will be discussed in a separate publication is not acceptable. Please provide at least one paragraph on this.

You do not discuss the unique depolarization measurements. You measure depolarization ratios at three wavelengths. This is a strong point.Why is that not used, not presented? Even if not used, please explain, how that is relized. It seems that paper

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is just a first, quick publication with some easy-to-produce products, and not a well-prepared manuscript. This is not good and readers may get a negative impression of the NASA activities.

How can you achieve uncertainties of 0.1-3% for the optical data (backscatter and extinction coefficients)? The calibration (backscatter coefficient, particle reference value in the free troposphere) and the use of temperature and pressure profiles in the retrieval of the extinction coefficients introduce already errors at least of the order of 5-10%. Backscatter coefficients at 1064 nm can never be obtained with accuracies of less than 10%.

Page 1063, line 15 to page 1064 line 5: These paragraphs should be moved into the introduction. If HSRL has so large advantages why do we see only a few systems running? In contrast, why do we have so many Raman lidars running, although it is of disadvantage. Please discuss more seriously, along realistic facts. It depends on the application! I would never run a HSRL, if I just want to collect aerosol climatological data which I can best get with robust Raman lidars. But in the case of aircrafts, HSRL is of clear advantage. . ., or better there is no alternative.

Please provide some references (conference abstracts, ILRC?) for the new HSRL.

Page 1064, lines 4-5: "Details on this automated software and the results of the simulation studies will be given in a future publication''. This statement is not acceptable. Please provide a useful introduction (one or two paragraphs, if possible with references to conference proceedings. . .). Otherwise, this short publication is useless at all (something like a black box: results are shown, but in which way, is not said). This is unsatisfactory.

Page 1064, lines 10-13. I do not understand, the aerosol in situ observations cover the particle radius range from 50 nm to 5.35 microns, and the HSRL data inversion radius range is from about 50 nm to 6 microns. Isn't it better to consider even large particles with radii up to 10 or 15 microns (you may have mineral dust!), and then consider for

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comparison only HSRL results fore particles with radii of less than 5 microns?

There is no compact overview of errors (optical as well as microphysical properties), here, in this section. Please provide all error information within one or two paragraphs.

Results:

Did the in situ observation always indicate that there were no large particles, i.e., particles with radii larger 6 microns?

What happens when you ignore particle with radii larger than 6 microns in the inversion, but they are present?

Figure 2 is a nice and a convincing figure.

Figure 3 is not so nice. The y axis indicates effective radius, but only for the first two plots.

Better put all figures b,c,d,e in another Figure 4.

All in all a good paper, but too short.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1059, 2014.

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Interactive Discussion

Discussion Paper