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Interactive comment on "A study on aerosol extinction-to-backscatter ratio with combination of micro-pulse lidar and MODIS over Hong Kong" *by* Q. S. He et al.

Anonymous Referee #2

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General comment:

The paper contains unique observations of aerosol optical properties in the subtropical part of East Asia. In particular, observed data of the lidar ratio, that is an important parameter in lidar remote sensing of particle extinction profiles, are rare for the East Asian region.

A unique approach of combining passive remote sensing from space and ground based lidar is shown. This approach may be used later when spaceborne lidar and radiometer are flown in formation (NASA Calipso, A-train).

However major revisons are mandatory for two reasons:

(A) The paper deals with lidar ratio observations, but the literature in this field is obviously not known. This cannot be accepted. Since about 2000, many observations, done with Raman lidars in large field campaigns (e.g., ARM site Oklahoma, ACE 2, INDOEX, ACE Asia) and within networks (e.g. EARLINET), are published. Thus, there is a wealth of observations and that has to be mentioned. It is no longer true that lidar ratio observations are rare!

(B) The technique critically depends of the overlap effect correction (in the near field of the lidar). If the overlap effect is not corrected for (or badly corrected) the retrieved column backscatter value is highly erroneous, and thus the retrieved column lidar ratio are highly questionable. However and very surprisingly, nothing is said to that rather important problem. So, without an extended discussion of the overlap problem by showing one case with an uncorrected and with an overlap-corrected backscatter coefficient profile the paper cannot be accepted. These two cases have to be shown to give the reader a fair chance to make his own opinion about the quality of the retrieved data. The overlap effect, in terms of remaining uncertainties after the correction, must be quantified in addition.

Specific comments:

p3100: Abstract has to be rewritten after all the required changes.

p3101, 14-16: HSRL and Raman lidars are automatically run in Oklahoma (even at daytime) and in the Arctic (cf. ILRC proceedings, Italy, 2006). 50% of the EARLINET lidars (European Lidar Network) are Raman lidars, almost routinely operated... (partly at daytime). So the statement that advanced lidars are rare and do not allow frequent observations is not true.

p3101, 29, p3101, 1-12: Ferrare (JGR, 2001, North American lidar ratios), Ansmann (JGR, 2001, 2002, ACE2, maritime and European lidar ratios)), Franke (JGR 2001, 2003, maritime and South Asian lidar ratios, long term record), Mattis (GRL, 2002 (Saharan dust lidar ratios), GRL 2003 (smoke lidar ratios), JGR, 2004, (European lidar ratios, long term record), Mueller (JGR 2002 (ACE2) , JGR 2003 (INDOEX), JGR 2004 (Artic haze), JGR 2005 (Siberian and Canadian forest fires)), Murayama (JGR 2003, ACE Asia), Sakai (GRL 2003?, cirrus and dust), and several other EARLINET groups (Balis et al., Amirids et al., in GRL and JGR in 2004-2005), De Tomasi (South European lidar ratios, Saharan dust lidar ratios, JGR 2004-2005, Appl. Opt. 2003-2004), and Pappalardo (ILRC Italy, EARLINET, Network lidar ratio observations at 10 stations)..... are many examples of the quickly growing number of the papers on measured lidar ratios in America, Asia, and Europe.

p3102, 13-17: Ansmann (JGR 2002, ACE2) showed combined observations with multi wavelength Sun photometer and six wavelength backscatter lidar and discussed all the needed input parameters. Because they measured lidar ratio profiles (after the Raman lidar technique) in addition they had a fairly good idea about the uncertainties in the photometer/lidar analysis when assuming a height-independent lidar ratio. As in the paper here, they made measurements at the coast and demonstrated how difficult the retrieval is when lofted continental haze plumes are present above the maritime boundary layer.

p3104, 1-10: In this paragraph the overlap effect should be mentioned for the first time. To my knowledge, the incomplete overlap affects the lidar measurement up to 5 km height in the case of MPL. Please provide the true height for the Hong Kong MPL. Because of the typical configuration of the MPL receiving optics the minimum height of complete overlap is certainly 3km. Please provide a sophisticated discussion here on the overlap. Mention receiving optics characteristics and how you determined the overlap profile in the measurement practice. This topic is highly important!

p3105, 1-17: Without a clear discussion of the overlap impact on the overall error (as mentioned above, show two profiles one with and one without overlap correction) the paper cannot be accepted. This is a fundamental point and must be discussed

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extensively.

p3105, 18-27, p3106, 1-6: The Fernald procedure needs two input parameters: (a) the particle lidar ratio (height-independent) and a reference value (at the far end, backward integration) or at the near end (forward integration). So if you use the forward mode of the Klett method how do you know that the reference value (in your case the system constant C) is always constant. You vary the other input parameter (column lidar ratio) until the best solution is obtained. Her you assume that the system constant C is really constant. But C may vary too.

The other way around, why did you not use the backward integration method (calibration in the clean free troposphere)? This method is well-accepted in the lidar community. To assume that the lidar constant C was constant over the entire measurment period (of more than a year) is not convincing to lidar scientists! Our experince is that the constant also changes (atleast slightly) from day to day, and changes especially after re-adjustments. Please comment on that. By the way, the same is true for the overlap, it changes from measurement to measurement. How did you fix this problem? Please comment on that, too.

Thus, my feeling tells me that the overall error of the retrieved backscatter coefficient profile for the lowermost 1500m is certainly close to 50% rather than close to 15-20% (as mentioned in the manuscript). Keeping this in mind, unrealistic lidar ratios of 12sr and the significant deviations between the lidar extinction values and the one retrieved from the visibilty observations (on average a factor of 2!!) can easily be explained.

p3108, 1-14: When comparing the lidar extinction values with the visibility values one should consider the most appropriate Angstroem exponent (of about 1.0) and convert the visibility values to 532nm. One may also consider a single scattering ratio of 0.9 to correct for the possible absorption effect. The remaining bias is certainly linked to the overlap correction effect. The bias cannot be explained by different measurement heights (2 m versus 145 m), especially not with a systematic increase of the extinction value with height. That is strange.

p3108, 9: Wandinger et al. (2002) report observations done in Europe.

p3108, 25: An extinction value of 0.4km-1 at 532nm does not indicate clean conditions! The haze conditions may be denoted as moderate pollution.

p3108, 27-29: A lidar ratio of 23 sr represents maritime aerosols (almost perfectly), and does not just indicate a mixture of suburban with maritime particles. The Angstrom value of 0.46 corroborates this assumption. Impact of mineral dust? Road dust or desert dust? Show trajectories!

p3109, 6-29: As mentioned, the discrepancies between the lidar and the visibility sensor should be removed as much as possible to allow a better comparison. Why not taking the Angstrom values retrievd from MODIS to correct the wavelength dependence of extinction obtained from the visibility sensor. And again, the discrepancy of 0.5 seems to be mainly related to an unsatisfactory overlap correction.

p3110, 3-16: This paragraph is a bit confusing. Why not simply mention that the lidar ratio depends on size distribution, chemical composition (absorption), and shape (if large non spherical desert dust particles are present).

p3111-3112: Section 4.3 must be rewritten. It is unacceptable to present monthly mean values that are based mainly on 2 or 3 values. 8 out of the 13 months have less than 5 observations. These are just snapshots. Discussing monthly mean values is highly misleading and confusing. The same holds for the shown seasonal trends. So Table 1 and 3 are fine, Table 3 should be removed as well as Figs. 6 and 7.

In view all the published lidar ratio observations mentioned above, one should no longer give reference to Ackermann (1998) only. That paper is based on ONLY ONE size distribution for each aerosol type. Thus the values are not representative at all. One may also cite several others mentioned above (e.g. Mueller or Franke) and also Catrall (JGR 2005).

p3112, 24: The negative correlation between the lidar ratio and the Angstrom exponent appears to be questionable. This effect may arise from the overlap correction uncertainty and the relatively low number of measurement cases for the range of low lidar ratio values. The lidar ratio should be comparably small for maritime particles when the Angstrom is small too, and should increase with increasing influence of small urban particles indicated by an increasing Angstrom value. Large lidar ratios together with low Angstrom exponents can be explained by desert dust or large road dust particles. But how to explain Angstrom values around 1.5 together with lidar ratios of 20sr? I have never seen such cases in literature (and never measured). Again, the overlap correction may have caused such a result.

p3113: Section 4.4 combines surface wind observations with column lidar ratios. Please check also the correlation between surface flow and air flow at the 850 hPa level, based on backward trajectories. The 850 hPa transport pattern are linked to the column lidar ratio.

p3113, 21-25, p3114: Again, the discussion of the correlation between the lidar ratio and the Angstrom exponent is strange. As long as the overlap problem is not clarified, the results are highly questionable. The conclusion would be: The extinction coefficients depend mainly on the size distribution (that influences the Angstrom exponent) whereas the backscatter coefficients mainly depend on shape characteristics of the particles and absorption (influencing the lidar ratio).

p3115: Conclusion section is confusing. Good agreement between the lidar and visibility sensor??? (...deviating by a factor of 2). Furthermore, the only papers that contain directly measured, realistic, and representative maritime lidar ratios is the paper of Franke et al. (2001, 2003). They made many Raman lidar measurements in clean maritime air over the Indian Ocean in July and October 1999. They found values between 20 and 35sr. Note, that not only large sea salt particles are responsible for the lidar ratio but also gas-to-particle conversion processes in the maritime air. The latter aspect (causing small particles and thus larger lidar ratios) is ignored by Ackermann.

Fig.9: How are the first two data points (open circles) to the left in (a) and (b) produced. I do not see any dots.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3099, 2006.