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Atmospheric Chemistry and Physics Discussions

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.

Q. S. He et al.

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General comment: Comments (C): 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 revisions 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. Reply (R): The comment of reviewer is valuable and developmental for the improvement of manuscript quality. Some significant revisions are carried out in the new version of manuscript. A plenty of publications about lidar ratio observations are cited in the revised manuscript, which not only indicate the import of lidar ratio observations but also illustrate the major characteristics of lidar ratios in the worldwide locations. In fact, we have been paying considerable attention to the overlap effect correction in the process of lidar observation and succedent data processing. But, we just unilaterally stress the algorithm of the lidar ratio retrieval and the statistical result, and neglect the statement about lidar data pretreatment, which reduce the integrality and technicality in a certain extent. Therefore, as the reviewer suggests, the lidar equation is rewritten to include the overlap effect. Meanwhile, a sophisticated discussion on the overlap correction is supplied in the context.

Specific comments: C: p3100: Abstract has to be rewritten after all the required changes. R: Pag. 3100: The abstract has been rewritten.

C: 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

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at daytime). So the statement that advanced lidars are rare and do not allow frequent observations is not true. R: Pag. 3101: The literatures about HSRL and Raman lidar observation are added in the text.

C: 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. R: Pag. 3102: Some examples of the measured lidar ratios in America, Asia and Europe are summarized into a table according to the referee, which indicates the explosively growing number of lidar ratio observations in recent years.

C: 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. R: Pag. 3102: The paper by Ansmann (2002) about combined observations with Sun photometer and six wavelength lidar is cited in this manuscript.

C: p3104, 1-10: In this paragraph the overlap effect should be mentioned for the first

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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! R: Pag. 3104: The lidar equation is rewritten to include the overlap effect. Meanwhile, a sophisticated discussion and a figure on the overlap correction are supplied in the context.

C: 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 extensively. R: Pag. 3105: A discussion of the overlap impact on the overall error is also given in the same paragraph.

C: 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 measurement period (of more than a year) is not convincing to lidar scientists! Our experince is that the constant also changes (at least 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

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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. R: Pag. 3106: As the referee pointed, the system constant C does vary from one case to another, but the variability in the system constant derived at intervals during the measurement period is steady and monotone. We consider that the recalculated system constant C from interpolation between the measured ones (differ from day to day) can be used to retrieve aerosol extinction coefficient without significant error. Initially, we intended to employ the backward integration method to retrieve the aerosol extinction coefficient, but we found that the background noise of the lidar signal in the free troposphere is significant, especially in daytime when MODIS is available, even though the signals are averaged for one hour. This could result in strange and invalid retrieval. Contrastively, the forward integration method can avoid the impact of background noise on retrieval of extinction coefficient profile. Together with the measured system parameter C at intervals during the measurement period, we consider that the forward integration method is the best choice for this research.

C: 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. R: Pag. 3108: The comment of referee is valuable and developmental. In the figure displaying the extinction coefficients profile on 1 November 2003, we still retain the initial visibility value for comparison because we just intend to indicate the variational trend of visibility values in different pollution condition. In contrast, in the figure comparing the lidar extinction values with the visibility values,

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we convert the scattering coefficients from visibility sensor into extinction coefficients at the same wavelength as that of lidar according to single scattering albedo of 0.9 and aerosol Ångström exponent of 1.0.

C: p3108, 9: Wandinger et al. (2002) report observations done in Europe. R: Pag. 3108: The literature by Wandinger et al. (2002) is replaced by Mueller et al. (2001).

C: 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. R: Pag. 3108: The text in line 24 "Ěfor a very clean condition" alter to "Ěfor a moderate pollution condition"

C: 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! R: Pag. 3108: The lidar ratio of 23 represents maritime aerosols base on back-trajectory analysis but not shown in this manuscript for concision. We have modified the conclusion.

C: 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. R: Pag. 3109: The comment of referee is valuable and developmental. In the figure comparing the lidar extinction values with the visibility values, we convert the scattering coefficients from visibility sensor into extinction coefficients at the same wavelength as that of lidar according to single scattering albedo of 0.9 and aerosol Ångström exponent of 1.0, and the scatter plots of disposed results are showed in the same figure for a better comparison.

C: 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

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(if large non spherical desert dust particles are present). R: Pag. 3110: The paragraph is revised according to the comment of referee. One sentence is removed for concision.

C: 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). R: Pag. 3111-3112: The comment of referee is reasonable. Indeed, discussing monthly mean values that are based mostly on several values is improper and misleading. So, we remove the paragraph about monthly mean LR variation and the corresponding figure. But, we retain the seasonal trend analysis because the example number in each season is enough to indicate the relation between aerosol optical property and seasonal meteorological background condition. The more literatures by Mueller et al., Franke et al. and Cattrall et al. are cited in this manuscript to support the result of this study.

C: 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

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correction may have caused such a result. R: Pag. 3112: The result of negative correlation between the lidar ratio and the Angstrom exponent has been carefully checked again to confirm the validity of observation and retrieval even though one obvious low LR value of 12 induced by cloud contamination is removed from the dataset.

C: 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. R: Pag. 3113: The comment of referee is right to check the correlation between surface flow and air flow at the 850 hPa level, which is also carried out in this study but not shown in this manuscript. Though the backward trajectories show the similar characteristics as that of surface wind and LR, the trajectories analysis seems to be difficult to represent the impact of wind speed on LR in detail.

C: 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). R: The sentence " large particles contribute more for the extinction coefficient" has been altered to "large particles contribute more under the situation of heavy haze event". The correlation coefficient between the lidar ratio and the Angstrom exponent is still negative with the value of -0.93 even though the overlap correction is considered and the abnormal low LR of 12 sr induced by cloud contamination is removed from the datasets. Therefore, the relationship of LR and Angstrom exponent observed over Hong Kong represents the characteristics of Asia Aerosol, which is something different from that of Europe and America.

C: p3115: Conclusion section is confusing. Good agreement between the lidar and visibility sensor??? (...deviating by a factor of 2). Furthermore, the only papers that

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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. R: Pag. 3115: Conclusion section is rewritten to correct several results and add more literatures.

C: 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. R: Fig. 9 is redrawn according to the new dataset and the open circles represent the mean values in the right spaced bin of LR.

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