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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Vertical distribution of aerosols in Mexico City during MILAGRO-2006 campaign" *by* P. A. Lewandowski et al.

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General comments: The paper presents an interesting study of lidar data in the Mexico City. I am not aware of any other series of lidar measurements from a moving vehicle. The article provides an indication that such measurements may be used to observe the spatial variability in the vertical distribution of aerosols above a major city. The paper also attempts to use these data to provide estimates of aerosol mass loading above Mexico City. While the paper presents an interesting dataset, there aspects of this paper that prevent me from recommending publication.

Response to the General Comments:

We appreciate reviewer's recognition of the uniqueness of the measurement approach.

There are no lidar studies, to the authors knowledge, performed from a moving vehicle.

Major comments:

Comment: 1. There is far too little information provided regarding the uncertainties associated with the attempts to compute mass extinction efficiency and mass loading. In section 4.4 the authors acknowledge that the algorithms presented have numerous assumptions (such as a known refractive index, proper calibration, sphericity of the particles, spatial and temporal homogeneity of the aerosol distribution, reliance on Mie theory, meteorological conditions, the exact location of the instrument, uncertainties in the lidar retrievals of aerosol extinction). However, little if any attempt is made to estimate the size of these uncertainties or their impact on the retrievals of mass extinction efficiency and mass loading. There should be some attempts made to estimate these uncertainties and their impacts on the mass com; otherwise, there is little likelihood that such estimates will be useful for modelers.

Response: This is an excellent point. The authors are aware of the need for a proper uncertainty analysis. The manuscript presents a simplified uncertainty assessment such as the total optical depth from the lidar compared to the one from the photometer. The discrepancy between the lidar inversion and the direct total optical depth measurement is 30%. This is the most reliable way of estimating the lidar-derived extinction coefficient.

In case of the uncertainty of the MEE, the numerous assumptions had to be made considering that the sampling volume of the sun photometer extends throughout the entire column of the atmosphere. There are no implications of what would be the range of values of the refractive index, sphericity, etc of the aerosols in the entire column over any given location at a certain time. The proper uncertainty analysis with the photometer derived aerosol size distribution would be an extensive research project on its own and which has not been demonstrated in detail in the literature (most error analyses in the literature are in fact sensitivity studies). Nevertheless the value of

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MEE used for the study has the uncertainty associated with random spread of the measurement and the aerosol size distribution analysis (MEE=0.90$\pm$0.17 m2/g), which results in the cumulative uncertainty of the concentrations of 35%. A paragraph was added to the manuscript (equation 6).

Comment: 2. In the abstract and elsewhere, the paper states that the aerosol mass estimates have a 1.5 m vertical resolution. This is apparently based on the 1.5 m vertical resolution of the lidar backscatter measurements. However, this does not reflect that the size distribution measurements used to compute these estimates are a column average and, therefore, contain no information about how the aerosol size distribution varies with altitude. The authors have made the assumption that the size distribution is constant with altitude and that the only variation is represented by the lidar measurements of backscatter. Given the complexity of the aerosols observed (combustion, pollution, dust, etc.), this does not seem to be a very good assumption. There were airborne measurements of aerosol size, composition, etc. made by several aircraft throughout MILAGRO that the authors could have consulted to examine this assumption.

Response: This is a very good point. We agree that the aerosol size distribution may significantly vary across the column. But at the time of the measurements, it was impossible to provide spatially resolved aerosol size distribution data.

The fundamental reason why we believe the sun photometer can provide a better representation of the aerosol size distribution data for the lidar is the fact that the both instruments share the same sampling volume. On the other hand, the reviewer suggests using the airborne platforms which provide data only from in-situ measurements. Also the similar argument could be brought up whether to use the ground station aerosol size distribution data for that matter.

We think that for the MEE approach in estimating concentrations using lidar, the fact that the instruments have a common sampling volume outweights the fact that the sun

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photometer is not spatially resolved. The column averaged aerosol size information overall better represents the entire sampling volume over the in-situ measurements from the aircraft or the ground stations.

Comment: 3. There were many other aerosol-related investigations and measurements made during MILAGRO. Some of these measurements, in particular other lidar measurements of aerosol distributions in the Mexico City region, would have been quite relevant to this study. However, there was no mention or reference to these measurements and how the lidar measurements presented in this paper relate to these other lidar measurements. This is a major omission.

Response: The authors examined one case of lidar measurements for March 7th, 2006, for the morning North-to-South transect through Mexico City and southern rim of the basin. To the authors best knowledge, there was no active mobile or airborne lidar in that time located in the area. Further, ground based measurements of particulate size distributions, for example, are of limited utility in comparing to a column average. If there is other data that is relevant, we would appreciate knowing which paper or data have the reviewer had on their mind with respect to the dataset presented in the manuscript? Any assistance would be appreciated as we would gladly address this problem.

Comment: 4. In the abstract, the authors seem to imply that aerosol loading derived from the lidar measurements compares well with the hourly-averaged PM10 ground observations from the RAMA network. However, there are no detailed comparisons of these measurements and so it is very difficult to really assess the level of agreement between these measurements. In the conclusion, the paper states that the lidar derived estimates have a similar order of magnitude as the observations from the RAMA network. Does this really constitute good agreement?

Response: Good point. The abstract was rephrased for clarification. The PM10 concentration from RAMA network was intended to provide a general reference for the

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lidar-derived estimates of the concentration. There are objectives preventing the authors from comparing RAMA results and lidar direclty. First, the measurements are 200 m apart vertically (RAMA is ground based, and 200 m is the lowest useful lidar range). Second, the comparison would only involve two data points (only measurements for 7 and 8 am are co-located). Third, the RAMA results are hour average and it would pose new confusion for how to average the lidar data in time/space domain (spatial interpolation would be a challenge).

Overall, the RAMA measurements were presented for a general reference and a sanity check for a new approach in estimating aerosol concentrations from a lidar. This was emphasized in the manuscript.

Minor comments:

Comment: 1. (abstract, line 12) Is this supposed to say ": : :compares well: : :" As discussed above, there is little indication of this provided in the paper.

Response: The text in the abstract was rephrased. Although the measurements are difficult to compare directly, it is to the advantage of the reader to present the results from RAMA network. One can see the general agreement between the two indirect measurements of the aerosol mass concentration.

Comment: 2. (p. 6831, line 25) What is the lowest altitude for the lidar backscatter measurements?

Response: This is a good question. The appropriate explanation was added to the manuscript. The lowest useful altitude is 200m. There is a major reason why the concentrations from the lidar were retrieved from 200 meters above the ground in Figures 6c and 7. It is related to the dynamic range of the digitizer (12bit ADC). The signal observed by the detector decreases with distance as 1/R2, so within the first 200 m the signal is huge compared to the signal at for example 2000 m. By disregarding the signal from the first 200 m, the maximum cap of the signal (governed by 1/R2) is

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brought down to the lower values and therefore increased the dynamic resolution of the digitizer in the range of interest (500 – 3000 m). Ignoring the signal in the first few hundred meters is a common practice in co-centered lidar systems (laser co-centered with the telescope). It is a trade-off between the minimal useful range and the dynamic resolution of the signal.

Comment: 3. (p. 6832, line 9) The Langley method provides a means to calibrate the direct solar measurements; how are the sky radiance measurements calibrated?

Response: Good point. They are not. They are only relative measurements. In the analysis for the aerosol size distribution (Nakajima et al.), the measurements are normalized to the intensity of the sun, which is a well known value. So there is really no need to absolutely calibrate the sky radiance measurements. The direct solar measurements are in fact calibrated using Langley method.

Comment: 4. (p. 6823, line 12) Were the Sun photometer measurements made while the vehicle was moving? How long did it take to acquire each set of measurements?

Response: Very good comment. This is now clarified in the text under 2.b. Sun Photometer.

Comment: 5. (p. 6833, line 16) The Klett solution requires an estimate of the lidar ratio to derive aerosol extinction from backscatter. What value(s) was (were) used in these retrievals? How were the values obtained? What is the uncertainty in the retrieved aerosol extinction values associated with uncertainty in the lidar ratio?

Response: The Klett method does not require the knowledge of the lidar ratio. For that reason it does not give a complete solution to the lidar equation (i.e. both attenuation and backscatter). It estimates extinction coefficient based on the assumption of a far extinction, in this case one derived from the free troposphere (Klett, 1981, 1985).

As mentioned earlier, the best method to estimate uncertainities associated with the lidar inversion is to compare it to the direct measurement of the total optical depth,

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which was demostrated in the manustript.

Comment: 6. (p. 6834) The discussion of MSEE should include an indication that n(r) and Qext(r) are functions of altitude and location.

Response: Good point. Qext(r) depends only on the wavelength and the refractive index. Since there are no means to measure the refractive index with the altitude, for the sake of the analysis Qext(r) is not a function of the altitude. N(r) is in fact a function of space. The indication was made as suggested.

Comment: 7. (p. 6835, line 5) SKYRAD.pack.4.2 assumes spherical particles which is likely not a good assumption with all the nonspherical dust particles observed over Mexico City.

Response: Great comment. Mie theory (used in SKYRAD.pack.4.2) in fact gives a solution for spherical particles. Several studies shown that to first order it works for real particles (e.g. Curtis, D. B., B. Meland, M. Aycibin, N. P. Arnold, V. H. Grassian, M. A. Young, and P. D. Kleiber (2008), A laboratory investigation of light scattering from representative components of mineral dust aerosol at a wavelength of 550 nm, J. Geophys. Res., 113, D08210, doi:10.1029/2007JD009387). The text was changed to reflect the comment.

Comment: 8. (p. 6836, line 27) How do you know that the concentration decaying with time is due to water condensation on the particulates? What measurements indicate or support this?

Response: Very good comment. The balloon soundings from Mexico City airport 1200UTC March 7th, 2006, indicate that the lifted condensation level (LCL) is found at 4500 m MSL, which is about where the intense concentration decaying with time is observed. The appropriate paragraph was added to the text.

Comment: 9. (p. 6838, line 24) The pseudo-3D display of the lidar data on a digital elevation map is not new.

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Response: We agree. It was an overstatement. The manuscript was changed.

Comment: 10. (p. 6839, line 25) Were the lidar estimates of AOD obtained by integrating the lidar extinction profiles between the surface and about 3 km above ground? If so, what is the estimate of the amount of AOT above 3 km that the lidar estimates would not include? This additional amount would increase the size of the lidar overestimate.

Response: This is an excellent point. The lidar observed the atmosphere between the ground and about 6 km above MSL. Since the planetary boundary layer generally does not grow that high, the atmosphere beyond 6 km MSL contains little particulate matter. The real problem here is the subvisible cirrus clouds, which are difficult to take into account. An appropriate paragraph was added to the manuscript.

Comment: 11. (p.6840, line 7) How much higher are the lidar estimates of concentrations than the RAMA measurements? These comparisons have not yet been discussed. It looks like sections 4.5 and 4.4 should be switched. Also, elsewhere in the paper, it is implied that the lidar and RAMA mass estimates are in agreement. Here the statement is that the lidar is overestimating the RAMA measurements; which is it, good agreement or overestimate?

Response: The comparison of the values of the concentrations from the RAMA network and the lidar poses certain difficulty. As mentioned before, both instruments have fundamentally different measurement techniques, therefore one should be careful comparing the results. Please refer to the answer to the major comment 4.

Comment: 12. (p.6840, line 14) Without providing uncertainty estimates, it is not possible to judge the utility of the method.

Response: This point was addressed earlier and the manuscript was changed to answer the comment. The full extent uncertainty analysis involving all steps of the analysis would constitue a research work of its own. Therefore certain simplification were necessary and we think were adequate.

Comment: 13. (p. 6840, line 26) With all the other potential problems and uncertainties associated with the assumptions and analyses, it is hard to believe that the major reason the lidar and RAMA estimate is due to particles larger than 10 micrometers affecting the lidar measurements and not the RAMA measurements.

Response: We agree with the reviewer. It is not a major reason for the discrepancies. The manuscript was changed.

Comment: 14. (figure 4) What do the various lines in each of the graphs represent?

Response: The various lines represent separate sun photometer measurements. There were 3 measurement sessions (therefore 3 graphs) which consisted of 5 individual sky radiance measurements. The early morning session 8:58am – 9:07am had the most scattering angle measurements (the sun had the lowest elevation angle) and was used as a reference measurement for the size distribution analysis. The text was changed to clarify the confusion.

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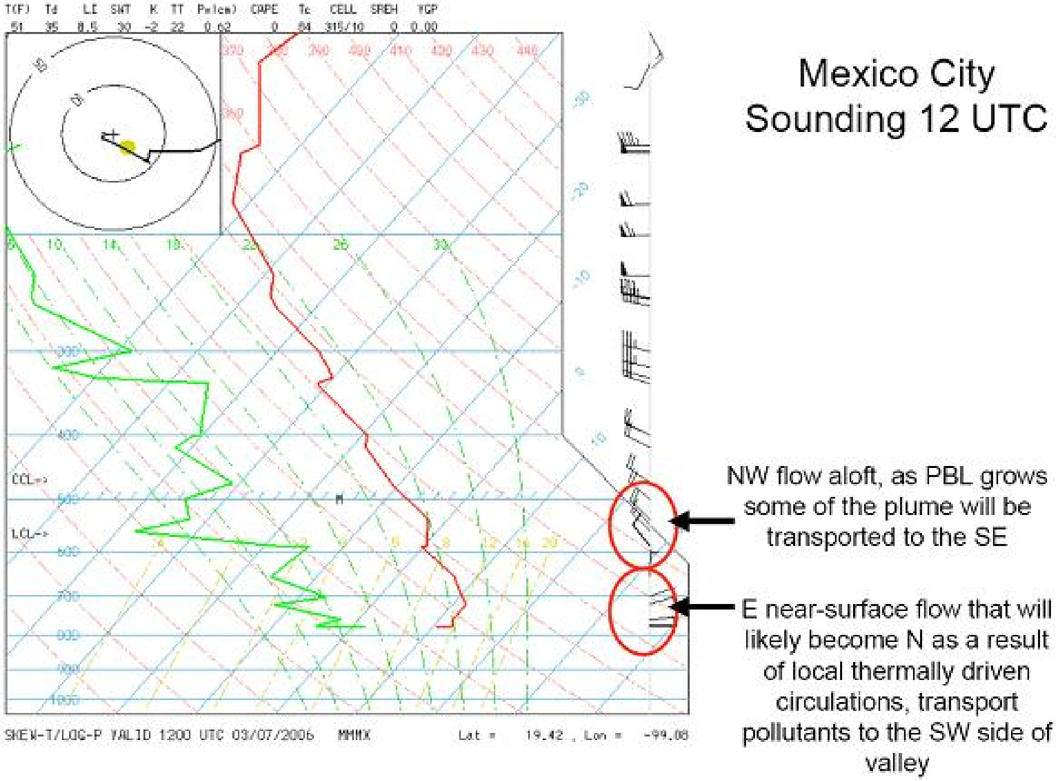

Fig. 1.

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