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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Single particle characterization of black carbon aerosols at a tropospheric alpine site in Switzerland" *by* D. Liu et al.

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This manuscript is a careful study of the properties of single particles containing refractory black carbon. There is an on going discussion in the community of users of the single particle soot photometer (SP2) with respect to terminology when referring to what is measured with this instrument. At the 2009 SP2 Users' group meeting, the concensus seemed to be that the incandescence channel of the SP2 measures refractory black carbon (rBC) as opposed to light absorbing carbon, suggested by Bond and Bergstrom, or BC that is most often used in models or derived from filter based optical absorption measurement. I would like to recommend that the authors change BC to

rBC throughout in order to be consistent with the recent publications by Schwarz and others.

Secondly, I found no errors in how the data were interpreted and the authors have done a thorough job of highlighting the uncertainties and limitations of the SP2 while at the same time using the strengths of this instrument to evaluate the possible pathways by which rBC evolves and is removed from the atmosphere.

There a a number of places, however, where I think the paper would be strengthened by adding a little more analysis that will take additional advantage of the information that can be extracted from the SP2 measurements, as well taking some extra analysis steps to further highlight the differences in air mass histories as reflected in the rBC properties. These suggestions follow.

Page 8775: Line 23: The comparison of the SMPS with the SP2 in the same size range is very interesting but a 36% difference is fairly high. The explanation is that the lower size cut may actually be larger than the 200 nm mobility diameter of the SMPS. How much would the SMPS diameter need to be adjusted to account for the difference in concentration? Should there also be a correction for the difference between electrical mobility and optical diameter? I assume the SMPS was calibrated for the pressure of the research site? Are concentrations being reported at standard temperature and pressure or local conditions? In general, for comparison with other data sets, I think the mass and number concentrations should be normalized to STP.

Page 8776: Line 6: The method by which precipitation type is determined needs clarification. Backscatter and forward scatter extinction is contradictory. Extinction, I believe, is by definition the sum of absorption and scattering. Given the potential differences in scavenging efficiencies of rain versus snow, I think it is important to explain the way this instrument derives the precipitation type.

In addition, it is important to know if the precipitation is localized or wider spread in order to understand how long the particles have been in cloud and the time available

for wet removal. This probably means looking at the satellite images. The alternative is to use the back trajectory analysis that provide information on the liquid/ice water content along the trajectories.

Page 8776: Line 17: It is mentioned that a significant amount of the rBC mass will not be measured at the small end, but what about the mass of rBC that saturates the detectors, i.e. rBC in the large particle tail? I think that this is an important parameter to evaluate, along with the other indices like the mixing efficiency. Although you can't determine how much actual mass is, the percentage of particles causing saturation can be evaluated and could be a sensitive indicator of washout.

Page 8777: Line 24: As seen in Fig. 9, sometimes the log normal fit is very good but in the high BC cases there is a larger difference, particularly in the big particle tail. This uncertainty should be quantified somewhere. From Fig. 4 there is more scatter than I would have expected between the SP2 and the MAAP. Are these comparisons only in cloud-free air? Is this indicative of the uncertainty in deriving total rBC mass from the SP2? There are very few comparisons between filter-based light absorption measurements and instruments that measure BC or EC directly. Although it is somewhat outside the scope of this paper to evaluate the MAAP, the data set is rich in the variety of rBC types. If the primary limitation of filter based absorption measurements is the interference by scattering, it could be interesting to color code Fig. 4 based on the rBC mixing efficiency to see if there are systematic difference related to the relative amount of non-absorbing coating on the rBC.

Page 8779: Line 12: "The advantage of using MF to characterize the coating abundance is that this definition avoids the sphericity assumptions made when calculating a coating thickness." Doesn't the denominator of the equation for MF require the assumption of asphericity?

Page 8780: Section 4.3: Previous studies of air mass aging, for example many of those published in the MILAGRO special issue, normalize the particle mass concentrations

with the CO minus the backgroubd CO. Doing this type of normalization with these measurements might help to highlight the differences in the air mass histories and the various degrees of aging.

Page 8781: Line 9: "The pollutants were rarely removed by wet deposition during this phase, as shown in Fig. 3, and hence BC mass loading reached its maximum during the experiment." I don't understand how this can be concluded from the available measurements. I also think that dry deposition needs to be at least acknowledged as a possible source of removal.

The other parameter that I think is very useful is the mass equivalent diameter (MED). This is one of the statistics that can be easily derived from the mass size distribution and may also be related to the age and mixing state of the aerosols. I would really like to see if it changes significantly depending on the air mass history.

Page 8782: Line 22: "The measured results in the free tropospheric background are statistically analyzed and presented in the first row of Fig. 8.". Maybe I am just arguing semantics but this isn't really a statistical analysis. I would call it more a graphical analysis since these are frequency histograms. I am also quite surprise how high and variable the CO values are for being free tropospheric. How do these compare with published values from other mountain site or airborne measurements?

Page 8784: Line 26: It is difficult to tell from these curves that they are the same shape or not. This is where having additional parameters like the MED and maybe even standard deviation would be better indicators. Also, the number of particles saturating the detector I think is also important. One way to compare the curve shapes is to normalize them by the area under the curve.

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