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Comment

Interactive comment on “NASA LaRC airborne high spectral resolution lidar aerosol measurements during MILAGRO: observations and validation” by R. R. Rogers et al.

R. Rogers

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These comments were very helpful and we are very appreciative. We have addressed all of your specific corrections and responded to the specific comments below:

The authors repeatedly state that an Angstrom coefficient of unity was assumed when scaling measurements of extinction and optical depth to the HSRL wavelength of 532 nm. The choice of this value needs some justification. I find it particularly unusual in the AERONET comparison (section 3.4) since AERONET measurements provide measured Angstrom coefficients.

This was chosen for simplicity, however we feel this is a valid point and have repro-

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cessed the analysis with all AERONET data scaled with the 500_870 nm angstrom coefficients. The table has been updated to reflect this modification as well as some of the text explaining discrepancies between HSRL and AERONET.

The units of time should be “HH:MM UTC”. The use of decimal hours is confusing, especially if the notation HH:HH is used.

At reviewer #1’s suggestion we implemented a common notation throughout the document and chose fractional hour in UTC.

Interpretation of the scatter plots of extinction values (or differential AOD) based on values extracted from a small number of profile measurements (sections 3.1 & 3.2) needs care, since the individual measurements from a given profile cannot be considered independent (a fact that is clearly evident from the patterns of points apparent in figures 7, 9 & 10). This should be briefly discussed in the manuscript.

The following was added to Section 3 prior to 3.1: All of the vertical profiles and related scatterplots presented here have interpolated the profiles to a common 50m vertical grid, determined from the AATS-14 retrieval. This altitude grid is slightly oversampled for the HSRL and AATS-14 extinction measurements, and in addition to the potential for vertical lofting of air masses, we cannot consider adjacent points unique. This should have little effect on the bias and regression parameters reported here, however care should be taken in interpreting the profile data shown here.

I agree with Referee #1 (RC C350, 23 Apr 2009) that there is too much repetition of results that appear in tables 2 & 3 in the text. Simply referring to the tables would make the text easier to read.

Done.

It is good practice to take estimated uncertainties into account when comparing datasets. The lack of error bars on the in situ data for which the errors were not available is acceptable (but rather points to an obvious deficit in these data), but

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don't see any reason why they were not included when comparing against AERONET. AERONET level 1.5 and 2 both contain uncertainties for measured AODs, or if the data is averaged, the standard error on the mean should be used.

We do not yet have an uncertainty product for the HSRL AODs so did not want to mislead readers with errorbars. Based on your comment I have included the standard deviation of all the AOD points going into the mean as error bars to give readers a sense of the the variability in each scene. Using standard error of the mean may be slightly misleading because HSRL acquired more data points than AERONET in a given "coincidence" frame.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8817, 2009.

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