General comments

This paper presents the results of validation of aerosol extinction and optical depth derived from the NASA Langley Airborne High Spectral Resolution Lidar (HSRL). Validation is undertaken by comparison of a range of near-coincident/collocated, independent aircraft and ground based in situ and remote measurements made during the MILAGRO campaign. The particular strength of the HSRL method is that, unlike most lidar measurements, it is able to unambiguously measure aerosol backscatter and extinction (given knowledge of the atmospheric density profile).

The work makes an original scientific contribution by providing the first extensive validation of the instrument in question. The measurements presented are also of interest in their own right as they show a great deal of structure in the spatial and temporal variation of aerosol properties over the MILARGO study area, although the paper doesn't really attempt to interpret these results in detail. The manuscript is well written and has a logical structure, with generally clear explanations of the analysis taken. I recommended that, once the points detailed below are addressed by the authors, the paper be accepted for publication.

Specific comments

NB. Unless stated otherwise below, I agree with the comments already posted by Referee #2 (RC C1320, 29 May 2009).

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.

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

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.

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 refering to the tables would make the text easier to read.

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 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.

Specific corrections

pg 8826, starting line 17: The sentence which starts “The G1 instruments” is to long and difficult to follow. In particular, it is unclear what wavelength has been plotted in figure 4a (ideally, this should be stated in the figure caption itself).

pg 8828, line 2: “...PSAP instruments are also shown...” (replace is with are)

pg 8836, around line 15: It is unclear whether any spacial/temporal averaging applied to the AOD values from either HSRL or AERONET. I am I to assume that AERONET values were averaged for an hour around the aircraft overpass time, while HSRL values within 10 km of the site were averaged?

Figures 2, 3 & 4: These figures all show signs of lossy compression (JPEG or similar). Combined with the small size of the text in the figures, this makes them quite hard to read.

Figure 2: (a) and (b) labels need reformating. Also, the text referring to this figure uses Mexico city as a reference point, therefore its location should be indicated on the maps.

Figure 8: The text in this figure is too small.