Atmos. Meas. Tech. Discuss., 4, C1766–C1770, 2011
www.atmos-meas-tech-discuss.net/4/C1766/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Aerosol classification using airborne High Spectral Resolution Lidar measurements – methodology and examples" *by* S. P. Burton et al.

Anonymous Referee #2

Received and published: 14 October 2011

General comments:

This is a very important paper that describes the aerosol classification methodology that has been applied to a very large data set acquired in 18 campaigns sampling diverse aerosol types over nearly five years. I rate it highly in terms of Scientific Significance, Scientific Quality, and Presentation Quality.

It addresses relevant scientific questions well within the scope of AMT. It presents novel concepts, ideas, tools, and data en route to reaching substantial conclusions. For the most part, its methods and assumptions are valid and clearly outlined, although I make some recommendations below to improve clarity.

The method description is for the most part sufficiently complete to allow others to apply it to their own data sets and test the generality of the conclusions (but see further recommendations). The language is quite fluent and precise.

Some figures are hard to read, but I suspect that the author's originals have been shrunk to fit the AMTD format, as happens too often. The editors need to take care to restore the figures to a readable size in AMT.

Specific comments:

p. 5636: "Since the intensive variables do not depend on the amount of aerosol loading,

there is a much smaller effective limitation on the loading that can be used for classification than what was required for Dubovik et al. (2002) and Cattrall et al. (2005)." This is probably true, but the observation would be more useful if the authors provided an estimate of how large layer AOT or height-resolved backscatter needs to be to obtain adequate signal for a successful classification.

p. 5637: "The HSRL instrument is self-calibrating at 532nm for measurements of aerosol and cloud backscatter and extinction". A few words as to why the technique is self-calibrating would help.

p. 5638: "aerosol wavelength dependence, which is the A˚ngstro¨m exponent for aerosol backscatter". It would be better to give the equation for Angstrom exponent for backscatter.

p. 5638: "Ames Airborne Sun Photometer (AATS-14)". "Tracking" is missing between Airborne and Sun. Sun Photometer should be Sunphotometer: Ames Airborne Tracking Sunphotometer (AATS-14).

p. 5638: "3% (0.01 km^{-1}) ... 50% (0.015 km^{-1})." Something seems wrong in the comparison as stated. 50% is 17 times 3%. But 0.015 km^{-1} is only half again larger

AMTD

4, C1766–C1770, 2011

than 0.01 km^{-1}. Is something amiss?

p. 5639: “The campaigns include many process-oriented field projects for NASA, the Department of Energy (DOE), and the Environmental Protection Agency (EPA)”. NOAA is missing from this list but included in Fig. 1 (GoMACCS, CalNex).

p. 5639: “locations of these missions has enabled”. “has” should be “have”.

p. 5639: “”. Another reference to ARCTAS smoke measurements is:

Shinozuka, Y., Redemann, J., Livingston, J. M., Russell, P. B., Clarke, A. D., Howell, S. G., Freitag, S., O’Neill, N. T., Reid, E. A., Johnson, R., Ramachandran, S., McNaughton, C. S., Kapustin, V. N., Brekhovskikh, V., Holben, B. N., and McArthur, L. J. B.: Airborne observation of aerosol optical depth during ARCTAS: vertical profiles, inter-comparison and fine-mode fraction, *Atmos. Chem. Phys.,* 11, 3673-3688, doi:10.5194/acp-11-3673-2011, 2011.

p. 5642: “The classification algorithm itself is not sensitive to outliers and noise and including or eliminating them has no effect on the classification of the remainder of observations.” This is an unexpected statement and seems to contradict the following statement from p. 5651: “For the HSRL data classified into eight classes as described

above, Wilks’ lambda is 0.083. If outliers are also included, the value is 0.137”. The apparent contradiction should be resolved.

p.5649: “Russell et al. (2010) indicate that absorption A˚ngstro¨m exponent [AAE] derived from AERONET shows promise for separating them [urban and smoke aerosols]”. Actually, Russell et al. (2010) speak of

“Ambiguities in aerosol composition or mixtures thereof, resulting from intermediate AAE values” and note the need to reduce these ambiguities “via cluster analyses that supplement AAE with other variables, for example Extinction Angstrom Exponent (EAE), which is an indicator of particle size.”

They also note:

“the value of combining several different types of remotely sensed information in multi-dimensional

cluster analyses to derive the most information on aerosol type (e.g., to reduce the ambiguity resulting from the partial overlap of the biomass burning and urban-industrial clusters in Fig. 5 [a plot of AAE vs EAE])”.

Some rewording of the p. 5649 statement is in order to note these ambiguities and the need for multidimensional analyses.

p. 5649: “The elevated smoke layer also has slightly higher particulate depolarization (8–10%)”. At 532 nm? This is one of several examples where “depolarization” means “depolarization at 532 nm”. Being more explicit would help clarity.

p. 5651: “The dust category and the “fresh smoke” category”. I think “dust category” here means “pure dust category”. Clarify.

p. 5652: “The ranges of the intensive parameters applicable to each of the aerosol classes are displayed in Fig. 10”. Showing the Wilks partial lambda value in each frame of Fig. 10, and referring to it here, would help show the correspondence of this parameter to the spread of measured lidar parameters (relative to their standard deviations) among aerosol types.

p. 5654: “the sum of the AOT for the two classes exceeds the dust fraction as computed using the Sugimoto and Lee (2006) algorithm.” I don’t understand how one can compare dust fraction and absolute AOT. Is the wording amiss here?

p. 5672 (Fig. 2) and other 6-frame figures like it: I think it would improve charity if the Frame (f) right axis label were “Backscatter Wavelength Dependence” or “Backscatter Angstrom Exponent” rather than “Aerosol Wavelength Dependence”.

p. 5673 (Fig. 3): Here the terminology (and variable) are “Backscatter Color Ra-

AMTD

4, C1766–C1770, 2011

tio" rather than "Aerosol Wavelength Dependence", "Backscatter Wavelength Dependence" or "Backscatter Angstrom Exponent". It would help to choose one variable and terminology and stick with it throughout.

p. 5674 (Fig. 4): A more explicit description of the "two-sigma covariance" used to draw the ellipses would help (maybe in an appendix?).

Also: "Aerosol Depolarization" on the horizontal axis of the right frame. It's probably at 532 nm, but it should be stated.

p. 5680 (Fig. 10): See p. 5652 comment above.

And again: "Aerosol Depolarization" on the horizontal axis of the top left frame. It's probably at 532 nm, but it should be stated.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5631, 2011.