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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long term trends in aerosol optical characteristics in the Po Valley (IT)" *by* J. P. Putaud et al.

Anonymous Referee #2

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General Comments:

This study presents a careful analysis of aerosol data from one of the more polluted valleys in Europe. The findings are interesting in that significant decreasing trends in aerosol amount, SSA and direct aerosol radiative forcing were found over the time period reported. The suggestion is made that using these methods at other locations where similar measurements are made could be valuable for generating regional aerosol climate forcing estimates and for providing information to policymakers of changing air quality and aerosol effects on climate.

The methods used in this paper for data analysis are fairly robust and have been used in previous studies. The specific methods for measuring the aerosol properties are also

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well-tested, although as noted below some other correction schemes should perhaps be investigated. The major question I have is whether a record of 7 years can be considered appropriate for determining aerosol trends. Suppose the duration of this study had been 3 years? You can obtain statistically significant slopes in the time-series data but what does it mean? Could this have been a 7-year period of decreasing aerosol loadings bounded on either side by increasing or flat aerosol levels? If so, the longer term trend might not be decreasing. It is too bad that the data for this study ended at the end of 2010.

Specific Comments:

Abstract: The units for the decreasing trend are % yr-1.

pg. 9043, Line 20: Define 'intensive', and also 'extensive' when first used. Please provide a reference.

pg. 9044, Lines 25-27: The Weingartner et al. (2003) correction scheme is used for calculating absorption coefficients from the aethalometer. The atmospheric aerosol data from which this correction was developed were from thr JFJ site, where aerosol loadings are light and the aerosols are highly aged. This is a simple scheme to apply and does not rely on concurrent light scattering measurements. It does give good agreement with some other methods for certain types of aerosols.

The authors, however, have concurrent light scattering measurements available so why not use them to correct the aethalometer data? The prevailing scientific opinion is that scattering particles deposited on the filter surface of a filter-based light absorption instrument along with light absorbing particles will affect the measurement and should be taken into account. See the paper by Collaud-Coen et al., Atmos. Meas. Tech., 3, 457-474, 2010. There are several other methods evaluated in the AMT review, as well as 2 new methods.

There is no real problem with using the Weingartner correction but the authors should

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say why this one was chosen over the other more recent ones given that they have nephelometer data that can be useful in correcting the aethalometer data for different aerosol types.

pg. 9045, Lines 11-14: The authors need to be careful in relating growth in mobility diameter to growth in physical diameter or scattering coefficient. What assumptions go into this?

pg. 9045, Lines 24-25: AERONET Level 2.0 data are those data that are both cloud screened and quality assured, but they are also available for only the higher AOT episodes (e.g., AOT > 0.4 @ 500 nm). How much of the IPR surface aerosol data could not be compared with AERONET Level 2.0 data because there were no Level 2.0 data? Monthly mean values are presented for the aerosol data but some months undoubtedly have more data than others.

pg. 9047, first paragraph: Please add a sentence on how the residuals in Fig. 1 and other figures were calculated. There are different ways to calculate residuals.

pg. 9047, Lines 12-13: 'A significant decreasing trend... is observed... at 0% RH too.' Again, care must be taken in making this adjustment using the results from an HTDMA. Perhaps a paragraph explaining the method and assumptions belongs in this paper in addition to the reference to Adam et al. (2012).

pg. 9048, Lines 13-14: Change to '...(defined as the ratio between the truncation-corrected aerosol backscattering coefficient and the truncation-corrected aerosol total scattering coefficient)...'.

pg. 9049, Lines 3-6: Please explain how measurement uncertainties for nephelometer and aethalometer of 10% and 30% (should have references), respectively, result in a 'median uncertainty' of 8% for the aerosol SSA. What kind of uncertainty is being discussed? Quadrature sum of errors, or RSS error?

pg. 9050, Lines 3-6: This is a bit troubling. The argument is made in this work that EC

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is well related to absorption and PM2.5 is well related to scattering. I believe that is the point behind Figs. 7a and 7b. A decreasing SSA means that the ratio of scattering to extinction, or scattering to (scattering plus absorption) decreased. Put another way, absorption increased relative to scattering. In Line 5 the authors suggest, however, that changes in the ratio of EC to PM2.5 could not be responsible for the observed decreasing trend in the SSA.

Do the authors believe that other absorbing substances could be responsible for this trend? If so, this should be stated clearly with whatever proof is available. Was enough brown carbon or organic carbon measured to account for this discrepancy? Was the wavelength dependence of light absorption as measured with the aethalometer consistent with increased levels of OC (e.g., larger AAE's, more absorption at shorter wavelengths)?

Technical corrections:

Abstract, Line 2: Change 'Aerosols' to 'Aerosol'.

Introduction, Line 1: Change 'Air suspended' to 'Air-suspended'.

Pg. 9044, Lines 21-22: TSI nephelometer should be model 3563. Nephelometer should not be capitalized.

pg. 9048, Lines 22-23: Change 'A significant decreasing trends is...' to 'Significant decreasing trends are...'.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9041, 2014.