

Interactive comment on “Seven years of measurements of aerosol scattering properties, near the surface, in the southwestern Iberia Peninsula” by S. N. Pereira et al.

Anonymous Referee #2

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The manuscript presents a seven year time series of aerosol light scattering coefficient at three wavelengths measured at a site in southwestern Portugal that experiences air mass advection from the Atlantic, Central Europe and N. Africa with various amounts of more local influence. The data represent aerosol less than $10\mu\text{m}$ aerodynamic diameter. The time series of measured and derived optical parameters is presented statistically and in the context of air mass trajectory analysis.

The calibration protocol was done at least once per year and a zero signal was measured once per hour. The calibration was thus infrequent and the results of the calibration stability are not presented. It is not clear what the “zero signal” represents

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and what the results were. The referenced TSI nephelometer uncertainty applies to a different calibration and zero protocol. The results of the actual calibration and zero measurements and any drifts over time should be presented and used to calculate the uncertainty in the data.

The referenced correction function for non-ideal illumination and truncation is rather uncertain for light scattering measurements that include particles greater than $1\mu\text{m}$ especially when the Ångström exponent is low. Has this uncertainty been included in the analysis?

The aerosol is often described as being dominated by “fine” particles. What is the definition of fine, submicron, fine mode, accumulation mode? What does “dominated” mean quantitatively? Does this mean that more than 50% of the scattering was due to “fine” particles? Does it mean that the mass concentration of “fine” particles was more than 50% of the total.

Previous papers report a mass to scattering ratio. Was that available for this extended data set or parts of it?

Except for the statistical analysis with respect to trajectory category, the results are not put into context of meteorological or other aerosol parameters e.g. mass or chemical concentration, size segregation, which would be of value in relating the aerosol optics to sources for inclusion in global climate models. At a minimum the effect of local wind vectors and boundary layer stability should be included. The description of the trajectory categorization is minimal. How were the sectors and advection paths within them defined? Was a cluster analysis done?

The results and discussion in section 3 are a descriptive narrative of the tables and graphs. The conclusion does not present much in addition to section 3. How might the results be used in global climate models in order to decrease the uncertainties of climate forcing?

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P13725

The last two paragraphs in section 1 are not needed.

P13726

Greater Lisbon

“... anthropogenic aerosols from major industrial regions in the Iberian Peninsula or Central Europe or from the Saharan desert.”

Integrating Nephelometer should not be capitalized.

... every second and was recorded every 5 min. ??

P13730

A table of the comparison locations and values would be easier for the reader to comprehend than the textual presentation.

P13731

Where were the forest fires? How distant from Evora and in which trajectory sector?

“This means that globally ...”

What is meant by “globally” here? Clearly it is not meant to refer to an average over the earth.

P13734

Again, globally?

P13735, line 24

“The difference in the respective aerosol properties seems to validate the separation between M and MIB trajectories.”

Is this statistically significant? Please quantify.

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Figures 3, 5b and 7 do not add much to the description of the aerosol. Graphs in 5 and 7 should be put on the same axis ranges. Figure 5b is a scatter plot in which the data points are so heavily overplotted that no information can be gained. At a minimum a 2-dimensional probability density analysis should be done and the results plotted as contours of equal probability. The trend of the maximum probability should be similar to the data in 5a within instrumental uncertainties.

There are numerous spelling errors and instances of awkward English syntax.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 13723, 2010.

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