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Atmospheric Chemistry and Physics Discussions

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.

S. N. Pereira et al.

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Anonymous Referee #1

Q1. What is there in the results that make the publication of seven years worth of observations important? R: Most publications on aerosol properties contain data for short-term time series, e.g. campaign measurements or data over 1 year only, especially for the Iberian Peninsula and Portugal. These publications therefore cannot assess inter-annual variations, yearly trends, multi-year means etc. This information makes the publication of a seven year time series important.

Q2. Is there a trend over 7 years and, if so, how is it explained? R: No trend is observed.

This issue is now included in the manuscript in section 3.2.1. The marked decrease of scattering in 2008 can be explained by the lack of winter data and the absence of summer forest fires e.g. the total area burnt in 2005 (∼426000 ha) decreased down to ∼17000 ha in 2008; also, this latter value is one order of magnitude lower than the 1998-2007 average (∼163000 ha).

Q3. Add a mean line to Fig 7 to give some perspective on the variance from year to year. R: Annual average and median values (also for the whole period) are now included in figure 1 (we believe that the reviewer meant figure 1).

Q4. Calibrations were done "at least once a year. There needs to be a record of how it varied to estimate the uncertainty attached to comparing one year to another. R: Yes. The calibration constant changed less than 10% per year. This change has a higher influence on shorter-term observations, e.g. when comparing monthly values of the same year, but has a negligible impact on a seven year trend (due to regular recalibrations).

Q5. The discussion of the coarse particle correction and the influence of coarse particles on the observations needs more attention, as in the specific comments. R: See our replies to the specific comments for details.

Q6. The backscatter observations are most important for climate considerations. Their inclusion would help this paper. R: According to the suggestion, we included more information on the backscatter coefficient in parallel with the other quantities (spectral scattering coefficients and Angstrom exponent).

Specific comments:

Page 13724 Line 2 – how "near the surface"? R: "(at about 10 m height)" was added, although the height of the sampling head had already been mentioned in section 2.2.

Line 3 – "valuable" in what way? R: See our reply to comment (1)

Line 8 – "were" grammar. R: "are" substituted the former "were"

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Page 13725 Lines 6-7 – how will your data advance a GCM? R: In a strict sense, data of a single point, although over several years, will not advance a GCM. However, in general, published aerosol properties and their timely variation can be used to check existing parameterizations and may be used for model validation. Significant discrepancies between model outcome and measurements will usually lead to improvements of models.

Line 8 – "had shown" grammar. R: "have shown" substituted the former "had shown"

Line 13 – "where mainly incident" grammar. R: The former expression was substituted by. ". . .where based on measurements carried out during short periods."

Line 20-21 – how do neph measurements "quantify the aerosol load" and I would remove qualitative from the sentence as it contradicts the earlier quantifying. R: Since the scattering and back scattering coefficients are extensive properties they are directly related to the aerosol load. The term "qualitative" was removed.

Page 13726 Line 6 – presumably "greater Lisbon" R: It was changed to "greater Lisbon".

Lines 21-23 – is the Anderson and Ogren correction sufficiently universal for this dataset? (eg. Marshall et al., J. Applied Meteorol. 2005) R: The article by Marshall et al. reports measurements in the remote marine boundary layer where scattering was essentially "... a result of coarse particles consisting almost entirely of sea salt." They indicated that scattering was due to coarse sea salt particles, which is not the case of the aerosol sampled at our site. Therefore the average Angstrom exponent taken from Marshall et al. is 0.5, whereas for Évora is 1.5. The aerosol measured at Évora is of continental/polluted type and therefore the scattering is, in general, dominated by sub micrometer particles. Therefore we think that the A&O correction is suitable for the dataset presented in the manuscript.

Page 13727 Line 1-2 – calibrations "once a year" seem a little slim. You need to demon-

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strate that the calibrations did not vary much from year to year. Otherwise the data within any year will be suspect. R: As already pointed out above, the change of the calibration constants was less than 10% per year. Therefore- we think – there is no need to suspect our measurements.

Line 11 – I suggest to remove “Usual”. Just say “Temperature, relative humidity and wind speed and direction were also: : :” R: It was removed according to the suggestion.

Line 17 – “hydrophilic” rather than “non-hydrophobic”? R: It was changed.

Line 19 – grammar. R: The phrase was modified to “The light scattering humidification factor, f(RH), quantifies the influence of RH on σsp(ïĄň). It is the ratio between σsp(ïĄň) at high and low RH values where the low RH value is considered to be lower than 40 % (...)”.

Page 13728 The discussion of the RH influence begins (line 10) with the suggestion that the measurements “are assumed to be performed essentially with the aerosol under dry conditions: : :”. Then you turn about and say that you make a correction for RH. This discussion needs to be clearer. R: Fig 1. showed the histogram of the frequency of occurrence of relative humidity values inside the nephelometer chamber. It shows that only a few measurements were done under not completely dry conditions. These few measurements might have a very small influence on the statistical results if the RH is not taken into account. Therefore we applied a RH correction. Although this RH correction might not be perfect because it is based on literature values we believe that the corrected values represent better dry aerosol conditions than uncorrected values.

I have a few comments about the classification scheme you use (Table 1) to separate the aerosol types. I have never seen a clean/bkgd aerosol with a scattering coeff of 60 Mm-1 unless it includes sea salt or dust. Since you have defined a dust criterion,we can keep the discussion to sea salt. You also defined the clean/bkgd as any alpha value, and therefore sea salt effects may be included. But what about clean/bkgd for alpha<1? If there is no sea salt influence then what in the clean/bkgd environment contributes to

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a scattering of up to 60 Mm-1? Since you have classified trajectories, why have you not used that to improve your classification scheme? You need to better define the clean/background aerosol and include the trajectory information in your RH correction criteria. R: Figure 1 shows the Angstrom exponent versus σsp(450) for clean/bkgd data. Basically, when the Angstrom exponent is lower than 1 the σsp(450) values are low, essentially below 35-40 Mm-1, therefore not reaching 60 Mm-1 as questioned by the reviewer. Otherwise the regime would tend to fall on desert dust type. As to the classification scheme used in table 1, it was based in previous studies by Elias et al., (2006) as stated in the manuscript.

Page 13729 Lines 14-15 – Why if the distributions are well described by a log-normal would the geometric mean values not be a better measure of the average conditions? R: Median and mean values were reported for the sake of better comparison with other studies. In fact, several aerosol quantities (such as optical depth, mass concentration, scattering coefficients or number concentration) tend to be represented by log-normal distributions. Even though the majority of studies summarize these quantities using means and also (but not always) median values. Geometric mean values are now included in the manuscript.

Page 13731 Is the anything interesting about these time series? Is there any long-term tendency? R: No trend is observed. This issue is now included in the manuscript in section 3.2.1. The marked decrease of scattering in 2008 can be explained by considering the lack of winter data and the absence of summer forest fires. For example, the total area burnt in 2005 ($\sim$426000 ha) decreased down to $\sim$17000 ha; also, this last value is one order of magnitude lower than the 1998-2007 average ($\sim$163000 ha).

Lines 20-21 – grammar. R: The incorrect expression “At a large extent (. . .)” was substituted by “to a large extent. . .”

Line 29 – something other than “globally” needs to be used; it is misleading. R: “globally” was removed from the manuscript and the phrase was changed to: “This means

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that the increase in the aerosol load is mainly associated with. . .”

Page 13732 Line 6 – “and consequently pollution dilution.” R: The expression was changed.

Line 11 –“by far” is unececssary. R: The expression was removed.

Page 13733 Lines 22-24 – From looking at Figure 6, this statement (i.e. “not observed “) is not true for the spring data. R: You are right. The mentioned behavior is much more evident for the winter period. The manuscript was changed.

Page 13734 Line 6 – during instead of along. R: It was changed.

Line 18 – “global” needs to be replaced or explained. R: It meant the absolute minimum. The expression was substituted.

Line 29 – grammar. R: The phrase was modified.

Page 13735, line 28 to Page 13736, line 1 – two “also”s. R: The first one was removed.

Page 13736 What about the M trajectory? It has the lowest overall alpha, suggesting some sea salt influence, which is maybe not so high in intensity but certainly highest in frequency. R: We don’t have chemical information and hence we cannot know if the low alphas are related to a possible sea salt occurrence at inland. We agree with the reviewer that sea salt may play a role in explaining low alpha values.

As you state, the AF component also has a peak in the alpha distribution at about 0.7 suggesting that there is some direct dust influence. But you then dismiss it. It may not be dominant in terms of alpha frequency, but you need to demonstrate how the scattering frequency corresponds. E.g. the AF scattering freq has a mode about 110 Mm-1. Does that correspond with the lower alpha? R: No, there’s no correlation between σsp(550) and α as shown in the figure 2. The highest values of σsp(550) correspond to values of α ranging from 0 up to 2.

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Interactive Comment

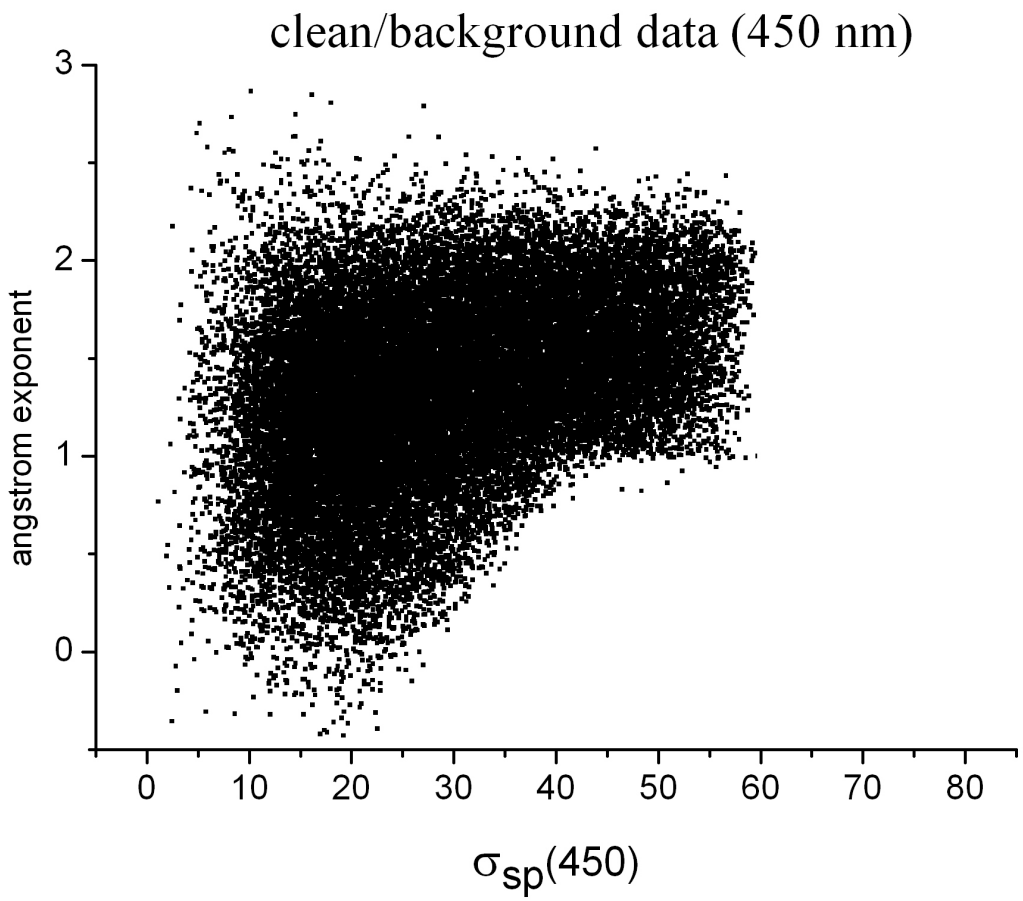

Fig. 1. Angstrom exponent versus σsp(450) for clean/bkgd data

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