Atmos. Chem. Phys. Discuss., 13, C858–C861, 2013
www.atmos-chem-phys-discuss.net/13/C858/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Relating aerosol absorption due to soot, organic carbon, and dust to emission sources determined from in-situ chemical measurements" *by* A. Cazorla et al.

Anonymous Referee #3

Received and published: 31 March 2013

General comments

In this paper authors propose a methodology aiming at chemical speciation of solar radiation absorbing aerosols. More specifically, as an extension to the study by Russell et al. (2010), the methodology achieves this goal based on wavelength dependence of aerosol optical properties, namely by correlating between Absorption Angstrom Exponent (AAE) and Scattering Angstrom Exponent (SAE). The methodology is applied to several year data from 10 AERONET stations in California and authors validate it by making a synergistic use of in-situ measurements of aerosol optical properties and single particle chemical composition measurements.

The paper is interesting and contributes to the reduction of existing uncertainties related to aerosol radiative forcing, and more specifically that associated with absorption of solar radiation. It aims at the discrimination between the main absorbing aerosol types, i.e. dust and carbonaceous aerosols, and moreover between elementary and organic carbon, EC and OC, respectively. It also assesses the limitations of this technique through the attempted validations against in-situ measurements, despite their own limitations.

I find that, despite its limitations, the applied methodology to ground based remotely sensed AERONET aerosol optical properties and the obtained results are reasonable. The limitations are more or less expected given the nature of columnar remotely aerosol data.

My only concerns are:

(i) The choice of AAE and SAE thresholds in scatterplots

(ii) The organization of the paper

These are explained in more detail below whereas a few minor comments are also given.

Main Comments

1. Figure 1, where the methodology is explained and also obtained results for 33 globally distributed AERONET stations are shown, displays the so-called Angstrom matrix. More specifically, the different aerosol types are discriminated through scatterplot analysis of AAE and SAE, setting specific thresholds to both of them. However, a question arises here as to the choice of these thresholds: (i) how these thresholds were defined/decided? This is not discussed in the text. (ii) what are the uncertainties/errors in the measurements used and how these can affect the classification? As stated in lines 16-17 of page 3454 of the manuscript, these 33 AERONET sites have dominant aerosol species. Nevertheless, it should be taken into account that apart from domi-

nant aerosol types influences by other types are possible as well. For example, it is known that such influences are seasonally dependent. Has any care been taken in order to minimize this influence, for example through screening procedures applied to data and excluding specific seasons?

2. I find that the organization of the paper can be improved. In its current form, the methodology and data section already gets into the presentation and discussion of obtained results. This can bring the reader into a little confusion, though a more traditional methodology presentation, referring to existing knowledge and clarifying the added new one here and focusing to the specification of decided thresholds (as stated above), would be preferable. This would leave the paper with

Minor Comments

1. Sub-section 2.1, page 3457: it is reported that, and presentation of results is based on that, data are divided by seasons, grouping together winter and spring, and summer and autumn. It should be explained why such a grouping is applied and what are the criteria for this selection. There should be specific reasons related to specific aerosol regimes in the study region, but these are not discussed at all.

2. Related to the previous point, probably it is useful to say a few words at the end of sub-section 2.1, on the consistency of the first findings. This will be helpful to the readers who are not familiar with the prevailing aerosol regime in California, and does not need to be detailed but just based on knowledge from existing literature referring to natural and anthropogenic aerosol emission sources and transport processes in the region.

3. Sub-section 2.2, page 3458, second paragraph: "PSAP data were corrected . . . ". Please explain what the corrections were applied for.

4. Sub-section 2.2, page 3459, 2nd paragraph: "Spectra are grouped . . . ". A few words about this will be helpful.

5. Sub-section 2.2, page 3459, lines 16-17: Please explain the units in axes x and y.

6. Sub-section 3, page 3463, line 3: replace ". . . burning sources respect to the" by ". . . burning sources with respect to the".

7. Sub-section 3, page 3463, lines 12-13: here it is stated that dust is not expected to make large contributions in urban areas. However, this is not true under specific conditions, e.g. areas in proximity of great deserts or undergoing dust transport. Also, it should be taken into account that the results refer to the whole atmospheric columnar aerosol loading, and it is possible to have transported dust aloft. Some reference to this is being made at the end of this paragraph. Nevertheless, for making statements like the one at this part of the paper, such dust transport cases should be excluded. This can be done based on prevailing synoptic conditions or findings in existing literature.

8. Sub-section 3, page 3465, line 4: replace ". . . indicating that the those were . . ." by ". . .indicating that those were . . .".

9. Sub-section 3, page 3466, line 27: replace "absorption due OC, which . . ." by "absorption due to OC, which . . .".

10. Table 3: with regards to the 3 aircraft campaigns, outlined in this Table, it should be noted that all three only partially cover the year, e.g. from late winter to early summer. A comment should be made with respect to this, namely on whether this is a problem or not as to the representativeness of regional aerosol regime, in terms of validation of proposed methodology.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3451, 2013.