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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Column-integrated aerosol microphysical properties from AERONET Sun photometer over Southwestern Spain" *by* N. Prats et al.

Anonymous Referee #2

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General Comments: The manuscript deals with aerosol micro-physical particle properties derived from sun photometer measurements using the AERONET inversion procedure. The measurements were analysed for 1 station only, but the measurements cover about 10 years. The manuscript follows previous works which investigated the aerosol optical depths (AOD) and the Angstrom exponent (AE). And it is planned to publish another paper focusing on aerosol types. It is questionable to split measurements of 1 instrument at 1 site and respective data analysis into 3 different parts and publish them separately.

Specific comments p.18351, lines 13-16: "thanks to the collaborative effort of the At-

mospheric Optic Group of the Valladolid University (GOA-UVA), Instituto Nacional 15 de Tecnica Aeroespacial (INTA) and the Laboratoire d'Optique Atmosphe′rique (LOA) of the University of Lille." This comment should be moved to the acknowledgement section

p.18351, lines:19/20 "This site is representative of the regional area of Southwestern Europe." Without proof this sentence is just a assertion and should be deleted. Otherwise a reference to a proof or a proof is needed.

p.18352, lines 8/9 add surface area concentration to "between number and volume concentration"

p.18352 lines 19/20/21 the sentences can be shortened. There is no need to repeat twice that El Arenosillo is surrounded by pine trees.

p 18352, lines 21/22 "giving a homogeneous and low surface albedo along the year" please point out, how the surface albedo is homogeneous if the station is close to the water shore.

p. 18355/18356 I don't understand which inversion algorithm was used, version 1 or version 2? If only version 2 was used, the text could be shortened significantly. But if both versions were used, a discussing of potential errors introduced by the use of different versions should be done and how this affects the results.

p. 18358, lines 8-10 It is not obvious from figure 2 that the radius of the maximum particle concentration increases with decreasing particle distribution concentration. It is well visible for the two extremes given in the text (August and December). I am missing a discussing how the errors of the inversion procedure influence the accuracy of the size distribution and consequently on these findings.

p. 18358, lines 11-13 "it is clear .... is not sufficient ... a significant correlation between these values." This sentence should be re-phrased. If the correlation cannot be determined than the behaviour is not clear. However if the authors use the word "significant"

ACPD

11, C8500–C8503, 2011

as a statistical term they have to provide the level of confidence they applied.

p. 18359, line 7 "total concentration" change to "total volume concentration"

p. 18359/18360 the discussion is not complete and would benefit if the authors would discuss the influence of different aerosol types with different properties (e.g. forest fires were omitted). But the authors want to discuss aerosol types in a different paper. I recommend to re-think this intention and to include the discussion of aerosol types into this manuscript.

p. 18362, line 12 "number or volume particle size distribution" add: surface area concentration

p. 18362, lines 19-22 "In this histogram we observe a bimodality with a minimum at AE=0.75, separating situations where coarse particles predominate in number (AE<0.75) from situations with the dominance of fine particles (AE>0.75)." This conclusion cannot be drawn! First because no analysis of particle number distribution and AE was done and included in the histogram. The provided threshold is based on AE alone. Second: although the AE is an indicator for the relative importance of coarse or fine mode particles, it cannot be concluded that the coarse mode dominate in number concentration. The statement might be almost correct for volume concentration. I dont want to discuss the limits and accuracy of the inversion for small particles (smaller than 0.1 um) and the influence on such a statement. But I suggest that the authors convert several volume size distribution into a number concentration distributions and verify/falsify their statement. And third, the authors argue in the following lines with the volume ratio Vf/Vt and not with the number ratio Nf/Nt.

p. 18363, lines 1-3 again, the authors comment on number concentrations but use volume concentrations as arguments. The is a mixing up between 2 different quantities. The authors should provide the number ratio if they want to discuss the predominance of coarse or fine particles in number, or alternatively the discuss the predominance of coarse and fine particles in volume and continue with the volume ratio.

ACPD

11, C8500–C8503, 2011

p. 18365 figures 10 and 11 are related. It seems that in figure 10 a linear function could be fitted between AE and effective radius for the total distribution. Furthermore the authors could add a comment in the final manuscript that the AE indicates the relative importance between fine and coarse mode of the total size distribution and hence a correlation between AE and effective radius or between the volume ratio Vf/Vt and effective radius can be expected but not for either fine mode or coarse mode alone.

p. 18366, line 16 add surface area to "(volume and number)"

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18349, 2011.