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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol optical, microphysical and radiative properties at three regional background insular sites in the western Mediterranean Basin" *by* M. Sicard et al.

Anonymous Referee #1

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General comments: This paper presents a seasonal analysis of the optical, microphysical, and radiative aerosol properties in two insular sites in the western Mediterranean (Ersa and Palma de Mallorca). A third insular site in Alborán is chosen to examine the possible gradients in the aerosol properties between Northeast and Southwest (NE-SW) areas within the Basin. The analysis is based in AERONET measurements and inversion products. The authors conclude that the (NE-SW) gradient were observed in 3 extensive (AOD, CVc and ARFBOA) and 3 intensive (AE, rVc and Sphericity) parameters.

However I have several reasons regarding to the publication of this paper on ACP: The paper is too long and lacks a clear focus. The authors report the data and the

seasonal averages at each site, describing the aerosol properties individually with no interrelation between them. This result in a mostly descriptive paper, that presents a lot of ideas and comments which are not well organized, making the reading difficult. This is reflected in the text and in a many figures which only describe the annual evolution of the aerosol parameters (e.g Fig.2). Some results sections describe how the parameters are obtained (sect. 4.2; 4.4 and 4.5). Please consider a new Methodology section in order to facilitate the paper reading.

In some cases the discussion is mainly based in the literature and the data and figures do not support the affirmations done in the text. (e.g.P19-L8. The sentence: "The influence of European pollution decreases along the NE–SW axis and, logically, the coarse mode volume median radius decreases " seems logical. However the authors do not support this affirmation with their data. e.g. P20-L20: The authors conclude: "We conclude that the differences in the IRI440 values and in the behavior of the IRI spectra are due to a higher influence of mineral dust and/or BrC in Palma" without supporting it with the data they are using)

I see some inconsistences in the analysis of individual aerosol parameters separately. E.g. P24-L23. the authors assert that the asymmetry parameter at Alborán indicates that the scattering direction is driven by marine aerosols. However, when they analyze the SSA (P23-L20-25) the authors affirm that is probably due to the dust mixing with urban/industrial.

I think that the dataset used is too short in order to stablish an aerosol climatology in the western Mediterranean. However, I found that a more in deep analysis of the NW-SE gradient of the aerosol properties would be interesting to investigate the mixing mechanisms in the Mediterranean. Why do not use this data to investigate the NW-SE gradient during the period of simultaneous measurements in the three sites? Why the authors have limited the NW-SE gradient analysis to summer season? I suggest on one hand, synthesize the discussion and statistical analysis the data of the first part of the paper and on the other hand try to answer the question that the authors expose in

last paragraph of the seccion 4.3: “In our opinion two major and interesting questions remain opened: why the absorption properties of the long-range transport aerosol in Alborán are observed neither in Palma, nor in Ersa? What are the processes which inhibits the BC and/or soot absorption properties during the transport to the northern part of the WMB?”

On the other hand, the aerosol forcing is a consequence of the impact of the different aerosol types on the radiative field. If the authors want to validate the AERONET forcing at TOA with the CERES database, I suggest a new paper on this topic using longer data series from more AERONET stations. The results obtained in this paper are compared to those obtained in the literature for Mediterranean and no Mediterranean sites. It could be especially relevant in the case of aerosol radiative forcing since many of these sites to be compared with have a very different surface albedo with respect to the observed in the Mediterranean Islands. Then, the results may be substantially different.

The authors should keep always in mind the dataset they are working with in term of the restrictions imposed to some parameters in the AERONET L2 inversion retrievals. i.e. AOD440nm > 0.4 and 50°< sza < 80°. Under these restrictions most of the studied cases will be mainly due to Saharan dust outbreaks or severe pollution episodes as the authors stated several times throughout the paper (e.g. P9 - L19; P17-L15). This will be reflected in the monthly and seasonal averages only if the frequency and intensity of these events represent a notable fraction of the total number of retrievals passing the aforementioned restrictions. However, no analysis of the frequency and intensity of these events has been done and most of the conclusions are based on that.

Specific comments:

P3-L13: please change “..the fraction fine mode to total AOD. . .” by “ . . .the fine mode fraction to total AOD..” as this parameter is usually named

P4-L26: please change “..the fraction of fine mode to total AOD. . .” by “ . . .the fine

mode fraction to total AOD..” as this parameter is usually named

P12-L6: please change “There is relative few aerosol measurements..” by “ There are relative few aerosol measurements..”

P14 – L6: The authors state that the AOD maxima observed in Fig. 3 for Ersa and Palma are related to mineral dust outbreaks. However there is nothing in Fig.3 supporting this affirmation. The monthly mean AE values are higher than 1, which is mainly associated to the presence of small particles (Eck et al., 1999), or mixed cases (Pace et al., 2006; Schuster et al., 2006). What basis have been used to assert this idea? The author should be explained better.

P14-L26: It is really difficult to use AE in a monthly basis to classify the aerosol type, since the monthly statistics tends to smooth the values. Since the Ersa and Palma show AE higher than 1 for almost all months and also slightly different between both sites, I think that is not possible to differentiate the aerosol type asserting: “The slightly higher values in Ersa compared to Palma indicate the presence of finer particles at Ersa throughout the year”. Can the author support this asseveration using other arguments? I think that a rough aerosol classification using AOD and AE make sense only using instantaneous measurements.

P19-L20: Why the authors do not compare the dust refractive index provided by AERONET with those values obtained for the dust layers during the Charmex flights (e.g Denjean et al., 2016)?

P22-L5-L20: I think that the description of the SSA spectral behaviour is too detailed and difficult to follow. The authors try to observe differences between the sites and seasons that I think they are within the SSA uncertainties. The SSA differences for 440nm among sites and seasons could be representative of different absorbent aerosol types. However the spectral behaviour for larger wavelengths is nearly flat. I suggest to shorten the discussion reducing it to the essential which is observed in Fig. 8.

P23-L9: I am not sure if the differences in time and aerosol volume sampled between the Nakajima code and AERONET retrieval can be reflected in a such way using a monthly statistics. I believe that these differences are mostly due to the different algorithms.

P24-L10: The authors assert: ". . ..at constant AOD the solar radiation scattered to the surface is greater for mineral dust than for urban/industrial aerosols." And this is not totally true since it is dependent on the solar zenith angle. Larger asymmetry parameter indicates larger forward scattering. Since the AERONET almucantar measurements are done at sza > 60°, most of the scattered radiation is returned back to space, reducing the scattered radiation reaching the ground surface (e.g. di Sarra et al., 2008)

P.25-L.20. It is not clear for me if the authors have used the AERONET fluxes retrieval or they used their own calculations. Can you explained better? If the flux retrieval have been done by the authors it should be interesting to have a brief description of the used methodology. If not, why the authors start the sentence with: "Similar to the AERONET retrieval approach.." it shouyld be changed by "The AERONET retrieval approach. . .".

P35-L19. (43% of them). . .I think that it should be the same value in P35-L19 than in P35-L24

P37-L2: The AERONET product comparison is not carried out during the 4-year period (2011-2014). As we can see in Fig. 1 there are no more than 2-years of simultneous measurements at Ersa and Palma.

P38-L13: The authors assert "the gradient of rcV (a decrease along the NE–SW axis) reflects the decreasing influence of European pollution along the NE–SW axis". I think it is an affirmation too strong for the analysis of a single parameter, and only in summer. I see no clear relationship with the European pollution

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