General comments from the authors

First of all we would like to express our great acknowledgments to both referees for their time in revising our paper. In view of their major critics the authors had to modify substantially the paper. As a result the revised manuscript is quite different from the first submission. Because of the many changes, two versions of the paper are sent: one with all the visible changes (as supplement), and one with all changes accepted.

The revised manuscript deals with the comparison of monthly and seasonal averages of AERONET products at two sites in the western Mediterranean Basin: in Ersa and Palma. In this version Alborán is left for the very last section in which only temporally coincident measurements at the three sites are compared to examine possible North-South gradients. The authors would like to address three comments, all made by both referees, in a common way:

Representativeness of the AERONET dataset considered in the paper

In order to strengthen the representativeness of the dataset a series of actions have been led:

1. The dataset, originally from 2011-2014, is now also including 2015, so that it is based on a 5-year period. In that period more AOD level 2.0 data are available in Ersa, but not in Palma. And unfortunately level 2.0 product inversions are still not available for 2015.
2. As mentioned in the short comment posted by the authors in the Interactive Discussion on 11 March, AERONET data were checked against satellite data for the period 2011-2014 (satellite data are not available yet for 2015). MODIS, OMI and MISR AOD extracted daily in Ersa and Palma were compared, as well as OMI and MISR AAOD and SSA. The results are unfortunately not as good as expected and for that reason they are not included in the revised manuscript. However we would like to comment quickly our results. Fig. 1 shows the comparison between AERONET AOD daily mean and the satellite AOD in Ersa. AERONET AOD were calculated at the satellite wavelength (550, 555 and 500 nm for MODIS, MISR and OMI, respectively) from AOD_{440} and $AE_{440\text{-}870}$. One sees large discrepancies between both ground- and satellite-based AODs. Satellite AODs are in general higher than AERONET AODs. The same tendency was shown on SEVIRI data recently over the Balearic Islands by Chazette et al. (2016) who found a systematic overestimation of SEVIRI AOD of 35 % compared to AERONET. Chazette el al. (2016) explain that aerosol mixing causes this difference by making difficult the identification of the proper aerosol model in the satellite retrieval. Fig. 2 shows the monthly AERONET AOD_{440} (like in the paper) and the monthly means of the satellite AODs in Ersa. The shape of OMI AOD is similar but the values are much higher (bias > 0.05). OMI and MISR AODs seem anti-correlated to AERONET. Similar discrepancies are observed in Palma. OMI and MISR AAOD are underestimated compared to AERONET, which results in an overestimation of the satellite SSA. In addition very few points are available for comparison given the AERONET restrictions ($50 < SZA < 80º$ and $AOD_{440} > 0.4$).
   All in all we decided to leave the comparison with the satellite data out of the paper, and maybe address the subject of the validation of satellite products related to the aerosol absorption properties in another paper.

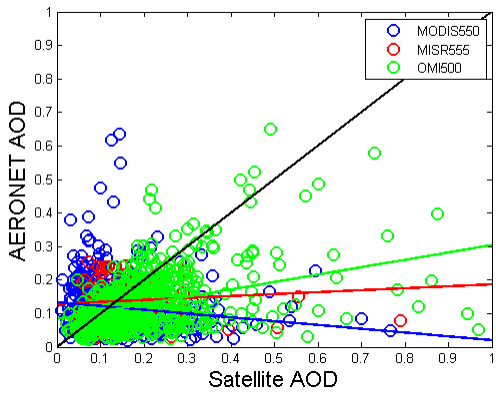

Fig. 1

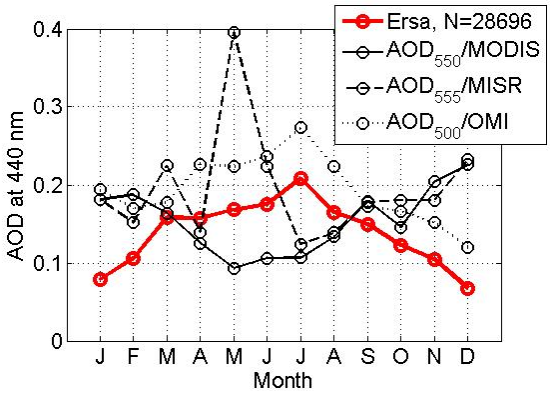

Fig. 2

3. With the new Ersa dataset (2011-2015) each monthly mean is computed with at least 3 to 5 years of data. In Palma each monthly mean is computed with 1 to 3 years of data. To check the representativeness of Palma data we have computed the monthly mean in Ersa for the same days than the Palma dataset included in the period August 2011 – December 2013, and superimposed it on the monthly means calculated over the whole Ersa dataset. Considering only the period of the Palma dataset leads to negligible differences for January and from May to December. The largest differences, up to -0.04, are found during the spring months. We suspect the Palma dataset to lead to an underestimation of the mean values during spring and warn the reader about the limitation of our climatology during that period.

Frequency/intensity of the high AOD events

Another critical point addressed by both referees is the characterization of the aerosol types mentioned all along the paper (initial version). In the revised manuscript a graphical classification method based on Gobbi et al. (2007) has been applied to all AOD retrievals (AERONET *.lev20 files) and to all product inversions (AERONET *.dubovik files) for the data with non-NaN values (see Fig. 2, 3, 6 and 11(a)-(c) in the revised paper). The graphical method is based on (δAE vs. AE) plots superimposed onto a grid of theoretical constant fine mode radii and fraction of the fine mode contribution to the AOD at 675 nm. δAE represent the Ångström exponent difference ($\delta AE = AE_{440\text{-}675} - AE_{675\text{-}870}$). With the dataset corresponding to the AOD retrievals, although the condition $AOD_{675} > 0.15$ is applied to minimize errors in the δAE calculation, the (δAE, AE) plots show the signature of almost all aerosol types with the added difficulty of mixed cases. With the dataset corresponding to the product inversions, much less points are left (49 in Ersa and 82 in Palma) and they fall within two well defined clusters corresponding to mineral dust and pollution. For this dataset we have quantified the frequency of mineral dust and pollution cases with their number (absolute and relative) of occurrences per season and their intensity with the seasonal mean AOD_{440} for each of both aerosol types (see Table 2 in the revised paper). The graphical method is presented in a new dedicated section (Section 3). The analysis of the graphs related to the AOD data and the product inversion dataset have been added at the beginning of Sections 5.1 and 5.3, respectively.

North-South gradients

North-South gradients are explored in a new dedicated section (Section 7) at the end of the paper, where all three stations (Ersa, Palma and Alborán) are considered. In this new section, and as suggested repeatedly by Referee #2, we have looked only at temporal coincident measurements in the period August – December 2011. Outside this period, at least one of the three sites does not have data available. This section is presented as a case study of possible North-South gradients during the period August-December 2011. During this period a similar number of AOD retrievals (~3700 – 3900) is available at each site. In turn, for the product inversions the number of measurements available (3 in Ersa, 7 in Palma and 5 in Alborán) is statistically poor and for that reason the AERONET inversion products based on that statistics (AAOD, AAE, RRI, IRI and SSA) are not analyzed in the paper. It is a pity, but this is what one can expect from case studies: they have their limitations. We believe anyway that the discussion of the differences between Ersa and Palma observed for AAOD, AAE, RRI, IRI and SSA is already representative of the temporal and spatial distributions of these parameters, and their complexity, in the western Mediterranean Basin.

References

Chazette, P., Totems, J., Ancellet, G., Pelon, J., and Sicard, M.: Temporal consistency of lidar observables during aerosol transport events in the framework of the ChArMEx/ADRIMED campaign at Menorca Island in June 2013, Atmos. Chem. Phys., 16, 2863-2875, doi: 10.5194/acp-16-2863-2016, 2016.

Gobbi, G. P., Kaufman, Y. J., Koren, I., and Eck, T. F.: Classification of aerosol properties derived from AERONET direct sun data, Atmos. Chem. Phys. 7, 453–458, 2007.

Answer from the authors to Referee #1

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.
Authors' reply: As said in our general comments, the paper has been completely restructured and revised in such a way that most of the text is new compared to the initial submission. Figures 1-10 and Tables 1-3 refer to the seasonal variations of a series of AERONET products in Ersa and Palma. Figure 11, refer to the case study used to explore possible North-South gradients. A new Methodology section has been added which describes the graphical method used to classify the aerosols. Part of introduction has been re-written to make clearer the focus of the paper. Most of the re-writing has been made trying to link as much as possible the findings found along the way.

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).
Authors' reply: The analysis of the figures about seasonal variations is made in parallel with the aerosol classification obtained with the new methodology (Fig. 2, 3 and 6 and Table2). In particular with Fig. 6 and Table 2 we quantify the frequency and intensity of the two aerosol types which contribute to large AODs (pollution and mineral dust).
The sentence: "The influence of European pollution decreases along the NE–SW axis and, logically, the coarse mode volume median radius decreases " has been kept in the text. We now demonstrate that relatively more European pollution episodes occur in Ersa than in Palma and that the coarse mode volume median radius decreases from Ersa to Palma. Dubovik et al. (2002) findings about dust and urban aerosol coarse mode radii (recalled in the text) make the logical between our two findings.
The second sentence "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" has been removed since we have no information allowing us make this hypothesis.

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.

Authors' reply: In the revised manuscript the aerosol classification for SSA allows us to distinguish between pollution and mineral dust and therefore to discuss the results having always in mind the type of aerosol (mineral dust or pollution) or mixing (mineral dust + pollution) concerned. The two conclusions cited by the referee have been deleted in the revised manuscript. Let's note anyway that mineral dust and pollution influence very differently the seasonal mean of SSA and *g*. In the new Fig. 8a1 the SSA spectra of the whole dataset is very close to that of mineral dust, while in Fig. 8b1 the spectra of *g* of the whole dataset seems in the middle of the spectra of mineral dust and pollution, and even a little closer to pollution.

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?"

Authors' reply: Please see our general comments and in particular the paragraph "Representativeness of the AERONET dataset considered in the paper". The dataset of Ersa has been enlarged and has been used to check the representativeness of the one of Palma. NW-SE gradients are now examined only in the last section of the paper (2 pages) as a case study and many parameters of interest for investigating mixing mechanisms (AAOD, SSA, etc.) are not shown because of a too low statistics (see again our general comments). The authors believe that investigating mixing mechanisms (one of the goals of ChArMEx) is a crucial issue for studying the aerosols in the Mediterranean that can be assessed in dedicated field campaigns with in-situ measurements, among others. Our paper aims at showing the result of this mixing, and we believe that investigating further mixing mechanisms should be left for a new paper, in addition to the one from Denjean et al. (2016), also maybe using ChArMEx summer 2013 field campaign.

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.

Authors' reply: Validating the aerosol forcing at the TOA has appeared to us a necessity to follow on with the discussion on the AERONET forcings at the TOA. We are planning a new paper on the validation of AERONET forcings at the TOA at more sites and with longer data series. The non-Mediterranean sites considered in the paper have been selected based on the aerosol type they are representative of and which are also found in Ersa and/or Palma.

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 º< sza < 80º. 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.

Authors' reply: Please see our general comments about Frequency/intensity of the high AOD events! The graphical method based on Gobbi et al. (2007) has been used to classify the aerosol. The method applied to the AERONET product inversions (parameters like AAOD, AAE, RRI, IRI and SSA) has shown that the restrictions AOD_{440} > 0.4 and 50 º< SZA < 80º led to 2 well-differentiated types of particles: pollution and mineral dust. Table 2 gives some number to quantify the occurrence of both aerosol types and their intensity (in terms of AOD).

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

Authors' reply: This formulation has actually been replaced by " ...the fine mode fraction of total AOD.." in the entire manuscript. We hope this is what the referee meant.

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

Authors' reply: This formulation has actually been replaced by " ...the fine mode fraction of total AOD.." in the entire manuscript. We hope this is what the referee meant.

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

Authors' reply: This formulation has been changed in the entire manuscript. Thank you!

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.

Authors' reply: We have now information available on the frequency and intensity of mineral dust and pollution events for each season of the year (Section 5.3, Figure 6 and Table 2). With these results we now assert that the AOD maxima "are due to a combination of mineral dust outbreaks and pollution events in Ersa and mostly to mineral dust outbreaks in Palma (see the seasonal aerosol frequency and classification in Section ¡Error! No se encuentra el origen de la referencia.**)". The fact that the AE stays above 1 even in summer means that if mineral dust outbreaks increase (in number), then episodes with smaller aerosols (like pollution or biomass burning) also increase. Here again mineral dust does not influence the two parameters AOD and AE the same manner: while AOD is greatly influenced by the increase of mineral dust episodes, AE is not.**

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.

Authors' reply: In the graphical method used in the revised paper each point is precisely an instantaneous value of (δAE, AE, AOD). The rough aerosol classification mentioned by the referee can be found in the revised manuscript at the beginning of Section 5.1 (and Figures 2 and 3).

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)?
Authors' reply: We now compare our results to Denjean et al. (2016) the refractive index (RRI and IRI) and for the single scattering albedo. Thank you!

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.
Authors' reply: This section has been significantly reduced. SSA is now determined for both mineral dust and pollution and their mixing.

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.
Authors' reply: This part of the paper has been deleted.

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 > 60º, 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)
Authors' reply: Placed in a general context, not only in the context of AERONET measurements, this sentence has been replaced by "This result implies that at near-infrared wavelengths (λ > 670 nm), constant AOD and low SZA the solar radiation …".

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 should be changed by "The AERONET retrieval approach...".
Authors' reply: This was a mistake in the text. The sentence starts now with "In the AERONET retrieval approach, …". Thank you!

P35-L19. (43% of them)...I think that it should be the same value in P35-L19 than in P35-L24
Authors' reply: This Section has been removed.

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 simultaneous measurements at Ersa and Palma.
Authors' reply: The phrasing has been modified in the abstract and in the conclusion.

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

Authors' reply: The authors agree with this comment that the affirmation is too strong. It has been deleted in the revised manuscript.

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Pace, G., di Sarra, A., Meloni, D., Piacentino, S. and P. Chamard, Aerosol optical properties at Lampedusa (Central Mediterranean). 1. Influence of transport and identification of different aerosol types, Atmos. Chem. Phys., 6, 697–713, 2006.

Schuster, G. L., Dubovik, O., and Holben, B. N., Angstrom exponent and bimodal aerosol size distributions, J. Geophys. Res., 111, D07207, doi:10.1029/2005JD006328, 2006.

Eck, T. F., Holben, B. N., Reid, J. S., Dubovik, O., Smirnov, A., O'Neill, N. T., Slutsker, I. and Kinne, S., The wavelength dependence of the optical depth of biomass burning, urban and desert dust aerosols, J. Geophys. Res., 104, 31,333– 31,350, 1999.

Answer from the authors to Referee #2

Summary: Authors don't understand what to put in figure captions. All caption lack details to allow the figures to be understood.
Authors' reply: In the revised manuscript we have tried to detail as much as possible the figure and table captions, saying where the data are from, the criteria applied, etc. The inconvenient is that some captions are quite long.

I'm am confused about what microphysical properties are analyzed. Typically, microphysical properties is measurement of the aerosol size distribution not just a fit of the size distribution.
Authors' reply: The microphysical parameters analyzed in the paper are the volume concentration and median radius of both fine and coarse mode of the retrieved AERONET particle volume size distribution. The AERONET size distribution is given in 22 radius bins without being fitted (Figure 5). The fit to a lognormal size distribution, made internally in AERONET and not a posteriori by the authors, is the only way to quantify both the volume concentration and median radius of each of the two size modes.

Captions need to define what is given in the figure and text provide interpretation. Paper need to be revised to do this. Caption do not define what is in the figure and text contains some of this information.
Authors' reply: Please see answer from 2 comments above!

Conclusion states that the frequency and intensity increases along the NE-SW axis; however, paper does not present anything that address if it is the frequency or intensity that gives the gradient. Why is it both, why not just one ot the other.
Authors' reply: As said in our general comments, an aerosol classification method has been applied to AERONET measurements and the revised manuscript counts now with information on the frequency and intensity of mineral dust and pollution events for each season of the year at both Ersa and Palma (Section 5.3, Figure 6 and Table 2).

Conclusion states that AOD and ARF annual cycles are well correlated. How could this be otherwise. AOD is major factor in ARF so they have to be correlated. How is this a conclusion of the paper. It just follows from the equation to calculate ARF.
Authors' reply: The conclusions have been totally re-written and this statement was deleted.

Page 37, line 27. "This result" be direct and state what is being talked about. Do not understand this conclusion.
Authors' reply: The conclusions have been totally re-written.

Paper does not define what is needed for gradient in the summer means? Seems to be a decrease along all three sites. Looking at 18 parameters, and getting 6 to show this seems to be just the result of luck. Most parameters are with in the standard deviation. How unlikely is it to get this result be chance assuming the values are randomly distributed?
Authors' reply: This Section of the initial version has been deleted in the revised manuscript.

Paper states repeatably that gradient is related to higher frequency and intensity. It would be nice to know how frequency and intensity contribute. Is the northerly site frequency of dust events that is important and the intensity the same or is the frequency similar and intensity less?
Authors' reply: Please see answer from 2 comments above!

Paper really lack data for a long-term climate average. There is only the last half of 2012 where all three sites have data. This is where the treads need to be computed from. Don't mix data

from different time periods, the data set is not long enough. For example, on Page 38, line 18, states that "we have observed a homogeneous spatial distribution (except during the month of March and April) of the fine particle loads over the three sites". How is this possible, figure 2 indicates the there is no data in March for one of the sites. Hence, the data does not support the conclusions of the paper.

Authors' reply: This sentence in particular has been deleted in the revised manuscript. Please see the paragraph "Representativeness of the AERONET dataset considered in the paper" in our general comments where we explained how we enlarged the database and how we checked the representativeness of Palma database which is shorter than the Ersa one.

Paper lacks focus, tries to confuse the reader with a lot of plots that don't contribute to the paper's conclusions. I see nothing in the paper that is not better supported by other papers; hence nothing new.

Authors' reply: We are sorry for the confusion. The novelty of the revised manuscript lies in the analysis of absorption properties (AAOD, AAE, SSA, RRI and IRI) separately for mineral dust and pollution and for the total (mineral dust + pollution) and in the validation of AERONET radiative fluxes at the top of the atmosphere.

I would suggest just focusing on the time period when there is data from all three sites and determining to what extent frequency and intensity contributes to different in AOD for dust events. Another option is just focus on two sites and the time period of them sites. It is confusing to include both.

Authors' reply: In the revised manuscript we present the two options suggested by the referee: 1) comparison at two sites of monthly and seasonal means of all AERONET products available during the 5-year period 2011-2015 (as said in our general comments Year 2015 was added to the dataset in the revised manuscript); 2) a short case study (2 pages) of temporally coincident measurements at the 3 sites. Alborán data are just in the case study.

The Solar Radaitive forcing comparison should be in it's own paper.

Authors' reply: Validating the aerosol forcing at the TOA has appeared to us a necessity to follow on with the discussion on the AERONET forcings at the TOA. We are planning a new paper on the validation of AERONET forcings at the TOA at more sites and with longer data series.

The pyranometer measurements don't seem to be openly available. Also, none of the software used in the analysis is openly available. The lack of data and software make it impossible to reproduce results. Furthermore, the method used is not fully described.

Authors' reply: The pyranometer measurements were taken from the Solar Radiation Network (SolRad-Net) Data Display at http://solrad-net.gsfc.nasa.gov/cgi-bin/type_one_station_flux?site=Barcelona&nachal=0&year=17&aero_water=0&level=1&if_day=0&shef_code=P&year_or_month=1, and are openly available.

The graphical method used to assess the aerosol classification has been thoroughly described and referenced in the text.

For generating monthly and seasonal figures, we did not use any particular software. We made use of simple Matlab routines, written by ourselves, to read AERONET text format files (*.lev20, *.dubovik, etc.) and to calculate monthly means, seasonal means, standard deviations, number of points, etc.

Figures and Captions: Figure 2: y-axis labels should have same accuracy. Hence 1.0 and not 1. 'Year' text is way too small, likewise 1.82 text.

Authors' reply: We made an effort to enlarge all sub-figures' fonts (titles, axis labels and legends). We have used by default a font size of 22 for the titles, and 20 for the axis labels and

legends. In some sub-figures the legend font size was reduced because of too little space available in the sub-figure. However, once inserted in one of the figure of the revised manuscript, all font sizes do not appear the same because each sub-figure is reduced differently depending on the number of sub-figures contained in the figure.

Figure 3: Month font is too small. Use solid circles. Give y-axis labels to same scale.
Authors' reply: The font size of "Month" has been enlarged. All circles have been replaced by solid circles (except in Fig. 9, in order to be able to distinguish circles that almost overlap). Y-axis scales was set equally in the new figures of AOD and AOD^f.

Figure 4: All fonts too small. Can't read or understand figure.
Authors' reply: Please see answer from 2 comments above!

Figure 5: All fonts too small. Either the label or the units are wrong on the figure. It is either dV/dr [umˆ3/umˆ-2] or dV/dlnr [umˆ3]. How are these volume size distribution determined? What months make up the season? How are the averages determined?
Authors' reply: The authors would like to refer to the document available online at http://aeronet.gsfc.nasa.gov/new_web/Documents/Inversion_products_V2.pdf which describes the AERONET inversion products. In the text it is referenced as AERONET (2016). There, we find the definition of the particle size distribution in terms of volume concentration dV(r)/dln(r) as a function of the number concentration dN(r)/dln(r) (Eq. 2):

$$\frac{dV(r)}{dln(r)} = V(r)\frac{dN(r)}{dln(r)}$$

This quantity is usually expressed in $\mu m^3 \cdot cm^{-3}$.
In the case of a discrete size distribution (which is the case of the one of AERONET), the volume concentration is not given for a single value of *r*, but for an interval Δr, hence the unit of $\mu m^3 \cdot cm^{-2}$ which is later converted to $\mu m^3 \cdot \mu m^{-2}$. Strictly speaking one should define the volume concentration as dV(r)/dln(r) * Δr, however it never appears this way in the literature.
The total volume concentration is the integral (the sum in case of a discrete size distribution) of dV(r)/dln(r) over the full range or radii (Eq. 5).

Figure 6: All fonts too small. Use same accuracy of the y-axis values.
Authors' reply: Please see answer from 4 comments above!

Figure 7: All fonts too small. The values are wrong. Can't be a values up to 10,000, must be 10ˆ-3 not 10ˆ3.
Authors' reply: 10^3 was replaced by 10^{-3} in the revised manuscript.

Figure 8 All fonts too small.
Figure 9 All fonts too small.
Figure 10 All fonts too small.
Figure 11 All fonts too small.
Figure 12 All fonts too small.
Authors' reply: According to the font, please see answer from 6 comments above!

Detailed Comments:
Page 5, Line 22-23: Please don't use acronyms WMB, just spell out.
Authors' reply: The acronym WMB for the western Mediterranean Basin was deleted in the revised manuscript.

Page 6, Line 6-21: Delete, no point in just stating what will be talked about. Section headers handle this and a well organized paper.

Authors' reply: The authors believe that the presentation of the structure of the paper at the end of the introduction helps in understanding the focus and objectives of the paper. Many scientific papers, probably a majority, and including ACP, follow this way of doing. The paper organization was kept in the revised manuscript.

Page 6, Line 26: Sites are not on a North-South axis. Seems that you would want sites close to the North African desert region and one far away.
Authors' reply: The sites are aligned along a Northeast – Southwest axis, and the text says "approximately aligned on a North–South axis". The criterion was to have sites at decreasing latitude along a North–South axis.

Page 8, Kine 9: Official reference to Web sites should be given instead of http references in the text.
Authors' reply: Such references have been added in the revised manuscript following ACP guidelines.

Page 8, Line 11: Author really don't understand the data they are analyzing as evident by saying, "the wavelengths at which the almucantar scans are performed." Scans are not done at a wavelength. Filters are used to get these wavelengths.
Authors' reply: This sentence has been replaced in the revised manuscript by: "All the inversion products spectrally resolved are given at 440, 675, 870 and 1020 nm."

Page 8, Line 16, define SZA.
Authors' reply: SZA= Solar Zenith Angle. It is defined the first time Solar Zenith Angle appears in the text in page 6.

Page 8, Line 16-18. These restriction to AOD above certain values need to be given in the caption of figures.
Authors' reply: The restrictions on the data presented have been added in all figures' captions.

Page 11, Line 1-6. Good to label the Azores, Bay of Biscay and Gulf of Lion in figure 1. There is wet and dry scavenging. Not clear what is being talked about.
Authors' reply: Bay of Biscay and Gulf of Lion have been added in Fig. 1. We refered to "wet scavenging". It has been specified in the text.

Page 11, Line 26: Space between value and km.
Authors' reply: Done!

Page 13, Line 1: "by the same authors, found in the same range that those for other suburban sites in Spain, suggests an important regional contribution of such aerosols" Be direct, state authors name, the range, what "such aerosol"?
Authors' reply: This sentence has been totally re-written and now says: "Carbonaceous aerosols in Mallorca have been found by Pey et al., (2009) in the same range that those in other suburban sites in Spain, which suggests an important regional contribution of carbonaceous aerosols."

Page 13, Line 15: Where does this dust event frequency come from? Paragraph is out of place.
Authors' reply: This sentence has been deleted in the revised manuscript.

Page 13, Line 20: Captions need to define the figure and text provide interpretation. Paper need to be revised to do this. This line is just one example of material that need to be in caption not in the text.

Authors' reply: We have tried as much as possible to be more direct in the text with the interpretation putting the reference to the figures and tables between parentheses. We also believe that presenting briefly (in less than 1 line) the next figure allows to make a smooth transition between two sections or two paragraphs.

Page 13, Line 25: Can't see annual cycle in figure only the maximum values. Not sure why figure 1 is included. Can just state time period of analysis. No point in figure. Remove or provide some reason for time series figure.
Authors' reply: Figure 2 has been removed in the revised manuscript.

Page 14, Line 2-3: it would be nice to have some analysis showing exactly how often and how much less the intensity differences between the slights. Seem this is a major objective of the paper.
Authors' reply: This question is answered in the revised manuscript thanks to the aerosol classification assessed on the AERONET level 2.0 inversion products. We refer here again the referee to our general comments and in particular to the paragraph "Frequency/intensity of the high AOD events".

Page 14, Line 16-18: What years are these summer averages over. One site only had one year of data. Should not the comparison be for the same time periods. Also, with the large standard deviations, I don't see how you can say there is a gradient. Taking into account the standard deviations, the values are the same.
Authors' reply: It is now indicated in the caption of Table 1. These values are the annual mean of all instantaneous measurements available in the period 2011-2015.
Alborán is not included anymore in the discussion of the monthly and seasonal variations.
For the representativeness of Palma data see the paragraph "Representativeness of the AERONET dataset considered in the paper" in our general comments. The standard deviations are of the same order of magnitude at Ersa and Palma. So we do believe that their magnitude is caused more by the natural variability of AOD at both sites, especially during the seasons with high number of strong dust and pollution events, than by a low statistics.

Page 14, Line 22: I don't see how there is a difference in the seasonal cycle. Need to provide standard deviations. Cycle look the same to me.
Authors' reply: There is indeed no annual cycle visible on the monthly AE. We have used the word "pattern" and by "different seasonal patterns" we mean that the monthly variations at both sites have different "shapes". The standard deviations have been added in the figure.

Page 15, Line 1-2: Don't see the clear gradient. Large standard deviations. Again, is this for the same time period or different.
Authors' reply: This statement has been deleted in the revised manuscript. In general we do not talk anymore about NE-SW gradients in the most part of the paper since it is based only on two sites (Ersa and Palma). Instead of gradient we talk about differences and Table 1 reveals a difference between Ersa and Palma (higher AE at Ersa than at Palma) during all season but especially during spring-summer-autumn, which is briefly commented.
The AE data are treated the same way than the AOD: the monthly means are computed from instantaneous values available in the period 2011-2015. It is clearly stated in the caption of Table 1.

Page 35, line 10: If data from all three sites are required for the gradient, why not just look at the summer of 2012 so the data is comparable. Why includes additional summer values for two of the sites?

Authors' reply: As said in a previous answer: the paper has been totally re-structured with 1) the comparison at two sites of monthly and seasonal means of all AERONET products available during the 5-year period 2011-2015; and 2) a short case study (2 pages) of temporally coincident measurements at the 3 sites. Alborán data are just used in the case study.

Page 35, line 15-20: The parameters analyzed are not independent. SSA and RRI/IRI are related for example. The percentage of parameters that are intensive and extensive are not useful because the parameters are not independent. Also, done of the parameters show a gradient above the standard deviation of the measurements. I suggest that a randomly generated data set would give gradients by looking at parameters using this analysis.
Authors' reply: This section has been removed in the revised manuscript.

Figures and Captions:
Figure 1: Figures are independent of text so all acronyms need to be defined. What is WMB? Define SW and NE. Give credit that this image was made using Google Earth.
Authors' reply: We have defined as much as possible the acronyms used in the figure captions. Credit to Google Earth has been added in the figure caption.

Figure 2: N is number of points but for what? Define AOD and give what time period the measurement is over. Is it an instantaneous measurement of an average? Can only tell the maximum values from plot. Box-and-whisker plots for a week or month would give far more information. Easy to make plot that does not show much information.
Authors' reply: Fig. 2 (of the initial submission) has been removed in the revised manuscript according to referee #2's ssuggestions.

Figure 3: Define acronyms. Monthly average of what time period measurements?
Authors' reply: We have defined as much as possible the acronyms used in the figure captions. The time period has been indicated in the figure caption.

Figure 4: Define acronyms. Define better what the box-and-whisker represent. Minimum and maximum values are defined twice. Season in legend should start with capital letter.
Authors' reply: We have defined as much as possible the acronyms used in the figure captions. All box-and-whisker plots were removed in the revised manuscript. The seasons are defined in the text at the end of Section 3, and were replaces by the first letter in capital of the months forming each season (e.g. summer, i.e. June-July-August = JJA). This nomenclature presents 2 advantages: it is shorter and it is a time reference independent of the Earth hemisphere the reader lives in.

Figure 5: Need to provide more caption information.
Authors' reply: All figure captions have been written with more details.

Figure 6: Don't understand what is the "Same as Figure 4:. Finally, a acronym that is defined RRI, why define this one and not all the others. Where do these measurements come from? How are they determined?
Authors' reply: In the revised manuscript Fig. 3 is the same than Fig. 2 but for another site. However we have followed the referee advice and repeat entirely the figure caption. In all figure captions the measurements used are specified.

Figure 7: Don't understand what is the "Same as Figure 4:. Please just provide the information in the caption. Why confuse the reader?
Figure 8: More details to describe what is in the figure.
Figure 9: More details to describe what is in the figure.

Authors' reply: Please see the previous answer!

Figure 10: Define what a data point is on this plot. How large of an area is the CERES data? What time duration of AERONET data.

Authors' reply: The nominal footptint of CERES is 20km. This can be checked in the CERES web page at https://ceres-tool.larc.nasa.gov/ord-tool/products?CERESProducts=SSFlevel2 in the Spatial Resolution submenu that applies for the TOA fluxes that we downloaded. It was indicated in the initial submission paper in page 27, line 12-13: "for a spatial resolution equivalent to its instantaneous footprint (nadir resolution 20 km equivalent diameter)".

All measurements (pyranometer, AERONET and CERES) are instantaneous measurements. At the surface we compared the closest pyranometer and AERONET measurements within ± 1 min. At the TOA we compared the closest CERES and AERONET measurements within ± 15 min. This is already stated in the initial submission at page 27, line 4 and 16.

The caption of Fig. 9 has been further detailed and now it also mentions the time difference allowed between observed and AERONET fluxes.

Figure 11: Monthly data is given, not seasonal as stated in caption. Define acronyms. Again, how is the data determined?

Authors' reply: "Monthly" now replace "Seasonal". Thank you! Acronyms have been defined and the origin of the data has been explicitly indicated.

Figure 12: Define what time period summer is. Define acronyms.

Authors' reply: Figure 12 (in the initial submission) has been removed in the revised manuscript. The seasons are now defined in the text at the end of Section 3.