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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol radiative effects in the ultraviolet, visible, and near-infrared spectral ranges using long-term aerosol data series over the Iberian Peninsula" *by* D. Mateos et al.

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Dear Editor,

please find below the answers to the remarks of the reviewer 2. We want to thank to the reviewer for his/her constructive comments. Our replies are highlighted with '++' symbols.

The authors

Referee 2

General comments This paper by Mateos et al. presents data on aerosol optical properties and direct radiative effect (ARE) obtained at different Iberian sites in the long-term period 2001-2012. The main aim of the paper is to analyse the trend in aerosol content, properties and their radiative effect during this time interval in order to provide an aerosol climatology over the whole Iberian Peninsula. The ARE has been calculated separately in four spectral regions (UV, VIS, SW, and NIR) and its dependence on the absorption properties and size of particles has been investigated. The objective of the paper is appealing, however I have several comments about data analysis, discussions, and presentation which are reported in the following.

One of my main concerns regards the discussion of the results, which appears very poor and restricted to basic considerations. For instance, in Section 4 last paragraph, you present the results of Figure 4 without practically providing any comment. What is the cause for the trend? The reduction of emissions in Spain? The change in dust outbreaks occurrence? Is there a seasonality in this trend which may help to understand? Is there a connection with climatic indices (NAO, for instance) which may explain part of the interannual variability? Have you investigated that? Similar consideration are related to Section 5, where the discussion does not provide additional elements.

++ Following the reviewer's suggestion, we added new topics to the discussion. The possible causes of the reduction in the aerosol load are given, together with recent studies which support these findings. The relationship between climatic indices and aerosol load goes beyond of the scopes of this study, but we mentioned one particular year which has been proved as very interesting for the atmospheric science community.

My second main concern regards the fact that part of the ARE analysis and discussion does not provide neither new methods nor results. The obtained ARE values and the dependence on SSA, as also discussed by the authors, are in good agreement with

several other studies performed in the Mediterranean basin. So basically the results of this paper confirm things that we already know. In my opinion, the most original and interesting part concerning ARE estimates is the discussion of the different spectral contributions (UV, SW, Vis, NIR), which unfortunately I have found only at the end of Section 6. I suggest the authors to consider reorganize the discussion around ARE estimates, especially in Section 6, in order to better highlight their findings.

++ In order to compare our results with the previous studies, new Table 4 summarizes the findings about AFE values. As the reviewer can see, the time periods of previous studies are shorter, being in most cases only referred to case studies. In our study:

- We highlight the AOD decrease in the 2000s in the Iberian Peninsula, which is relevant to understand the SW radiation increase (the brightening phenomenon) observed in this area (see, e.g., Mateos et al., 2013a). To our knowledge, this is the first study showing the AOD trends at several aerosol sites simultaneously in several sectors of the Iberian Peninsula.

- The method followed in this study with the aerosol properties simplifies the spectral behavior and the evaluation of ARE can be carried out with high accuracy.

- The ARE trends are also obtained (with a high significance level) for the "mean database" in the Iberian Peninsula 2004-2012. Again, this relevant result is necessary to analyze the brightening period.

- The used long-term databases have produced a detailed evaluation of ARE depending on aerosol properties.

- As the reviewer stated, the evaluation by intervals offers the possibility to perform a more complete discussion of aerosol radiative effects.

Third point, the analysis of uncertainties is not completely developed. For instance, can you provide error bars in Fig 4.? Also, it is not clear how the ARE uncertainty has been estimated. A certain number of assumptions have been performed to implement

model calculations (Sect. 3), however the possible effect of these assumptions on the calculated ARE is not investigated (see for instance the specific comments below). To assess these uncertainties is however necessary to better constrain your results. Finally, for what concerns the presentation of data and results, I find that the paper is quite repetitive in some parts, Sections 4 and 5 in particular. Also, I have the impression that Figs. 4-5-6-7 have in part similar “messages”, so probably they could be reorganized in order to merge them into 2-3 figures only.

++ We included the new Appendix A in the manuscript, where the reviewer can find the justification of the choices. The results shown in the Appendix support our methods. We consider that all the figures are necessary because the information they highlight is different depending on the discussion. For instance, Figure 4 shows the yearly evolution of the AOD values at the six sites, before the average series is presented in Figure 6. Hence, we considered that these two figures must be shown in the article. Figure 6 is a summary of Figure 5 considering the average series, and the temporal trend rates are also presented. We decided to maintain all the figures as in the previous version, with the modifications suggested by the reviewer.

Specific comments

Introduction I find that the scientific context and the main objectives of your study are not very well constrained. In particular, the second paragraph is quite confused; it seems for instance that you are interested only on dust, while also other aerosol types are investigated in the paper. I suggest you to revise this part.

++ According to the reviewer’s suggestion, we changed this paragraph focusing on three aerosol types. " With regards to surface SW radiative effect (ARESW), Di Biagio et al. (2010) obtained the maximum radiative daily effects for different aerosol types in the Central Mediterranean in the period 2004-2007: -61 Wm-2 (desert dust aerosols), -26 Wm-2 (urban/industrialâĂŘbiomass burning aerosols) and -43 Wm-2 (mixed aerosols). All these negative figures point out a cooling of the Earth’s surface.

Aerosol radiative effects in the LW (ARELW) are expected to be smaller than in the SW and with positive sign (see, e.g., di Sarra et al., 2011; Antón et al., 2014). "

Section 2, pg. 8785, line 19: this 1% difference should be added to the SSA uncertainty

++ Not necessary, the relative difference between the two databases is 1%, and it is in the range of the uncertainty given by the inversion method of SSA. Therefore, the level 1.5-filtered data seems adequate to perform our study.

Section 2, last paragraph: how the SSA 0.90 and g 0.75 have been chosen? Can you add references for this? How can you state that this choice "provides a good characterization of the aerosol absorption"? Can you evaluate the uncertainty on your estimated ARE based on this assumption? Have you performed sensitivity tests to support your statement? Moreover, in line 27 I would avoid the expression "we think".

++ In the new Appendix A we analyzed this choice. The results indicate that the values of SSA and g under clean conditions (low turbidity) are not relevant.

Section 3, pg. 8786, lines 12-13: I do not understand the meaning of this sentence? It means that in the cited papers (Bilbao 2011 and Mateos 2013) the authors provide with comparison of modeled irradiances with measured data?

++ We changed this sentence: " The libRadtran model (Mayer and Kylling, 2005) has been shown to be a useful tool for obtaining solar radiation data, presenting high accuracy in both cloudy and overcast conditions (e.g., Mateos et al., 2013b, 2014; Román et al., 2014). "

Section 3, pg. 8787, lines 13-14: you assume wavelength independent optical properties in the different considered spectral intervals. Can you provide an estimate of the uncertainty induced by this assumption?

++ The new Appendix A includes this discussion.

Section 3, pg. 8788, lines 11-12: I would eliminate "daily" since the relation is general.

++ Canceled.

Section 3, pg. 8788, lines 11-17: there are several repetitions in this paragraph, please rewrite it in a more concise and clear form.

++ Following the reviewer's suggestion, the paragraph has been rephrased: " The aerosol forcing efficiency (AFE) is defined as the rate at which the radiative effect varies per unit of AOD (e.g., Di Biagio et al., 2009; and the references therein). The linear relationship between aerosol radiative effect and AOD is well known (see, e.g., Costa et al., 2004, 2006; Di Biagio et al., 2009). Hence, in this study, ARE is obtained as the slope of linear fits in the ARE vs AOD500nm relationships. Therefore, AFE values are expressed in W m-2 per AOD500nm-unit (Wm-2τ-1)." Section 4 and Figure 4: can you specify the number of datapoints or measurement days for each year? Are they uniformly distributed throughout the different seasons for the different years? What about cloud cover? It is possible that some differences in the annual values reported in Fig 4 are related in part to specific episodes, such as for example an enhanced cloud cover during specific periods which has affected CIMEL measurements?

++ We added a comment about this topic to the manuscript. The aerosol measurements of CIMEL are only performed under cloud-free conditions, and this fact can produce that some events are only visible in some stations.

Section 4, pg. 8790, lines 5-15: how your classification is in agreement with the selection criteria by Toledano 2007 and Pace 2006?

++ We better explained this topic.

In line 14, what does it mean that "the value could be adjusted to the site"? How? Line 15, there is a repetition.

++ In studies dealing with previous studies, each station present different threshold for the classification using the alpha-AOD diagram. " Note that the limit of AOD440nm < 0.2 is arbitrary and this value could be adjusted according to the sites, which likely

produce a different distribution different distribution in the pie diagrams. Actually, even close stations can present slight differences in the α-AOD classification (see, e.g., Obregón et al. 2012). "

Section 5, pg. 8792, lines 1-2: I do not agree when you say that in the NIR the ARE seems more stable; I have the impression that there are not significant differences between the different plots in Fig 5.

++ We changed this conclusion, we focused it on the inter-annual changes.

Section 5, page 8792, lines 26-27: I guess the larger contribution in the visible is due to the fact that the max of the solar spectrum is found around 700 nm.

++ As the ARE is obtained as a difference between two SW radiation values, the maximum of VIS range in each SW radiation could not be true in the ARE. This is the reason why we emphasized the maximum in the VIS range.

Figures 4 and 6: I suggest adding error bars in the plots.

++ We added to the plots the error bars for one station and one variable, respectively.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8779, 2014.