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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol radiative forcing during African desert dust events (2005–2010) over South-Eastern Spain" *by* A. Valenzuela et al.

Anonymous Referee #1

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This paper deals with African dust direct radiative forcing estimation that relies on aerosol optical characteristics retrieved from principal plane photometric measurements. The presented study utilizes observations at a site in south-eastern Spain and focuses on long-term observations of dust transported from different sources in the northern Africa. The authors report high aerosol absorption that leads to stronger, relative to other studies, radiative forcing. In general, the defined in the paper objectives are important. Particular interest presents usage of principal plane photometer measurements for aerosol optical characterization. At the same time, I have serious concerns regarding poor description of the methodological part. In addition, the paper presents only values of forcing and does not show the actual retrieved aerosol parameters. Instead of providing direct discussion of the methodology and retrieved aerosol properties, the authors provide only references to previous publications. This forces a

reader to search in literature for information essential for proper understanding of what is done in this study. Moreover, even after looking the referred literature it is difficult to find consistency (although it can exist) between presented here forcings and reported in other two papers aerosol properties. In addition, in the referred "methodology paper" I found only about one page long description of the aerosol retrieval algorithm that did not contained the details needed to address all my questions. As a result the paper leaves impression of a non-transparent study, especially because the authors suggest that the main originality of this study is in using their own aerosol retrieval products instead of using the standard retrieval products provided by AERONET. In addition, the authors tend to make conclusions about "excellent agreements" without providing convincing and balanced discussion required for justification of such conclusion. Without proper justification the usage of such terms as "excellent agreements" in the text gives impression of immaturity of the study or its presentation.

In summary, I have concluded that the paper is not acceptable for publication in ACP in its present state and some significant improvements should be done in order to make paper acceptable. The corrections should address my general comments above and the specific remarks below.

Specific comments:

Abstract, line 9: You wrote, "The SBDART modeled global irradiances at surface have been successfully validated against experimental measurements obtained by CM-11 pyranometer, indicating the reliability of the radiative transfer model used in this work for the ARF calculations." You can be more specific and provide the estimated errors.

Abstract, line 13: The values of aerosol radiative forcing are provided, but it is not clear either they are averages of instantaneous values or 24 hours average?

Abstract, line 18: You wrote, "These results suggest that the African dust caused local atmospheric heating over the study location." This obvious conclusion is not necessary to report in the abstract. If you want to write something about atmospheric heating

please provide a value, e.g. x K/day.

Abstract, line 23: Do you provide instantaneous values or an average of these values? If they are instantaneous, then for what SZA? These numbers do not mean much without understanding of how it was calculated.

Abstract, line 23: In abstract you report some results for sectors A, B, C. It is not possible to know what are these sectors without reading the paper. Abstract should be self-sufficient.

Abstract, line 24: You claim that the found forcing values are larger than many of values found in literature due to the presence of more absorbing atmospheric particles during African desert dust intrusions over the study area. Maybe it is simply because of not consistent comparison with values in literature? Maybe literature gives forcing per 24 hours (daily averages), while you give an average during daylight duration only? Maybe solar geometry is different? It does not seem to me that SSA values reported in (Valenzuela, 2012a) are much lower than in many other studies of African dust. Otherwise, please be more specific and clear.

Introduction, p. 6595, line 17: “region” is used twice, please reword.

Introduction, p. 6597-6598: You describe advantages of aerosol retrieval from principal plane observations, but you do not mention at all any limitation. There is not discussion of fitting errors, quality controls, etc.

Introduction, p. 6598, line 5: Something is missing in this sentence.

Instrumentation part, p. 6599, line 11: Here you report the pyranometer spectral range of 310 – 3200 nm. In abstract you report 300 - 2800 nm for global irradiance simulations. Please check. If spectral ranges of pyranometer and of simulations are different, please provide an estimate of uncertainty due to this difference.

Instrumentation part, p. 6599, line 13: You wrote, “the calibration factor stability has been periodically checked against a reference CM-11 pyranometer”. So, what are

results of these checks? How often it was done? The data consists about 5 years. Was it necessary to apply any corrections during this period? Also, as I understand the global irradiance was measured by not shadowed pyranometer. In this case there can be an increase of uncertainty during morning and afternoon due to non-uniform illumination of the detector by the direct solar beam. How significant this uncertainty is? Further on in the paper you report smaller that 4 % difference between simulated and measured irradiances, which is similar or even less than expected accuracy of the measurements. In Figure 1 I see that the differences can be about 10 %. I would suggest showing absolute or relative errors, not a correlation scatter plot where tens Wm-2 errors are not distinguishable on hundreds Wm-2 scale. For example you can show relative errors vs. absolute values of global irradiance.

Methodology and data, p. 6600, line 4-7: Analysis in the current paper heavily relies on aerosol characteristics retrieved from principal plane photometer measurements. The authors state that the method has been previously described by (Olmo et al., 2008). However, Olmo et al., 2008 state in the abstract that “In this paper we test a parameterization of the particle shape in size distribution, single-scattering albedo, phase function and asymmetry parameter retrieval from beam and sky-radiance measurements, based on the model Skyrad.pack, taking into account the principal plane measurements configuration”. After looking through the paper I agree with the statement in the abstract that this paper is rather a test very few cases examined. But, it is not enough only to cite this paper without providing in deep discussion of methodology with comprehensive sensitivity tests and assessment of uncertainties. In any case, the paper by Olmo et al. (2008) is a limited study that cannot serve as a reference paper containing comprehensive description of the retrieval methodology. For example, you say that your method is an improvement of Nakajima et al. (1996) code where the most important change is the substitution of spherical model used in the original algorithm by spheroid model. However, Skyrad described in Nakajima et al. (1996) does not retrieve complex refractive index. How the complex refractive index retrieval is treated in your work? Please describe clearly your methodology of aerosol retrieval before

making conclusions about radiative forcing.

Methodology and data, p. 6600, line 9-15: How well the retrieved spectral aerosol optical properties represent the whole broadband spectral range? Are they only for nominal CIMEL photometer wavelengths? What is assumed for other wavelengths in broadband spectrum?

Methodology and data, p. 6600, line 17: You say that surface albedo is fixed to 0.15. At what wavelength? What is spectral dependence? I guess you account for spectral dependence of surface albedo; otherwise your calculations of forcing at top of the atmosphere are highly uncertain.

Results and discussion, p. 6603, line 18: Interpretation of provided here forcing values is not clear. What is "overall mean"? As far as I understand, these are not 24 hours averages. Why you conclude that these values indicate "significant Earth-atmosphere cooling" and "significant atmospheric warming"? Can you provide corresponding values of heating rate or compare them to the values for some other situation?

Results and discussion, p. 6606, line 6: Why in Fig. 4 you analyze forcing vs. AOD for the SZA lower than 65 degree? Please explain this choice. Can be that a large "cloud" of points in Fig. 4 for TOA is because you take forcings for different SZAs (range 65 to 80 degree) and this is why the correlation is poor. In other words, you have dependence not only on AOD, but also on SZA and therefore the derived forcing efficiency is very uncertain. It seems that correlation for surface forcing vs. AOD is better, which is because of weaker dependence of the forcing on SZA. By the way, why the offsets are removed in the linear regression equations of Fig. 4? Are they equal to zero? How the provided in the text uncertainties of the forcing efficiency were calculated?

Results and discussion, p. 6607, line 10-: Your forcing simulations are for spectral range 300 – 2800 or 3200 nm. Is it the same range as used in AERONET forcing calculations? If not, please check and discuss the influence on the conclusions about the comparison.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6593, 2012.