

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

Anonymous Referee #2

Received and published: 18 April 2012

General comments:

The paper aims at characterizing the aerosol radiative forcing and forcing efficiency due to desert dust that at a Spanish location (Granada) located relatively close to the Mediterranean Sea and in the path of dust-loaded air masses originating in the North African desert areas. Although the topic is sound, the methodology is poorly explained and the paper lacks a rigorous analysis in many senses. The paper would need substantial revision before it could be considered for publication in ACP and I suggest it is rejected and encourage authors to re-submit once they re-write the manuscript and address the concerns that follow below.

1. The result section and the conclusions are full of vague statements (such as ‘show agreement’, ‘high degree of agreement’ which are not demonstrated at all. A real

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quantification is needed to support the results. Of special interest are the differences in ARF and ARFE with respect to the AERONET retrievals, which are not quantified. The same applies to the comparison between model and flux measurements. There are also vague statements and even speculations without observational base ('...could be due to...'). The authors should stick to the observed data and avoid speculations, for instance about the SSA.

2. The main body of the paper should be consistent and adequately justified with the data that are presented. Citations to Valenzuela et al. (2012a,b) are not enough and the authors should consider including information about AOT and SSA at the site. You mention the seasonal AOT both in the abstract and the conclusions, but this not given in the paper! The number of investigated days (apart from or instead of the number of observations) should be given too.

Specific comments:

1. It is not clear to me whether all data or only data during dust episodes are considered in the analysis. Note the sentence: 'In fact, the monthly AOD (440 nm) value was slightly larger in June (0.37) than in July (0.31).' and the sentence 'the largest values of ARF at surface in April and July coincided with the lowest monthly values of the single scattering albedo for those months'. If only data from dust events is considered, the analysis is poor and needs to be enhanced with the aerosol properties in the absence of dust. If the complete dataset is considered (this is not clear to me), such AOT and SSA should be shown (see general comment 2).

2. There is also need for more rigorous statistical analysis. The ARF differences among sectors might not be significant. Please use any of the well-known specific tools (significance tests) to analyze whether the three populations of data are different in a statistical sense. At a certain confidence level, are the three ARF subsets statistically distinguishable? Note that the measurement uncertainty plays a role at this point too.

3. How did the authors accomplish the data quality assurance in principal plane re-

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trievals? Neither the cloud screening nor the error estimation are described. Compared to Olmo et al (2008) this paper deals with a larger dataset, so a manual approach does not seem adequate. What is the uncertainty in both the SSA and the ARF? Do you apply any limitation (in AOT or other parameters) as it is done in AERONET level 2.0 database? Neither method nor result is shown that assures the quality of your retrievals.

4. Have you tried using the CM11 flux measurements for evaluating radiative forcing at surface? Seems to be straightforward and this reviewer would find interesting to see the differences with the 3-step methodology (acquisition of sky radiances with a Cimel, inversion and finally flux modeling).

5. Please give information on how you calculate monthly ARF. This is important in order to allow comparison with other publications. Have you considered calculating monthly ARF with and without dust events, to estimate the contribution in ARF of dust with respect to the dust-free conditions?

6. If (as stated by the authors) the informations provided in table 3 may not be comparable, the table is nonsense to me. This is also linked to the comment above. The method how you calculate forcing must be provided.

7. Abstract: if AERONET is used as a well-established reference to prove the goodness of the analysis, quantitative differences should be given (see general comment 1).

8. There are abbreviations (e.g. 'ARFSurface') that are used only once in the text. Please be consistent.

9. If the forcing efficiency is (supposedly) seasonal dependent, why are all available data mixed up together in Figure 4?

Minor comments

- The English usage must be improved. I recommend that a native speaker edits the text. - The sentence 'This result suggests a relevant absorption of solar radiation in

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the atmosphere, leading to significant atmospheric warming.' is nonsense given that such data (atmospheric ARF) are available. Please rephrase. - Fig. 4: the regression statistics are missing (correlation coefficients, etc.)

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

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