Atmos. Chem. Phys. Discuss., 14, C2527–C2531, 2014
www.atmos-chem-phys-discuss.net/14/C2527/2014/

Atmospheric Chemistry and Physics Discussions
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ACPD
14, C2527–C2531, 2014

Interactive Comment

Interactive comment on "Estimation of mineral dust longwave radiative forcing: sensitivity study to particle properties and application to real cases over Barcelona" *by* M. Sicard et al.

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Received and published: 19 May 2014

Answer to RC C2061

The changes in the revised manuscript (posted soon) will be indicated in bolt font.

The paper addresses the study of radiative forcing due to atmospheric aerosol, with special emphasis on the longwave spectral range. Being this spectral range less studied than the solar spectral range the paper is worthy to be published in ACP. The

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use of a radiative transfer code that includes absorption and scattering effects of the aerosols in the longwave spectral range is an added value of the manuscript. The paper is well written and presents an appropriate structure. Nevertheless there are some points that must be improved before the paper would be ready for publication in ACP. Particular Comments - There is relevant question concerning the methodology and the way the authors define the radiative forcing concept. According to the literature the aerosol radiative forcing represents the change in the net solar irradiance associated to the inclusion/exclusion of atmospheric aerosols. Using this approach the use of equations 1 and 2 for the longwave spectral range is correct. For the shortwave spectral band equation 2 is correct at TOA but equation 1 is wrong at BOA, In fact the radiative forcing at BOA will be equal to equation (1) multiplied by the factor (1-alpha) with alpha the surface albedo. This fact needs to be clarified and carefully took into account in any comparison with results derived in other studies. In fact, the use of equation (1) implies an overestimation in the absolute values of radiative forcing strongly dependent on the surface albedo.
REPLY: We thank the referee for pointing out this issue. The equations 1 and 2 in the manuscript are wrong, or not completely self-explicative. The forcing at BOA and TOA are the difference between the net fluxes with and without aerosol, and each net flux is the difference between the downward and the upward fluxes. This has been clearly modifies in Eq. 1 and 2 in the revised manuscript. This modification also answers to the second question about the surface albedo which is included in the term F(BOA, up).

A second question concerns the way the authors do the radiative forcing computations. Thus they comment on line 7 page 8541that they compute the daily values, I understand that this means the integration over 24 hours for both the longwave and the shortwave forcing. But in section 5 they analyze particular cases that according to Table 3 correspond to short periods, when the lidar profiles are available, so it seems that these are instantaneous values. These points must be clarified in the revised manuscript.

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REPLY: The word “daily” on line 7 page 8541 was a typo. It has been deleted from the text. All forcings in Section 5 are instantaneous forcings. It is now clearly indicated in the captions of Figure 9 and Table 3.

- Recent studies published in ACP journal analyzed the aerosol direct radiative forcing in the shortwave spectral regions for Mineral Dust events detected over the Iberian Peninsula, Valenzuela et al. (2012). The authors must include these results in their comparison of radiative forcing estimates presented in section 5.
REPLY: The discussion in Section 5 has been extended in the revised manuscript and includes now comparisons with Valenzuela et al. (2012).

- Along the text the authors use AOT, aerosol optical thickness, to describe the aerosol load in the vertical column. The right term is AOD, aerosol optical depth that is the AOT in the vertical path. AOT depends on the solar elevation while AOD does not.
REPLY: In the revised manuscript the term AOT has been replaced by AOD. Thank you for this comment.

- Concerning the average volume size distribution in Figure 3, the authors must clearly state since the beginning that in addition to the mean size distribution there is some information informing about the deviation around the mean, included in Table 2. At least in terms of the standard deviation of the different parameters that the define de bilognormal distribution they use. In this table is a little bit surprising the rather low values of standard deviation for the different parameters, how the authors did these computations. Anyway in some cases the number of significant figures for the standard deviation is excessive, more than one significant figure is not justified is the more significant is larger than 2, otherwise two significant figures are enough to identify the uncertainty of the parameters
REPLY: In the revised manuscript the caption of Figure 3 makes now reference to

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Table 1, which contains information about the deviation around the mean. All the standard deviations given in the paper have been calculated the same way as the square root of the variance.
About the number of significant figures for the standard deviation, I am afraid we do not understand the referee's comment. In the ACPD manuscript all standard deviations in Table 1 are given with a number of significant figures equal or less than the number of significant figure of the mean value. Can the referee precise where the number of significant figures is not justified?
- In section 3.2.2 provide an average temperature from CERES. The value is offered with up to two decimal figures and with an standard deviation of 6.56 K, that clearly has no sense as a measure of uncertainty, 7 K will be the right figure. More information on the use of CERES data, like level and version of the data, acquisition time and temporal and spatial resolution are required. Furthermore, I have an additional question concerning the use of a fixed temperature for the "whole day", because the surface temperature is not constant along the day. How this hypothesis affects the study?, at least the part where the authors use the "model" they describe in Table 2.
REPLY: The standard deviation has been rounded to 7 K. Information about CERES products has been added in Section 3.2.2 in the revised manuscript. See also answers to S. Otto's (referee) comments 10 and 14.
The effect of the surface temperature on the longwave radiative forcing is discussed based on Figure 7f in Section 4. Also in Section 5 the surface temperature is identified as the reason for the differences observed between the measured and the modeled outgoing longwave radiation.

- More details on the atmospheric heating rates computation are required. Furthermore, it would be worthy discussing the results with the heating rates computed by Guerrero-Rascado et al. (2009) during an extreme episode of Saharan dust outbreak that affected the Southern Iberian Peninsula. The authors must revise Figure caption 10 that seems to be incomplete.

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REPLY: Some details about the computation of the atmospheric heating rates have been given at the end of Section 5 in the revised manuscript. It also includes references to the work from Guerrero-Rascado et al. (2009).

- The conclusions must be revised according to the previous comments. References Guerrero-Rascado, J. L., Olmo, F. J., Avilés-Rodríguez, I., Navas-Guzmán, F., Pérez-Ramírez, D., Lyamani, H., and Alados Arboledas, L.: Extreme Saharan dust event over the southern Iberian Peninsula in september 2007: active and passive remote sensing from surface and satellite, Atmos. Chem. Phys., 9, 8453-8469, doi:10.5194/acp-9-8453-2009, 2009.

Valenzuela, A., Olmo, F. J., Lyamani, H., Antón, M., Quirantes, A., and Alados-Arboledas, L.: Aerosol radiative forcing during African desert dust events (2005–2010) over Southeastern Spain, Atmos. Chem. Phys., 12, 10331-10351, doi:10.5194/acp12-10331-2012, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8533, 2014.