

Interactive comment on “Impact of dust aerosols on the radiative budget, surface heat fluxes, heating rate profiles and convective activity over West Africa during March 2006” by M. Mallet et al.

M. Mallet et al.

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This submission applies a mesoscale atmospheric model to describe a dust outbreak and related effects in March 2006. The model also includes an interactive radiation scheme which is used to quantify the solar and thermal-infrared radiative impact of the dust on the radiative energy budget at the top of atmosphere (TOA) and the surface. Similar investigations are known from the literature. However, what makes this manuscript worthwhile to be published is the fact that, additionally, the impact of the dust on the sensible heat fluxes, the heating/cooling rate profiles and the convective activity is investigated.

The manuscript is well written, although several changes outlined below should be

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performed before publication of the manuscript is warranted. Obviously, the authors submitted this manuscript shortly before, or almost at the same time when a special issue of Tellus (61B) published a series of papers on the Saharan Mineral Dust Experiment (SAMUM), some of them dealing with radiative effects of desert dust.

Please see: Tellus, Volume 61, Issue 1, Pages 1-353 (February 2009).

Among these papers at least three are of particular interest for the submitted manuscript:

- Heinold, B., Tegen, I., Esselborn, M., Kandler, K., Knippertz, P., Müller, D., Schladitz, D., Tesche, M., Weinzierl, B., Ansmann, A., Althausen, D., Laurent, B. Massling, A., Müller, Th. Petzold, A., Schepanskies, K., Wiedensohler, A., 2008: Regional Saharan dust modelling during the SAMUM 2006 campaign. Tellus, 61B, 307-324, DOI: 10.1111/j.1600-0889.2008.00387.x

- Bierwirth, E., M. Wendisch, A. Ehrlich, B. Heese, M. Tesche, D. Althausen, A. Schladitz, D. Müller, S. Otto, T. Trautmann, T. Dinter, W. von Hoyningen-Huene, and R. Kahn, 2008: Spectral surface albedo over Morocco and its impact on radiative forcing of Saharan dust. Tellus, 61B, 252-269, DOI: 10.1111/j.1600-0889.2008.00395.x.

- Otto, S., E. Bierwirth, B. Weinzierl, K. Kandler, M. Esselborn, M. Tesche, M. Wendisch, and T. Trautmann, 2008: Solar radiative effects of a Saharan dust plume observed during SAMUM assuming spheroidal model particles. Tellus, 61B, 270-296, DOI: 10.1111/j.1600-0889.2008.00389.x.

The authors should implement these references into the introduction of their manuscript.

A) This is effectively right and we have submitted this article just before two special issues now published in Tellus concerning the SAMUM experiment, in which a lot of information have been obtained concerning dust optical properties and associated radiative effects. As suggested by the reviewer, we have now introduced the different

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references quoted below in the new version of the manuscript (see introduction).

The concept the authors have chosen to quantify the radiative effect of the dust at the surface (both solar and thermal-infrared) is not perfectly consistent with common approaches and also not with the approach the authors use for the top of atmosphere (TOA). Usually, the radiative forcing (due to whatever impact) is quantified by the difference between NET irradiances in the dusty and the non-dusty cases. Net irradiance is defined as the difference between downwelling and upwelling irradiances. Thus we obtain for the radiative forcing of the dust (F indicating irradiance):

$\Delta F(z) = (F_{\text{down}} - F_{\text{up}})_{\text{dusty}} - (F_{\text{down}} - F_{\text{up}})_{\text{clean}}$ (1) For the TOA the authors follow exactly this common approach (at the TOA we have $F_{\text{down,dusty}} = F_{\text{down,clear}}$). However, for the surface the authors use another measure for the dust related radiative effects. Here the difference between the downwelling irradiances in the dusty and the clear cases is used ($F_{\text{down,dusty}} - F_{\text{down,clear}}$). This is what the authors call "dimming" and indeed this definition can be applied to describe the dust radiative effect at the surface. However, if the energy budget at the surface is considered, then rather the difference of the NET irradiances should be used instead, as suggested above (equation 1). I urge the author to rethink this approach.

The striking advantage of using the formula with the NET irradiances instead of using the irradiance difference is that you can clearly relate a positive radiative forcing $\Delta F(z)$ with a gain of radiative energy below level z, which commonly is associated with a warming of the layer below level z. Negative values lead to a loss of radiative energy of the layer below z and thus are linked with a cooling. What the authors are using (irradiance difference instead of the difference of NET irradiances) is not clearly linked with warming/cooling. The authors should also think about using the so-called forcing efficiency, which is the ratio between the radiative forcing and the respective dust optical thickness. Using this efficiency approach kind of normalizes the radiative forcing with respect to unity optical thickness. This makes the forcing values more comparable with available literature data.

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B) We agree with the reviewer about the definition of the aerosol surface radiative forcing, which should be calculated from the net irradiances. In the frame of this article and as explained in the first version, we took precautions to discuss about the effect of dust on the surface downward radiation, by calculating only the flux difference at the surface. As remark by the reviewer, this is effectively different from the named "aerosol radiative forcing". In this second version and in order to avoid confusions, we have included in the article (and more specifically in the new Table 2 and in the text (section 4.1)), the real dust radiative forcing (defined as $(F_{\text{down}} - F_{\text{up}})_{\text{dusty}} - (F_{\text{down}} - F_{\text{up}})_{\text{clean}}$). We also reported the associated forcing efficiencies in the Table 2. Such values could be used for comparisons with other studies related with dust aerosols over the West African region.

There are two major uncertainties which should be considered more carefully in additional sensitivity simulations:

(1) The single-scattering albedo (SSA) of the dust particles is always a serious issue in this respect. The authors use data retrieved from AERONET which seems to be the only feasible way in this case. However, the authors should not conceal the problems associated with refractive indices retrieved from AERONET. The conceptual issue is clearly that the AERONET retrieval results are always related to a columnar average, whereas the model requires data of SSA for individual particles. Also uncertainties in the AERONET retrievals should clearly be mentioned. If the authors could estimate error bars for the AERONET retrievals the simulations could be used to transfer these error bars for the single scattering albedo into uncertainty estimates for the radiative forcing (efficiency).

C) In this work, we used effectively AERONET refractive indexes of mineral dust (retrieved from the Dubovik et al. (2000) algorithm) to compute dust SSA. For a specific event, we think that such inversions are well adapted for describing the dust optical properties and more specifically its single scattering albedo. Indeed, dust optical properties are highly variables and not obligatory similar to those measured during a spe-

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cific experimental campaign. Regarding the dust optical depth observed during this specific event (~ 2 for different AERONET sites at 440nm), the uncertainty reported by Dubovik et al. (2000) on SSA is about 0.03. As suggested by the reviewer, we have now conducted 1D calculation to test the impact of uncertainties in SSA to dust surface and TOA direct radiative forcing. We have included this point in the Table 1 and in the text of the new version (section 4.1). Briefly, our results indicate an error on the dust radiative effect of about 10% at the surface and around 15-20% at TOA.

(2) The second major factor which may cause significant uncertainty in the modelled solar radiative forcing values is the surface albedo. The authors do not deeply discuss this issue. I even did not find information on which surface albedo data have been used. Again, a possible strategy to overcome this criticism would be to estimate uncertainty bars for the surface albedo and then to transfer these uncertainties by simulations into the solar radiative forcing.

D) This is effectively right and we have now improved this point in the article by including in the Table 1 a sensitivity test conducted for different surface albedo. Errors are then transferred to the estimated dust forcing both at the surface and TOA. Our results indicate logically important effects on dust TOA radiative effect, around 10-40%. This point is now clearly indicated in the text (section 4.3).

SPECIFIC COMMENTS:

- Abstract: Should be shortened. Acronyms (MesoNH, AERONET, SSA) should either be avoided in the abstract or be introduced properly. The year (2006) should be added after "9-13 March". BTW this is inconsistent with "7-14 March" mentioned in Section 2. Temperature changes should be given in Kelvin, not degrees Celsius. The value of -160 W m⁻² should more clearly be put into context (over which surface type: Sea or Land; where: TOA or surface). It is quite a large value, isn't it.

E) All the modifications reported by the reviewer have been taken into account in the abstract. The abstract has been re-written in order to be shorter and the different acronyms

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are now well introduced. We have also specified the context of the value indicated (-137 W.m⁻²), obtained at the surface, over the land (09°-17°N, 10°W-20°E) and at a specific time (all the results are instantaneous and given at 12:00 UTC). This explains this high value found to be consistent with previous studies listed in the article (4.1.1).

- 1 Introduction ... Include references to Tellus 61 special issue on SAMUM.

F) This is now made in the introduction of the new version. We have included the Heinold et al., 2009; Bierwirth et al., 2009 and the Otto et al., 2009 references.

- 2 Model... this section could be shortened and implemented into Section 1.

G) This is now changed in the article.

- 3 Data ... Please introduce all variables used in Equation 1. Also explain and introduce SSA as the ratio between ...

H) All the variables used and mentioned in the Equation 1 are now well detailed in the new section 2. Furthermore, the aerosol SSA is now introduced as the ratio between scattering and extinction (scattering + absorption) of the light in the text (section 3).

- 4 Results ... Uncertainties of the radiative forcing at the surface due to SSA, asymmetry parameter, and surface albedo problems are estimated by 10%. This seems very optimistic. Please solidify this estimate by some sensitivity studies as suggested above.

I) This is effectively right. In the new version, 1D sensitivity tests (summarized in the Table 1) have been performed using uncertainties of 0.03 on SSA, 0.05 in surface albedo and 0.04 in the asymmetry parameter. Our results indicate that we can reasonably consider an error of about 15% on the dust surface direct forcing. This point is now indicated in the section 4.1.

- It would be good to compare time series of downward irradiances (surface), and temperatures measured at specific stations with the simulated time series at the same

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locations. That would also help to prove the statement of the authors that the dimming is a result of the increased dust amount, and not that of changed meteorology (e.g., cloudiness). If the authors would show these simulated time series with and without dust it could also be shown if introducing the dust into the model improves the agreement between the simulated results and the measurements.

J) This is a very interesting remark but direct comparisons are not conclusive due to some differences between simulated and observed aerosol optical depth (see Tulet et al. JGR, 2008 in DABEX/AMMA special issue) at local scale. This could be due to the strong AOD horizontal gradient (Figure 1), associated with the spatial resolution used in our simulations (12*12km). However, we clearly observed that the surface temperature at Djougou decreases (daytime) during the dust outbreak for the 09 to 14th March period. It should be noted that similar results have been obtained by Slingo et al. (2006), who attribute to dust a drop in the daytime maximum temperature of about 10°C few days after the passage of the dust outbreak. However and as reported by Slingo et al. (2006), the reduction in the air temperature is at least partly a result of the arrival of cold air associated with the front and dust outbreak. Indeed, our results indicate that the net (shortwave + longwave) radiative heating of the atmosphere increases for all days. Despite this increased radiative heating, the lower troposphere cools during the dust event at Djougou, which illustrates the associated contribution of a relatively cool air flowing in from the desert. This point was not discussed in the first version of the article and now included in the text (section 4.2).

- An estimated uncertainty of 20% for the TOA forcing is not convincingly backed up in the manuscript.

K) This is effectively right and this point was not enough discussed in the previous version. As previously mentioned, we have now conducted sensitivity tests (summarized in the Table 1) in order to estimate the impact of uncertainties in SSA, asymmetry parameter and surface albedo on TOA dust forcing. This point is now indicated in the text (sections 4.3) by using the following sentence: "Sensitivity tests reported in the Table

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1 display significant uncertainties in the TOA dust forcing, mainly due to errors in the surface albedo. Our calculations indicate that uncertainties vary between -40% and +15%, depending on the parameters tested."

- Infrared cooling rates of -6 to -16 K per day are huge, something wrong here? The unit "per day" is a little misleading here. These are instantaneous heating/cooling rates which have been simulated at noon. The unit "per day" implies that this is a daily average (averaged over the solar cycle with high sun at noon, no sun at night), which is not the case here. I admit that using the unit "per day" makes these values better comparable to literature data.

L) In the previous version, we included incorrect profiles of the LW Heating Rate. As shown in the new Figure 13, we obtained positive values at the surface, with values comprised between +4 and +9 K by day at the surface. Infrared heating rate values are negative just within the dusty layer with values comprised between -0.10 and -0.20 K by day. This point is now changed in the text (section 5.4.2). Such values are more consistent with those reported by Mohalfi et al. (1998), who reported a value of -1 K by day.

TECHNICAL COMMENTS: - Figures: In the color plots of differences (Figs. 4, 7,8, 10 11) negative values should be plotted as green-blue, positive data with yellow-red. So far this is the case in Fig. 8 only. The location of the station Djougou should be indicated in the plots. The optical thickness data could be implemented as contour plot, in particular in Figure 10. The Figure captions should contain the time, this is given occasionally only. Most of the axis labels are too tiny to identify, or in some Figures the axis labels are completely omitted (e.g., Fig. 12-13).

M) All these modifications are now included for each figures, expected contour-plots. Negative values are indicated by green-blue colors and positive one with yellow-red. The location of Djougou and different AERONET sites are indicated in the figure 1. However, we prefer to keep in place the aerosol optical depth regional patterns in the

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Figure 1, which can be used for comparisons with other estimations. The time is now included in the Figures and the axis labels are now increased.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2967, 2009.

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9, S1729–S1737, 2009

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