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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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.

Anonymous Referee #1

Received and published: 12 March 2009

GENERAL COMMENTS:

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 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., Schepanskie, 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.

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

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(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_down - F_up)_dusty - (F_down - F_up)_clean (1)

For the TOA the authors follow exactly this common approach (at the TOA we have F_down,dusty = F_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_down,dusty - F_down,clean). 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.

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

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(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).

(2) The second major factor which may cause significant uncertainty in the modeled 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.

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.

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

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

- 3 Data ... Please introduce all variables used in Equation 1. Also explain and intro-

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duce SSA as the ratio between ...

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

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)

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

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