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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Saharan dust transport and deposition towards the Tropical Northern Atlantic" *by* K. Schepanski et al.

Anonymous Referee #3

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Review on the manuscript " Saharan Dust transport and deposition towards the Tropical Northern Atlantic” by Shepanski, Tegen and Macke Submitted to Atmospheric Chemistry and Physics Discussion

General comment :

This paper describes and analyses the main characteristics of Saharan dust transport and deposition toward the Atlantic Ocean simulated by a regional model, for three months of 2006-2007 corresponding to contrasted transport pattern. The paper is well written and organized however the interest of the presented work is quite limited due to a lack of experimental constrains on the simulations. For example, the authors discuss the capability of the model to reproduce a general trend described in the literature: the change in the altitude of Saharan dust transport between winter/spring and summer. However, no quantitative elements on the way the model describe the transport for the specific studied period are given. The only comparison of the simulations with observations concerns a parameter that does not describe the vertical distribution: the aerosol optical thickness, i.e. the columnar integrated dust amount. A validation of the simulated profile is feasible, at least for the two months of 2007, since aerosol vertical profile are available from the CALIPSO instrument onboard the Aquatrain. Such data should be used to test not only the seasonal changes in the altitude of dust transport but also the changes that are simulated at “sub-monthly” time scale (typically in January 2007). Similarly, the authors describe the simulated deposition pattern and contest the capability of satellite observation to be of any use to estimate dust deposition but they do not provide any evidence that the model reproduce correctly dust deposition over Africa. The comparison with AOT is made over the CapVerde Island only, while several other sunphotometers from the AERONET network were operational over Africa at the period of the simulation. These stations may be located closer from the dust sources responsible for the dust export over the Atlantic Ocean. The aerosol size distribution retrieved from the AERONET measurements can also be used to test simulated aerosols size distribution. Indeed, dust size distribution controls the transport and deposition fluxes and also the AOT, i.e. the main features discussed in this paper. Finally, the authors dedicated a significant part of the paper to discuss the result from a previous paper (Koren et al., 2006) on the estimation of the westward export of dust emitted from the Bodélé depression. It is difficult to be convinced that the estimation provided in this work is really relevant for such a purpose since the way the dust emissions are simulated from the Bodélé depression is not discussed and compared to any observations. In addition, the comparison of a two month estimation with an annual estimation is also questionable. In fact, a deeper analysis of the different source areas and of their relative contribution to this westward transport would be much more informative and original. Obviously, this would require a validation of the simulation of the different source activity, based for example on the IR satellite indexes the authors are familiar with (see Shepanski et al., 2007). The authors also contest the relevance of the empirical relationship used by the Koren et al. (2006), i.e. the ratio between the total mass and the AOT. Once again, to be convinced that the ratios found in the paper can perform better, a deeper quantitative validation of the simulations is needed. I would thus recommend the authors to improve the paper with additional comparison with observations and further analysis of the simulations in terms of (1) source location and contribution to the export over the Atlantic Ocean and (2) sensitivity of the simulated deposition pattern to the dust size distribution.

Specific comments :

Part 2 : In the description of the modelling system, the authors should indicate the initial dust size distribution used at the emission (and not only the size range of the different bins). The value of the extinction coefficient used to compute the AOT should also be given.

Part 3.1 It is not clear in the first part of the chapter, what elements are specific of Saharan dust transport and what elements are generally dynamical features (for example, the discussion on the evolution of the boundary layer, or the influence of the west African monsoon on the geostrophic winds). In addition to the simulated AOT, the simulated dust emissions should be displayed and compared with satellite observations. The influence of biomass burning aerosol on AOT is not mentioned despite the fact that it can significantly contribute to the AOT in winter. For the March 2006 dust storm, the authors discussed AOT values of 0.3 to 0.6 over Algeria while AOT higher than 3 have been measured by the AERONET stations in Niger and Mali. How does the model perform in these stations ? Many AERONET stations can be used to test the relevance of the simulations. MODIS AOT are monthly average for clear sky situations only. Are the cloudy days excluded for the computation of the simulated monthly AOT ?? This may explain part of the discrepancies between the model and MODIS AOT especially in July.

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Part 3.2 This part is of limited interest since there is no way to test whether the distribution of wet and dry deposition is reasonably simulated. Deposition being largely controlled by the dust size distribution, some constrains could be given the AERONET retrieval of aerosol size distribution. It would also be interesting to discuss the change in the aerosol distribution from the source areas to the transport and deposition regions. It would also be interesting to compare the simulated precipitation fields controlling the wet deposition pattern to observations of precipitations. In the last sentence, it is not clear what the authors mean by “unsettled weather character”.

Part 3.3 The discussion on the discrepancies between the simulated and observed AOT is not totally convincing. In particular there is no discussion on the capability of the model to reproduce the sources of the observed dust events. Part of the discrepancies in summer is attributed to wet deposition. As mentioned above, the authors can, at least, compare the precipitation used in the model with observations. The different altitudes of transport are given in km in the discussion but in pressure on figure 5.

Part 3.4 This part is not clear and confusing. The fact that significant eastward transport is simulated, especially at 20°W is very surprising. The authors argued that the high altitude transport eastward simulated in winter it is due to fact that the air flow in the upper troposphere is reversed compared to the lower layers. However, even if the possibility of such transport appears as realistic from a dynamical point of view, its intensity is quite surprising. The zonal flux being computed as the product between the mass concentration and the wind in the considered direction, this result suggests quite high concentrations in the upper troposphere that seems inconsistent with the vertical profiles displayed in figure 5. The result is even more surprising when looking at the zonal fluxes from Bodélé region alone. From figure 7, it seems that in January the high altitude eastward flux is as intense as the westward flux figure suggesting that a significant part of the dust emitted from the Bodélé depression in winter re-circulate over the Sahara at high altitude ?? If this is correct, this quite a new and original result … but in this case, it absolutely requires some experimental evidence. The

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eastward fluxes simulated close to the surface in summer is explained as being due to the monsoon flow however, once again, this meteorological feature explains the main flow direction but not its intensity. Does this mean that some of the dust transported in the Saharan Air layer deposit in the marine boundary layer and is backward transported in the monsoon flow ? The very high westward flux at 20°W at the higher level of the model is also very surprising. Such a high flux is not simulated at 10°W, suggesting the existence of active dust sources between 10 and 20°W from which dust can be lifted at very high altitude in such a small distance. Once again, it does not look realistic. The vertical axis of figure 6 corresponds to “levels” of the model ?? It should be changes in pressure or altitude

Part 3.5 As suggested in the general comment, a discussion on the contribution of the different sources as a function of the season would be of greater interest than this focus on the contribution of the Bodélé depression. Additional explanation on the positive high altitude fluxes in winter are absolutely needed. The discussion on the Bodéle export compared to the Saharan dust export is really difficult to understand and must be revised. Obviously, the sign of the zonal budget can be different when looking at Bodélé souce alone compared to the whole Sahara, but the fact the export from Bodélé can be higher than the export from the whole Sahara appears as impossible. The argument that dust from Bodélé can reach high tropospheric levels as “illustrated on figure 7” is not clear.

Part 3.6 It is not clear how the ratio M/AOD is computed in the simulation. Is it an average over the whole simulation domain ? Is it computed as the total monthly mass divided by the total monthly AOD or as an average of individual ratio M/AOD ?? The authors argue that most of the difference with the ratio used by Kaufman could come from differences in the aerosols size distribution, however the distribution of this ration in the simulation is quite limited, suggesting a moderate change in size distribution. So instead of giving the size bin limit, the authors should give the modelled size distribution for example at the sources and at different points of the 20°N transect. It is not clear whether the computation of the dust fluxes from case 1 to 3 concerns the three studied months or for selected periods.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16061, 2008.