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***Interactive comment on* “Simulation of the mineral dust content over Western Africa with the CHIMERE-DUST model from the event to the annual scale” by C. Schmechtig et al.**

C. Schmechtig et al.

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Reply to the editor comments

The paper entitled presents the implementation of the CHIMERE-DUST model for the simulation of mineral dust load and surface concentrations during 2006. The model outputs are compared with AOD and surface concentration measurements as obtained for different time scales (hourly, daily, etc). An evaluation study is presented here with the experimental databases. The reviewers coincide in pointing that the presented results on model outputs have good agreement with the observations in specific cases, while the overall model performance is often non-satisfactory. This is in my opinion the

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Discussion Paper



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Comment

main question raised that was already commented you before accepting the paper for discussion. Thus, when preparing the revised version, please consider this point and the need of a more close evaluation exercise using the US-EPA protocol for evaluation of modeling results commented by one of the reviewers. They will review again the revised version. I am also attaching some comments from my side.

COMMENTS 1. Abstract. Line 10. The adjective ‘fairly good’ is not adequate for a scientific paper. You should also support the adjective with statements on ‘for what purpose (climate, air quality,: : .) the quality of the fitting of modeled and experimental data is good? Based on what tests you conclude it is good? Support with numbers the goodness of the agreement’.

Reply : We removed from the abstract any appreciation of the level of agreement and simply mention to what extend the simulations and observations are correlated.

- Page 8038, line 28. There are by There is

Reply : Done

- Page 8042 Line 20. Do you mean South West, instead of South East?

Reply : Yes, this was corrected.

4. Important: Page 8053. Apply the comment n.1 here: ‘generally correctly reproduced’ it is not enough scientifically sound for an ACP paper. Also for AOD ‘consistent with measurements’ has to be supported. You should also support the adjective with statements on ‘for what purpose (climate, air quality,: : .) the quality of the fitting of modeled and experimental data is good? Based on what tests you conclude it is good? Support with numbers the goodness of the agreement’. The one suggested by US EPA should be applied.

Reply : The “non scientific” comments have been removed. Additional statistical parameters recommended by the US EPA and Boylan and Russel (2007) for PM2.5 simulations have been added, synthetized in a table and commented in the revised version

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of the manuscript, in order to quantify the level of agreement between the model and the observations. To estimate the quality of our simulation, we now use as a reference the range recommended by Boylan and Russel (2007) for these parameters but also the synthesis of values of these parameter obtained by air quality models for PM10 and soil dust simulation proposed by these authors

5. Important: Idem in Conclusions section ‘considered as satisfying’, ‘reasonably well reproduced’ for what and based on what? Support with available modeling test.

Reply : In the submitted manuscript, these appreciations were related on the performance of the previously published regional dust simulations. In the revised manuscript, they refer to the range of statistical parameters obtained for PM10 simulations by air quality models. Obviously, since these statistical parameters have never been estimated previously for dust models, it is not possible to discuss the performance of our model compared to the others based on these parameter; like it is classically made for air quality.

6. Figure 6 and related text shows different results from a number of satellite measurements, and the classical observations from Dubief in the 70s. Usually in winter, more southern source areas are active according influence of ITCZ position, whereas in summer, the central and northern parts of Sahara become more active. May you comment on it?

Reply : It is not clear for us what satellite measurements of work by Dubief show a different picture. A similar pattern was identified using Infrared Meteosat dust index by Brook and Legrand (2000), i.e. from a totally independent satellite product that the one displayed here. This reference has been added to support our comments on TOMS and deep blue AODs and o the simulated dust source location.

7. Also there is a problem of description in text, in summer you mention in text that SE zones are active. Do you mean SW by SE?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Reply : Yes, this was corrected.

8. Important: Figure 12. The surface PM₁₀ model results (in red dots, not in gray as stated in the caption, please correct it) give much lower results in Jan-April than the experimental data, also for Oct-Dec. This difference is yielding different seasonal patterns in the modeled and experimental PM₁₀ data. You should comment on it, search on causes (lack of anthropogenic emission data???), including this discrepancies when you describe the agreement between experimental and modeling data. Note that for January modeling data would yield PM₁₀ exposure levels of around 10 $\mu\text{g}/\text{m}^3$, whereas the experimental data reach 100 $\mu\text{g}/\text{m}^3$. This fitting is not appropriate for any air quality study.

Reply : The legend of figure 12 was corrected. The simulated surface concentrations are lower than the observed one in Jan-April and much higher in Oct-Dec. Both cases correspond to dust emissions from the Bodélé Depression. This means that the correction of the wind field we used is not appropriate all along the year. This is now stated and discussed in the manuscript. Since only AODs with low angstrom coefficients have been selected, the discrepancies cannot be due to anthropogenic aerosols. We also state the largest discrepancies are observed at these two periods. However, for eth whole year data set, the simulation reaches comparable performance level, quantifies though the errors and bias than air quality models simulating PM₁₀ or soil dust concentrations. We do understand that such discrepancies would not be acceptable for air quality studies, but this is not the objective of this work, which is to quantify how much of the dust variability can be reproduced by a CTM.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8027, 2011.

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