

Interactive comment on “Homogenized modeling of mineral dust emissions over Europe and Africa using the CHIMERE model” by R. Briant et al.

Anonymous Referee #2

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This study has tried to update CHIMERE chemistry-transport model on its boundary conditions, such as new soil and surface datasets, and on new aeolian roughness length datasets provided by GALAP. The authors have conducted numerical experiments under idealized conditions and made case study over Africa and Europe. CHIMERE is widely used for daily forecasts of ozone, aerosols and other pollutants. Thus, the results derived from model improvement would be interesting among atmospheric chemistry community. However, the model update was only made with boundary conditions and aeolian roughness height map. In this sense, the scientific interest is poor. Numerous model experiments were conducted but implemented experimental designs are poor and the discussion on the experimental results cannot be found in the manuscript. For these reasons, this manuscript isn't worth for a scientific original paper though some values are seen as a technical report. Thus I concluded this article

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to be rejected as an original paper for Geoscientific Model Development. Please see the detail in the following comments.

<Major comments:> 1. The main issue is that the design and setting of idealized numerical experiment, namely "academic test" in the manuscript, is unsuitable. Also the aim of the idealized numerical experiment (academic test) is unclear. In this academic test, properly selected model configurations and specifications were not applied, e.g. it was made under different model domain and different horizontal resolutions. In addition, the configurations of number of vertical atmospheric layers and boundary layer processes in each model are not mentioned in the manuscript or Tables. The authors have tried to conduct a sensitivity test of boundary conditions and a new aeolian roughness length database. But the authors used various kind of models with various model settings for this. Then, isn't it quite difficult to tell where the result differences have come out from? If the authors want to see the sensitivity of the new boundary condition effects, the setting and the configurations of model in the academic test must be always same.

2. The academic test results are shown in the figures and tables. But the authors only mentioned the differences of the results. No intensive discussion based on physical insights of processes and effects due to new soil and surface properties or new aeolian roughness length dataset can be seen. It is very ordinary that model result differs when the boundary conditions and the controlling parameter of flux scheme change. In order to clarify the scientific values on the result of the experiment, it is essentially important to present a scientific new finding based on a physical insights for the results and/or a practical advantages by comparing with former model representation. However, this academic test only presented the differences of model result by applying different model configurations, which are new boundary conditions and new aeolian roughness height map. Due to poor setting of model experiment and lack of discussion, it is unclear about the effect and sensitivity by the individual and total model update e.g. new boundary conditions and the new aeolian roughness height map.

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3. In the case study in Chapter 4, Figure 9 indicated that the difference of dust loadings over Africa and Europe between M13 composite model and ExtMod. But according to the Figure 10, it is unclear the model improvement of representation of observation results as compared with former model. And no discussion about this is made here. The authors should make more intensive analysis and discussion on the representation of dust loading by new model. And in Figure 10, 6 sites results were shown but these sites location map is not shown in this manuscript.

4. In this study, model verification was made only for vertical flux of dust and optical depth. For understand the model performance and predictability, transport and deposition processes are also important. I strongly recommend making a validation test for dust transport and deposition processes over Africa and Europe using PM10 network data and Lidar network data.

5. The notations of the models used in this study are too complicated. The followings are the example of model notation used in this study. Actually, these models can be classified into 3 models. CHIMERE dust production model, DPM (in abstract), Dust production model presented in Menut et al., CHIMERE-Europeïd' V05, CHIMERE-Africa, M13, M13 dust production model, CHIMERE African dust production model (P18 L17), CHIMERE model, CHIMERE (P15 L23)(in this case, CHIMERE is composite of M13 and V05), Extended dust model, ExtMod (P14 L20), Extended dust production model (P16 L1), and Extended model (P16 L13, L15) The authors should use a proper and simple notation for convenience and readability to reader. You may summarize the above models into 3 models, which may be expressed using proper abbreviation such as, CEur, CAfr and ExtC, instead of M13 and/or V05.

<Minor comments> Abstract: Shorten the former half of the abstract, Line 2-10. Results of this study are unclear in the latter half of the abstract. The authors should articulately summarize what is their original work and what are the findings in the abstract.

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P2 L14: academic tests > idealized numerical experiments? Same as above.

P4 L11-12: The authors wrote, "the aim(s) of this paper is to extend and update the dust production model, presented in Menut et al. (2013b)". In this chapter, however, no description was made on the outline of this model and its current issues that the authors want to extend and update.

P10: dust emission from the surface is driven by shear stress and friction velocity, u^* , is used for the measure of it in the literature. The wind erosion scheme used in the present study is described as a function of U instead of u^* . In that case, the authors should specify the boundary layer process and the specifications of each models, the height of lowest atmospheric layer of each models and also treatment of stability correction of U (lowest layer). Are the heights of lowest layer of each model same? If not, proper corrections of U in the each models are required to be added on the similarity of idealized wind speed settings. They are not shown in the text. Please explain these procedures and settings in the text.

Terminology: soil humidity > soil moisture or soil moisture content. Fecan (1999) used gravimetric soil moisture (%) as a controlling parameter of wind erosion. This is not a unit of humidity of the air within soil layer. However, a gravimetric soil moisture is being defined as Mv/Mt , where Mv is the mass of water and Mt is the bulk total mass.

P11 L8-11: In Lines 6-7, The authors wrote " the HUMIDITY case show(s) a significant decrease of the dust emissions, coherent with the use of soil humidity parameterization (Fecan et al. 1999)". On the other hand, in Lines 9-10, the authors wrote "over most regions of interest the HUMIDITY case tends to increase the total emission flux". It is quite confusing. When soil moisture effect is added to the dust emission scheme, the model calculated increased vertical fluxes both over Europe and Africa according to Table 4. Why did it occur?

P14 L3 and L4: Yearly variations > Monthly variations

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P15 L10-22: This part is lengthy. You had better simply summarize the specifications of GOCART, WRF, and CHIMERE in a specification Table.

P17 L16: "values" > what values?

Table 6: The value EAE is not explained in the caption and the main text.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3441, 2014.

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