Geosci. Model Dev. Discuss., 7, C1436–C1455, 2014
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Geoscientific Model Development Discussions
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Interactive comment on "Homogeneized modeling of mineral dust emissions over Europe and Africa using the CHIMERE model" *by* R. Briant et al.

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Received and published: 20 August 2014

20 August 2014

Answers to referees for "Homogeneized modeling of mineral dust emissions over Europe and Africa using the CHIMERE model" by Briant et al. (gmd-2014-78)

Dear Editor and referees,

Globally, we found that the reviewers focused on:

1. Some difficulties to see what is really new in this model development.

Many things are new in this model development: this is the first study with a deterministic calculation of mineral dust fluxes over any domain, compared to other state-of-the-art chemistry transport model and at the regional scale. This includes to take into account vegetation variability, soil moisture and additional surface characteristics enable to produce dust emissions. Model improvements and new findings of our work are now better explained into the introduction, the abstract and later on in the manuscript.

2. Some difficulties with the terminology used.

   The sections 2 and 3 are more clearly explained, new references to the bibliography and to the Tables summarizing the model configurations are added, notations are now better introduced and all abbreviations are now explicitly explained. The differentiation between idealized and real test cases is also better explained.

3. A lack of discussion about the results

   The results are now more accurately described and discussed in this new version.

We now give a detailed answers for all reviewers comments. Our answers are in blue in the text after each referee remark.

1 Referee #1:

The authors present a CHIMERE model set-up that includes a dust production model and can be applied to mineral dust modeling over Africa and Europe. As dust sources over Europe differ from those over North Africa, the authors make use of different input data sets describing the soil erodibility. The authors extensively present a state of the art perspective on dust production modeling (applied to CHIMERE), but unfortunately

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only very briefly discuss the improvements of the new scheme.

1.1 General comments:

1) Many abbreviation used in the manuscript are not introduced and may remain unclear to at least parts of the readers. Please introduce the abbreviations when used first.

We added explanation for all abbreviations.

2) What exactly do you mean by an "academic test case"? Following the reminder, the "academic test case" is presented in Sec. 2. To me, it rather reads as a summary on the "state of the art" of dust production modeling - tested within CHIMERE. Do you mean this by "academic test case"? If so, please clarify at the beginning of the section. Maybe you want to consider renaming this section to be more precise then? Please also consider renaming Section 3 into "Sensitivity study" and omit "academic test case" as this is naming rather confusing.

Indeed, the Section 2 could be refer to as "state of the art of dust production modeling". We named it "The dust production model: background and new developments" as it presents the state of the art along with new developments that has been made and that are used in following sections. However, it is not written anywhere that the Section 2 deals with academic test cases. It is written on P3444 L19: "Academic test case results are presented in Sect. 3.". "Academic test case" means idealized case study. In our case meteorological parameter are set to a constant values over the whole domain. The academic case section is the section 3, not the section 2. We replaced

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"academic test case" by "idealized test case" into the manuscript.

3) Do you account for changing roughness length due to soil preparation for the agricultural sources? Please comment on this.

Yes, we do apply a correction factor to the roughness length based on the green fraction coefficient. It takes into account monthly vegetation variations thus, indirectly, takes into account soil preparation for the agricultural sources. This is a simple correction that may be improved in future work, however, it does introduce a monthly variability into the emissions (see section 2.3).

4) Sec. 3: How realistic is the assumption of U=14 m/s? Can you comment on this, please?

Section 3 present theoretical cases results, therefore, the assumption $|U|$=14 m.s^{-1} is not made to be realistic. Considering $|U|$=14m.s^{-1} is a high value that is not reached under normal wind conditions. Furthermore, using this value as a constant value all over the domain at such scale is not realistic. However, major dust event occur under extreme wind conditions. We can see on Figure 4 that the threshold below which dust fluxes are zero and above which the average mineral dust flux increases is about 12$m.s^{-1}$. Therefore, we choose a value slightly above this threshold (i.e. 14$m.s^{-1}$) so that maps can represent the potential of dust emissions in every regions.

5) Sec. 4.1: This section is not that easy to follow. The first impression is that WRF, CHIMERE, and GOCART are three models that will be compared. Later it becomes obvious that WRF and GOCART are somehow used to initiate the CHIMERE simulations. So I would like to suggest rewording and restructuring this section.

Maybe, start with an explanation that CHIMERE is a model system (or part of a model system?) that requires input fields from global and regional models (?). Also, would not it make sense to have this introduction section on CHIMERE earlier? Furthermore, you may want to consider introducing the AOD measurements as a separate section not called "Model set-up".

Yes, indeed WRF and GOCART provide input data to the CHIMERE simulations. We rearranged this section which is now easier to understand.

The paragraph about CHIMERE deals with CHIMERE model set-up, therefore, should not be moved earlier in the paper. CHIMERE model is presented in the Introduction on P3444 L14 to L17, with all necessary references. The CHIMERE dust production models are part of the CHIMERE model and are explained with many detailed in Section 2.

We moved the AOD measurements paragraph into a new subsection called: "Aerosol Optical Depth measurements"

6) Sec. 4.2 and 4.3: The discussion of the results is very brief and should be extended. It would also be good if you can provide some implications for the application and use of the updated model version. Otherwise, it's not much the reader gets out here and the conclusion (Sec. 5) that an improved dust production model is presented is not obvious. In particular as you state in that the "differences among the models is small" (P3456, L18-19).

We extended section 4.2 (see comments below), 4.3 and 5.

The conclusion that the extended model improved results is not based on Figure 8 and 9. This conclusion comes from the better representations of major dust emission area in section 3 (e.g. western Asia on Figure 3) along with performance indicators in Table 6. Indeed, differences are small when looking at Figure 9 (P3456 L18-19). However, the figure shows the average AOD over Europe and Africa regions. It does not mean that differences are not important locally. Even though dust production model are different and gives very different dust emissions fluxes (see section 3), the average AOD trend remain similar. Major change in dust emissions fluxes do not necessarily creates a major change in the AOD. We added a comment on that in the manuscript.

7) Fig. 8 illustrates a di-pol like difference pattern over the southern Sahara. Any explanations for this? Furthermore, there are almost no differences evident over Europe, but you aim at showing that the new extended model performs better over Europe. Can you explain in more detail how you conclude this?

Indeed we observe a di-pol like difference pattern. We developed more this paragraph.

The AOD differences over Europe are small but the AOD values are also smaller than over Africa, thus, the relative difference is high. We added a relative difference map along with more comments.

Together with Table 6, Figure 10 shows the improvement of the extended model performances over the M13 model. In addition, independently from whether there is a gain or not in modelled fluxes, it is important to notice that this work corresponds to a major improvement of the representation of the processes in the model.

1.2 Specific comments:

P3442 L14-15 What do you mean by academic test cases? Do you refer here to the "state of the art" section?

"Academic test cases" means idealized cases study. We replaced academic by idealized in the manuscript (see previous comments). We refer, here, to the Section 3.

P3442 L13 What does "GARLAP" stands for?

GARLAP stands for (Global Aeolian Roughness Lengths from ASCAT and PARASOL). It is defined in the Introduction (P3444 L10). We added it into the abstract.

P3442 L24 The paper by Ginoux et al. (2013) provides a more recent view on the global distribution of dust sources and may be added to the reference list here.

We added the reference.

P3443 L07 Additionally to the here named studies on European dust emission, there are the studies by Korcz et al. (2009) using a model to estimate dust emission over Europe for different years.

We added the reference.

P3443 L09 What's been in the focus of the WELSON project and for what does

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WELSON stand?

WELSONS means: Wind Erosion and Loss of SOil Nutrients in semiarid Spain. The project aim at understanding and predicting the potential impacts of land-use change and management on soil degradation by wind erosion on agricultural land in Central Aragon, Spain. We added the explanation in the manuscript.

P3444 L05 ERS?

ERS means: European Remote Sensing. We added it into the manuscript.

P3444 L08 PARASOL? ASCAT?

PARASOL means: Polarization and Anisotropy of Reflectances for Atmospheric Sciences coupled with Observations from a Lidar. ASCAT means Advanced SCAT-terometer. We added it into the manuscript.

P 3444 L16-17 Can you provide a reference here, please?

We added a reference.

P3449 L21 "Each feature" - which features?

The features we refer to are the one presented in the Section 2 and summed up in Table 3: the use of new input datasets, the use of cropland landuse as erodible, soil

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moisture parameterization and the seasonal variation of vegetation parameterization. They are described from P3449 L23 to P3450L04. We added a precision in the abstract, the introduction and later on in the manuscript.

1.3 Technical comments:

P3451 L06 shows

The correction has been made to the manuscript.

References:

Korcz, M., J.. Fudala, C. Klis (2009), Estimation of wind blown dust emission in Europe and its vicinity, Atmospheric Environment, 43, 1410-1420.

2 Referee #2:

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

We show in this new paper version and in the following answers that we developed a model suitable to explicitely calculate mineral dust emissions fluxes over any domain, including both Europe and Africa. This model development is original and, to our knowledge, there is currently no regional chemistry-transport model able to do such calculations over any domain. The description of all idealized cases was rewritten and we think this is now more clear that these cases are adaptated to a step by step validation of the new implemented schemes.

2.1 Major comment 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.

As it was confusing for the referee #1 as well, we replaced the term "academic test" by "idealized test".

As stated page 10 L5-9, all idealized test cases were conducted using the same

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grid with the same 60 km horizontal resolution. Thus, selected configurations and specifications were properly applied. The simulated domain is clearly shown in the Figure 1

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.

In the idealized cases section only emissions fluxes were computed. Emissions fluxes are computed at ground level, thus, there is no vertical dimension. We added some precision about that in Section 3 and changed the name of the Section to clarify it.

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?

Each idealized test case is fully explained at the beginning of Section 3 and in Table 3. It is clearly stated that each test case is made with the same configurations. Each case differ from the previous one only from the feature that is being tested.

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.

Indeed, configurations of model in the academic test must be always same. That is what we did by using the same constant meteorological parameters and horizontal grid for each case. Each case differ from the previous one only by the additional

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feature that is being tested. The selected configurations specifications are described with many details at the beginning of Section 3 (pages 9 and 10) and are summed up in Table 3.

2.2 Major comment 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.

The scientific new findings of this study rely on the new features that are added to the CHIMERE dust production model, which are:

- The use of new soil, landuse and aeolian roughness length datasets and the use of the M13 dust production model over the entire simulated domain.
- Additional landuse types considered as erodible surfaces (USGS landuse categories ranging from 2 to 9 and category 19).
- The use of the Fecan parameterization over the entire simulated domain
- The use of the vegetation seasonal variation parameterization

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Physical choices that lead us to using these features are explained with many detail in Section 2. Section 3 is pure modeling work: each new feature is successively added in order to successively study the impact of each feature. Therefore, results are discussed from a modeling point of view whereas basis for using these features are physical choices made in Section 2.

We added more details about the new features in the introduction as well as in the Section 3 and in the abstract as requested below. This help to clarify the purpose of our work.

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.

It is not mentioned anywhere that the boundary conditions are a new feature that is added and tested in Section 3. Boundary conditions are not involved in the computation of dust emission fluxes. The use of a global aeolian roughness dataset is, indeed, one of the feature that is added to the model but it is not the only one (see the above list).

Due to poor setting of model experiment and luck 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.

Each idealized test case is fully explained at the beginning of Section 3 and in Table 3. It is clearly stated that each test case is made with the same configurations. Each case differ from the previous one only by the additional feature that is being tested. The

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selected configurations specifications are described with many details at the beginning of Section 3 (pages 9 and 10) and are summed up in Table 3 (see also above answers to major comment 1).

2.3 Major comment 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.

Independently from whether there is a gain or not in modelled fluxes, it is important to notice that this work corresponds to a major improvement of the representation of the processes in the model. To our knowledge, regional models only calculate dust fluxes over the main global sources, i.e African and China deserts. In our case, we added a deterministic calculation of these fluxes over all erodible surfaces and thus inside Europe. In addition and as mentioned in referee #1 answer, we did not conclude that there are some model improvements based on Figure 8 and 9. This conclusion comes from the better representations of major dust emission area in section 3 (e.g. western Asia on Figure 3) along with performance indicators in Table 6. Indeed, differences are small when looking at Figure 9 (P16 L18-19). However, the figure shows the average AOD over Europe and Africa regions. It does not mean that differences are not important locally. The aim of Figure 9 is to show that even though dust production model are different and gives very different dust emissions fluxes (see section 3), the average AOD trend remain similar and that major change in dust emissions fluxes do

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not necessarily creates a major change in the AOD. We added a comment on that in the manuscript.

Figure 10 represent hourly times series at some given points in the simulated domain. Together with Table 6, Figure 10 shows the improvement of the extended model over the M13 model.

And in Figure 10, 6 sites results were shown but these sites location map is not shown in this manuscript.

Coordinates of all sites are shown in Table 5. We added a reference to Table 5 in the manuscript.

2.4 Major comment 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.

In this paper, there are two different sections for the model development and validation, in addition to Section 2 which present the reasons for these developments. The section 3 is dedicated to the mineral dust emissions model development: idealized cases are suitable to compare old and new schemes for emissions only, for which there is no measurements. Consequently, this is a bi-dimensional comparison and transport and

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deposition are not relevant in this section. The section 4 is dedicated to a real test case, and, of course, all atmospheric processes such as transport and deposition are taken into account. These processes were already studied with CHIMERE model in previous articles. This is also explained in this study in paragraph pages 15-16. About model evaluation with data, we consider that for a large domain, the use of AOD is adaptated to the presented study and to answer to the main question of this paper: is it possible to model in a realistic manner mineral dust fluxes over areas such as western Europe? This paper corresponds to a model development, suitable for GMD. After this development, more studies will be conducted using this new scheme, focussed on field campaign measurements (including surface concentrations and lidar profiles), but this is not the goal of the present study.

2.5 Major comment 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-EuropeïÂąd’ V05, CHIMEREAfrica, 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 readableness 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.

We believe that M13 and V05 are better suited notations than CEur and CAfr as it

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follow notations in the cited papers (e.g. AG01, MB95). However, we did simplify our notations by removing unnecessary "CHIMERE-Africa" and "CHIMERE-Europe". They are, however, necessary to introduce the M13 and V05 notations. We removed unnecessary references to "CHIMERE African dust production model" apart from the abstract where there is no ambiguity. We also added some additional references to Table 1 as it sums up all these notations.

2.6 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.

We shorten the first part of the abstract and insist more on our original work and findings.

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

We replaced the term "academic test" by "idealized test".

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.

The description of the "model presented in Menut et al. (2013b)" is made on P4

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L12-14 and later on in Section 2.4. The main reason that lead us to the extension and update of the CHIMERE dust production model is described above in the introduction: a better assessment of mineral dust emission over Europe (P3 L4-18). As stated in the Introduction P4 L16-17 and in Section 2.4 CHIMERE European dust production model is a simplified model while there is a need for better estimation of mineral dust emissions. We added some precision in the Introduction.

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.

Shear stress is driven by the friction velocity which is itself driven by both the wind velocity and the aeolian roughness length. Therefore, the dust emission flux is driven by both the wind velocity and the aeolian roughness length. We added a precision about that in the manuscript.

In the idealized cases section only emissions fluxes were computed. Emissions fluxes are computed at ground level, therefore, there is no vertical dimension.

Note that only a brief description of the dust production model physical basis is made in this paper (Section 2.4). A detailed description of the physics equations of the dust

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production models would be outside the scope of this study which focus more on the required global input database for a global extension of the M13 dust production model, on erodible landuse types that must be set, on the Fecan parameterization and on the seasonal variation of vegetation. However, all necessary reference articles are cited (i.e. Marticorena et al. (1997), Alfaro and Gomes (2001) and Menut et al. (2013b)).

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.

We changed it into the manuscript.

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?

From P11 L1-7 we added each feature and explain how it affect results. And indeed the Fecan parameterization alone induce a decrease of the dust emissions. However on P11 L8-12, it is mentioned that we refer the total difference between the HUMIDITY (now SOIL_MOISTURE) and REFERENCE case, not only the impact of the Fecan parameterization (results in Table 4). And indeed, all cumulative features impact induces an increase of the total dust emissions.

We rearranged this paragraph to clarify this point.

P14 L3 and L4: Yearly variations > Monthly variations

We changed it into the manuscript.

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

For reviewer #2, this part is lengthy as for reviewer #1 this part was mainly unclear. We think that for the paper about a model development, the models used has to be clearly described. Thus, this section was rewritten to be more clear.

P17 L16: "values" > what values?

"values" refer to the averaged extinction Ångström exponent values in Table 6. We clarify it in the sentence.

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

EAE means: Extinction Ångström Exponent. We added it into the manuscript.

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