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Geoscientific Model Development
Discussions

Interactive comment on "Revised mineral dust emissions in the atmospheric chemistry-climate model EMAC (based on MESSy 2.52)" *by* Klaus Klingmüller et al.

Anonymous Referee #2

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General comments

This study presents updates of the dust emission scheme implemented in the global atmospheric chemistry model EMAC based on the previous work of Astitha et al. (2012). The land cover, vegetation topography and clay fraction maps are updated to more recent versions using higher spatial resolution. Changes are also imposed to the dust emission scheme directly. The updated dust emissions are evaluated with AOD measurements from AERONET, MODIS and IASI for the year 2011. The title, flow and structure of the paper are appropriate. All updates are well received and long needed, given the importance of quality input data to accurately parameterize physical pro-

cesses that cannot be described by first principles. However, the authors keep the evaluation part largely on the qualitative side, which does not help the reader and the community to fully understand why these changes were impactful and significant. The conclusions are also very brief for a model development/improvement paper. The authors miss a great opportunity to discuss the very interesting aspects of each revision and inform the community of which one should be considered more impactful (if not all). The specific comments below will help the authors revise the paper so it can be accepted for publication with GMD.

Specific comments/suggestions

Section 2.2 (Vegetation): Please elaborate on the calculation of fveg (Eq.1): What is the role of 0.35 and what is the meaning of fveg being 1 or less than 1.

Section 2.3 (Clay fraction): provide a map of the updated clay fraction in comparison to the one previously used in the model. It will provide context on the significance of changes that later affect the parameterization scheme.

Section 3 Page 4 (soil moisture): The soil moisture term in Astitha et al. (2012) and Eq. A1 in this paper is omitted from the threshold friction velocity. However, the authors correctly describe the dependence of dust emission on soil moisture at the end of this paragraph. What is not clear is if the statement "we consider a detailed parametrisation of the soil moisture effect to be essential to capture the observed trends in future simulations. This will require a comprehensive soil model providing accurate moisture values for the topmost surface layer" refers to an action already taken for this study or a future goal. In any case, a discussion on how the exclusion of soil moisture correction influences the simulations is important here. Page 5, (Surface friction velocity limit): a note must be placed that Eq.2 holds only when $u^* > u^*t$. Also, choosing to limit the threshold velocity to a maximum value of 0.4 m/s seems arbitrary and needs to be elaborated. What led the authors to this specific value? Some context and rationale must be provided. Page 5 (Topography factor): I am not sure of the role of the normalization factor 5.3 and how it conserves global emissions. It sounds like a tuning factor to me, so please elaborate on the role of the factor and the method used to estimate it. Page 5 (Mode mapping): This is not a strict update of the emission scheme but rather an alteration in order to use the GMXE aerosol model compared to M7 used by Astitha et al. (2012). A brief note must be included in this section to clarify that the original scheme (as well as the reference simulation herein) used a different aerosol module thus a different approach to particle size distribution. The omission of the eight transport size bins is surely a change from the original version. Figure 6: what is the higher value in this scale (above 0.1)? When we see 0.1 fraction of Ca++, does that indicate the mass, volume (or else) fraction of the total particles within each specific grid cell? A better explanation of the mineral cations fraction could be included also in page 6 (last paragraph of section 3).

Section 4.1, page 7: 1. “On the other hand, dust events observed by AERONET in January and December are reproduced by the validation simulation, but not by the reference simulation”: this comparison is not at all discernible in the plot as it is. If this is an important argument, the plot must be revised somehow to make the statement visible. 2. Given that Izana and La Laguna are within the same model grid cell, an average of the AOD from both sites could be an alternative way to compare with the model value. In addition, when evaluating numerical model simulations one can employ the nearest neighbor (as done here) or a bilinear interpolation between the observation and the model value from the four closest grid points. 3. The main criticism I have for the evaluation using the skill score (as with any other statistical metric) is the qualitative determination of which configuration provided the best results. Characterizations such as “slightly better” or “marginally larger” do not show robustness in the performed evaluation. My immediate question is: are these differences statistically significant? Are they statistically different? This is the only way to prove or convince the audience that a, say, 0.05 change in the skill score is significant enough. 4. What about using AOD of coarse vs. accumulation modes from AERONET? 5. How about using total PM concentrations wherever available (and for cases of high dust concentrations) to

evaluate model performance? An additional means of quantitative evaluation needs to be included.

Figure 8: Please consider replacing “Regions A, B,” etc. from the figure with the names of the regions as it is not convenient to go back and forth between Fig. 7 and 8 to identify the regions.

Sections 4.2 and 4.3: The scarcity of desert dust concentration measurements is a well-known problem in the modeling community when assessing model and parameterization scheme performances. This is when satellite and remote sensing observations come into play and are important tools of assessment. Nevertheless, leaving the comparison in the qualitative state only, influences the robustness of the conclusions. Looking at the IASI zoomed plots (Fig. 13), I would not immediately say that the validation is better than the reference simulation. They are different and somewhat both incorrect in my view. If the authors presented a quantitative assessment of the performance, there would be no doubt on the comparison. Also, why is the zoomed area over Middle East only? What is the special interest for this specific region? This has to be explained thoroughly so it will not be seen as “cherry picking”.

Conclusions: the conclusions are quite brief (two sentences in the end of the section). More discussion should be invoked on how the changes influence a better model performance (as long as there is a robust determination of “better” or “worse”). There are very interesting aspects in this study and it would be very useful for the community to understand how the changes that were implemented individually affect dust emissions. I believe that adding such discussion would greatly strengthen the paper.

Appendix: There are a couple of things missing in the depiction of the emission flux jemis (Eq A2): 1) I don't see the mass fraction (source to transport bins, M in Eq.4) that should be multiplied in the right side of the equation. 2) In Astitha et al. (2012) they used the relative surface area covered from particles with diameter D (Srel) to calculate the horizontal flux H. Did the authors omit this calculation in their revisions?

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