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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The mineral dust cycle in EMAC 2.40: sensitivity to the spectral resolution and the dust emission scheme" *by* G. Gläser et al.

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We thank referee #2 for his/her helpful comments. Replies to his/her suggestions are embedded below.

Specific comments

Abstract Page 27286, line 19: The reference to "very reasonable distributed emissions" must be either rephrased or accompanied by the justification of why the emissions are reasonable (based on the evaluation? on previous studies? etc).

- We add "based on a qualitative comparison to AEROCOM data" to this sentence.

Introduction Page 27287, line 20: The statement "detailed analysis and evaluation" can

stay as it is if the evaluation section is strengthened with in-situ and satellite measurements.

- In the revised version we add comparisons with in-situ measurements and satellite data.

Section 2.1 Page 27290, lines 18-19: The implemented scheme from Tegen et al. (2002) is the same with the implementation in ECHAM/HAM (Stier et al. 2005)? If yes, a note should be made in this section, as well as a small statement of the differences between the 2 models.

- It is the same. Differences between the models are for instance the radiation, aerosol sink processes, and the chemistry. We add this statement to the text.

Page 27291, line 22: The reference in the Thar desert "..of medium-fine and fine particles", according to Table 1 should have been "..of medium-fine and medium particles", since these appear with the higher fraction (0.55, 0.26). Please revise accordingly. Also the summation of the fractions in this table should have been 1 but it is 1.07 instead. Is this appropriate?

- We revise Table 1. The correct values are: coarse: 0.14, coarse-medium: 0.06, medium: 0.26, medium-fine: 0.54, fine: 0.00

Page 27291, Table 1: It would benefit the comparison of the 2 schemes if the authors could provide the threshold friction velocity from the Tegen scheme, as they do for the Balkanski scheme for the two deserts. Unfortunately, the vthr is not clear in Fig.1 for TG to avoid putting the values in Table 1.

- The threshold velocity for the Tegen scheme is 6.2 m/s in both regions. We add this information to the text.

Section 2.2 In the description of the in situ measurements (sections 2.2.1 and 2.2.2) please provide the frequency of the data (hourly, daily or else?).

- We add this information in the revised version: SAMUM: TSP 12-hourly (most of the time), PM10 daily Kleiner Feldberg: PM10 1 minute, IN 1-2 measurement per day

Section 2.2.3 In this paragraph it should be clearly noted that this is a qualitative analysis. The question arises on why the authors did not use the common MODIS, MISR, AERONET (among others) products to evaluate the model in a robust way (i.e. aerosol optical depth). I strongly encourage the authors to use the AOD from satellites and from model output to strengthen their analysis and results. In any case, the authors' choice should be justified in the text. Another option is to compare the mass concentration from the model with MODIS mass concentration (g/cm2), which will also be qualitative but will provide a general picture of the dust mass distribution globally.

- We strengthen the analysis with comparisons to MODIS vertically integrated mass concentration and five measurement data sets of dust deposition and surface dust concentration from Ginoux et al. 2001, Tegen et al. 2002, Stier et al. 2005, and Mahowald et al. 2009.

Section 2.2.4 The use of a regional model to validate the global one is not adding to the efficacy of the dust emission schemes in EMAC. These models use different emission schemes, different meteorology, projection and resolution, so the results will a-priori deviate. Again it should be noted in the text that this is a qualitative comparison.

- We completely revise the paragraph comparing to the DREAM results. In the revised version we leave out the panel showing the DREAM results in Fig. 7. A short qualitative comparison is discussed in the text.

Section 3 Page 27293, line 19: With the phrase "..is applied because it is planned to compare the results of the simulations.." the authors mean that they plan on evaluating the model with data from previous years as done in Stier et al. (2005), Huneeus et al. (2011), and several other studies? If so, why they did not use these data in this paper? Such comparison would strengthen and justify the authors' recommendation of T85 or T106 as the most appropriate setup for climate simulations. Important information

missing from the manuscript is the vertical resolution used for the performed simulations. Did the authors use the same vertical layers for all setups in the five-year time slice simulations? This information should be included in the text. The same applies for section 4 (for the dust episodes with T85TG).

- We are working on simulations for the Little Ice Age (LIA) at the moment. To compare our simulations discussed here with LIA-simulations, all the simulations have to be free-running ones because there are no nudging data for the LIA, of course. Information on the vertical resolution (i.e. 31 layers up to 10 hPa for all simulations) is added in Sect. 2.1.

Section 3.1 Page 27296, line 7: To choose a simulation closer to "reality" is a very difficult task, especially when dealing with uncertain parameters like dust concentration. It should be added here that in order to distinguish the more realistic simulation, it is necessary to use in situ measurements and satellite retrievals globally.

- With additional comparisons to measurements we justify this formulation in the revised version.

Page 27296, line 15: Is x the mean value of each field over the globe?

- Yes. But due to the comment of an other reviewer, we drop Eq. 1.

Section 4.1 Page 27300, lines 14-20: I disagree with the statement that the only possible way to directly compare simulated and measured dust concentrations in the one available station. The aerosol optical depth is one of the most common parameters to evaluate the model's performance, either locally (AERONET) or in a larger scale (MODIS, MISR, etc). Also if the authors used the available data of multi-year annual dust concentration (as in Stier et al. 2005 and Huneeus et al. 2011) and compare them to their "climatological" mean dust concentration of the 5year simulations, the bigger picture of the model performance would be present in Section 3. This would have provided a basis on all other comparisons being done with one station later in the text.

- This is correct. We add the analysis of the MODIS mass concentration and the five measurement data sets (see above) to the evaluation of all eight simulations.

Page 27301, lines 23-29: In the section of the trajectory analysis using the LAGRANTO model, the authors should include some information on the emissions used for the trajectories. Are they taken from EMAC? The values we see in figs 8 and 11 and in the text for dust emission and concentration are calculated with EMAC? Does LAGRANTO take into account deposition processes? This information is important to understand the significance of the dust concentration and emission discussed along the trajectories in this section.

- LAGRANTO calculates the trajectories from the EMAC output. The variables along the trajectories show the EMAC values. We add this information to the text.

Section 5 The summary and conclusions section is well written and points out the most important findings and weaknesses of this study. My only addition is to point out that a proper evaluation of the model using satellites and in situ measurements in several locations globally could lead to a solid recommendation on the scheme and resolution for studying the mineral dust cycle with the EMAC model.

- With the additional analyses in the revised version we strengthen our evaluation.

Technical corrections

Page 27292, line 22: Please rephrase the sentence "..to Germany since five years". Maybe the authors mean "..since the last five years"?

- This is correct. We change this.

Page 27293, line 15: correct the word "deteils" with "details".

- We correct this typo.

Figure 6: This figure is quite difficult to read since the boxes and the abbreviations make it too complicated. I encourage the authors to remake this figure in the same

way as figure 10. Also, in figure 6, the vertical axis title is missing and also the notation that the black line is the model result.

- We delete the vertical lines. The measurement data are plotted as a histogram as it is in Kandler et al. 2009. This is the best way to show these data because it illustrates exactly for which time one data point is valid. The horizontal bars are needed to give the mean values of the shorter periods. We add the title to the vertical axis and the notation of the black line.

Figure 6, caption: Correct the word "dottet".

- We correct this typo.

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