

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.

Anonymous Referee #2

Received and published: 4 November 2011

General comments

This paper presents an analysis of the performance of two dust emission schemes implemented in the ECHAM5/MESSy2.40 global atmospheric chemistry model, with a focus on the model sensitivity to the spectral resolution. The title of the manuscript reflects the contents of the paper and the abstract is sufficient.

In terms of scientific quality and significance, the manuscript does not include a development of a new dust emission scheme but a brief description of two dust emissions schemes (the default and a new implementation based on Tegen et al. 2002) which have been described elsewhere in detail. The most significant aspect of the manuscript is the analysis of different spectral resolutions and how these affect the dust production and circulation globally. This result is important and useful for global climate simula-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



tions that include the dust cycle. The major deficiency of this analysis is the lack of a detailed evaluation of the dust schemes using available observations (dust concentration and aerosol optical depth) in several locations worldwide. The current evaluation is based on measurements from 2 stations (Morocco and Germany) and qualitative comparison with a regional model and the dust RGB composite of the Meteosat satellite. In my opinion, this is necessary in order to conclude on the “reasonable” dust emission scheme. I am in favour of publishing this article in ACP, if some important issues that have arisen during the review can be sufficiently answered by the authors.

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).

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.

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.

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?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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 v_{thr} is not clear in Fig.1 for TG to avoid putting the values in Table 1.

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?).

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 ($\mu\text{g}/\text{cm}^2$), which will also be qualitative but will provide a general picture of the dust mass distribution globally.

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.

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), Huneus 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).

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

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 Huneus 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.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the significance of the dust concentration and emission discussed along the trajectories in this section.

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.

Technical corrections

In general, the text is well written and clear. Only 3 grammatical errors were found. One requested change in figure 6 is the most important technical correction.

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

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

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.

Figure 6, caption: Correct the word “dottet”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27285, 2011.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

