

Reply to referee 1

We thank the referee for the comprehensive and constructive review. All aspects have been addressed in a revised manuscript and a new supplement. Below please find our point-by-point reply to the comments.

This paper entitled “Revised mineral dust emissions in the atmospheric chemistry-climate model EMAC (based on MESSy 2.52)” and submitted to GMD presents new developments concerning the parameterization of dust emissions in the global model ECHAM/MESSy. These new developments have been evaluated and compared to the previous version of the model in terms of the resulting aerosol optical depth. The use of ground-based (AERONET) and satellite (MODIS, IASI) has shown the improvement brought by this new version. This paper is therefore interesting for the community working on dust modeling, and the manuscript is well written. However, the current version needs major revision before considering the publication in GMD because of the following points:

- The evaluation of the revised emissions is limited to the aerosol optical depth, which is not enough to estimate the quality of the parameterization. AOD is indeed a relevant parameter to evaluate the integrated effect of dust aerosols on radiation, but it can hinder some compensating errors. Besides, such parameters as the dust size distribution, the dust vertical profile or dust deposition are essential for radiative budget and effects on climate, and are not constrained by AOD. The authors could for example add an evaluation of surface concentrations, dust deposition, dust emission fluxes or dust vertical profiles, as done by similar recent studies (Kok et al., 2014; Albani et al., 2014; Klose et al., 2014; Gherboudj et al., 2015).

We appreciate the advice and have added the evaluation of surface concentrations and dust deposition for an even more comprehensive validation. Regarding the current evaluation we would like to point out that our comparison with the satellite retrieved 10 μm DAOD goes beyond the evaluation used in other studies and, combined with the AOD comparison at visible wavelengths, amongst others probes aspects of the particle size distribution.

- As there are many papers on dust modeling, the authors should highlight more the originality of their work. In this purpose, they should add a paragraph in the introduction presenting the state-of-the-art in dust modeling in global chemistry-climate models. This would be useful for the whole community, and would not restrain the impact of the paper to the ECHAM community as it could be the case with the current version of the paper.

We have extended the introduction accordingly.

Specific comments:

- Abstract: The authors mention several times the possibility to run high resolution simulations. What is the targeted resolution? Do the scheme need any modification for this high resolution?

The upper limit of the target resolution is given by the resolution of the updated input data. The target will be T255 (about 0.5 degree at the equator) or higher (which so far is only mentioned in the conclusions). With at least 0.1 degree resolution, the new input data will also serve considerably higher resolving simulations. Also the emission scheme itself can be used straight forwardly at higher resolution. As it is not entirely resolution independent the overall scaling might need to be adjusted. We have added the following sentence to the introduction:

Page 2, line 29 “To equip the model for simulations at resolution T255 (about 0.5 degree) or higher, new input data should have at least 0.1 degree resolution.”

And in section 3:

Page 6, line 7 “When switching to different model resolutions, the scaling factor can be used to balance potential resolution dependencies of the emission scheme.”

- Page 2 Lines 18-19: The authors should justify the “rapid changes of deserts and semi-arid regions in recent decades”

References to Figs. 1 and 2 and literature have been included.

- Page 3 Section 2.1: Looking at Fig.1, I get the impression that there are more regions with shrinking deserts, is it true?

That is correct, the area with positive correlation coefficient covers $1.3 \cdot 10^6 \text{ km}^2$ globally which is about half the area with negative correlation coefficient ($2.6 \cdot 10^6 \text{ km}^2$). Additionally, the regions of shrinking deserts are spread over a larger area because they are predominantly surrounding the large deserts whereas expanding source areas are located more centrally. We have added the numbers to the text.

- Page 3 Line 27: Any justification for the equation (1) giving the vegetation factor? Is it used in other models?

The vegetation factor is the same as used by Astitha et al. 2012 and interpolates linearly between full emissions for no vegetation and entirely suppressed emissions for $\text{LAI} > 0.35$; the threshold value was introduced by Mahowald et al. 1999. We have added the references.

- Page 3 Lines 29-30: Could the authors clarify which statistical test they have used?

The trend has been calculated for each pixel by fitting a linear regression model to the time series of annual average LAI values using least squares. The resulting slope yields the trend and is considered significant (i.e., it is plotted in Fig. 2) if the corresponding p value is below the significance level of 0.05. We have rephrased the sentence.

- Page 4 Section 2.3: Contrary to Sections 2.1 and 2.2, the authors have not elaborated on the differences between the two versions of the clay fraction maps. Which is the expected impact on dust emissions?

The two versions of the clay fractions are now compared in Figure S1 in supplement. The impact on the dust emissions is discussed in the new section 4. The expected impact of the new data is a better representation of details below the 1 degree resolution such as river valleys. Moreover, as mentioned, the new data is more appropriate to represent the relevant topmost soil layer. As the clay fraction map is assumed to be static (based on the longer typical time-scales of the relevant geological processes), unlike in Sect. 2.1 and 2.2 we could not perform trend and variation analysis.

- Page 4 Lines 26-29: Is there any work forecast to include again the effect of soil moisture on dust emissions? It might be important in some regions like Sahel.

We agree that the effect of soil moisture is important in regions like the Sahel (or the Middle East) and it would be very desirable to consider it in the model. A prerequisite would be a soil model more detailed than the current bucket model to obtain soil moisture values for only the topmost surface layer. While the inclusion of new soil models in EMAC is discussed, to our knowledge one has yet to be implemented.

- Page 5 Line 5: This equation differs from the one given in Astitha et al. (2012), the authors should correct it or explain why it is different.

Eq. (9) used by Astitha et al. (2012) implies that the horizontal flux H is proportional to

$$\begin{aligned} u_*^3(1 + u_{*t}/u_*)(1 - u_{*t}^2/u_*^2) &= u_*^3(1 + u_{*t}/u_*)(1 + u_{*t}/u_*)(1 - u_{*t}/u_*) \\ &= (u_* + u_{*t})(u_* + u_{*t})(u_* - u_{*t}) \end{aligned}$$

which agrees with the RHS of our Eq. (2).

- Page 5 Line 9: The authors should justify the choice of 0.4 m/s, and clarify what they call “good results” explaining what has been compared.

The value is justified by the results presented in Sect. 4 which are good in the sense that the validation simulation produces, compared to observations, significantly more realistic results (in terms of skill scores and correlation coefficients) than the reference simulation using the original emission scheme of Astitha et al. (2012)

which already proved to yield realistic results in other studies. We have added results from simulations using different limits in the supplement (Figure S3).

- Page 5 Line 29: The parameter d_{max} could be added in Table 2

d_{max} has been added to the table.

- Page 6 Lines 10-15: I did not understand if finally the chemical composition of dust is included or not in the model.

As mentioned on Page 6, lines 31f the chemical composition is included in the model for both, reference and validation run. As the corresponding changes to the dust emission scheme are independent of all other modifications, do not affect total dust emission flux and can be used with the original and the revised emission scheme, their effects (see Karydis et al. 2016) have been excluded from the evaluation, but code and data are released with the revision presented here.

- Page 6 Lines 19-21: The list of submodels is unclear for readers not familiar to the model. The authors should add a reference to have the details about these parameterizations.

We have added a reference (http://www.messy-interface.org/current/auto/messy_submodels.html).

- Page 6 Line 25: What is the Tanré climatology used for? (AOD or only other optical properties?)

It is used for extinction, single scattering albedo and asymmetry factor (now mentioned in the text).

- Page 6 Line 30: A reference to Table 1 should be added to present the simulations.

The reference has been added.

- Page 6 Line 32: Is a one-year simulation long enough to evaluate the revised dust emissions? Is there any reason to select the year 2011?

While longer simulations are preferable, the one-year period suffices to yield statistically significant differences between reference and validation simulation and has been chosen considering the computationally expensive model setup used. The year 2011 has been selected to represent a recent period well past the time period on which the old, outdated input data is based on, and to allow to continue the simulation within the period of available new input data in case this would have been necessary to collect more statistics.

- Page 7 Line 15: Which level of AERONET AOD has been used in this comparison?

Level 2 data has been used (now mentioned in the text).

- Page 7 Line 17: Maybe the authors should divide the region B in two sub-regions, for the reader to identify more easily the different stations.

We have divided the region into a northern and southern part.

- Page 7 Lines 29-30: I don't understand how this skill score based on correlation can be affected by a bias.

The overestimation of the AOD during dust events results in an overestimated amplitude of the AOD variation between dust-free (when both reference and validation simulation yield AODs close to zero) and dusty periods. Accordingly, variance and standard deviation are overestimated, the latter entering the skill score defined in Eq. (5).

- Page 7 Section 4.1: It could be also useful to add one or two time series in stations where the score has increased.

We have added time series plots of the five stations with the largest increase to the supplement (Fig. S5).

- Page 8 Line 1: Which is the altitude of the model grid cell?

The model grid cell has a surface altitude of 63 m which we now mention in the text.

- Page 8 Line 21: Is this increase of spatial correlation statistically significant?

The statistical error of the numbers is small, the error estimates obtained by jackknife resampling of the more than 10^5 pixel values are 0.004 for the correlation coefficients and 0.006 for the skill scores. Therefore, the digits provided are presumably exact and the probability for the increase under the null hypothesis (assuming no improvement) is virtually zero.

It should be stressed that the improvements reflected by the numbers are substantial and the improvement of the global AOD distribution is a major advantage of the revised emissions.

- Page 8 Lines 28-30: The authors could think about adding a score for the measuring the improvement in seasonal cycle, which could reinforce the robustness of their results.

We have added statistical analysis of the seasonal AOD and DAOD values over the Arabian Peninsula to the supplement (Figs. S6, S7).

- Page 9 Line 17: Same remark for the significance of the increase in the skill score.

Here, the error estimates for the reference results are slightly larger than above (0.012 for the correlation coefficient, 0.017 for the skill score) due to the distinct peaks in the reference DAOD distribution, but still very small compared to the increase due to the revised emissions, therefore again the increases are highly significant.

We apologise that the numbers in the text (page 9, lines 16f) are not the correct numbers provided in the caption of Figure 12. This has been fixed.

- Page 9 Line 32: The time dependence of land cover and vegetation has not been tested here because the simulations were too short.

We agree, however, using the input data based on observations from the year simulated (2011) likely contributed to the more realistic results.

Technical comments:

- Page 2 Lines 8-9: The abbreviations DU_Astitha1 and DU_Astitha2 are useless since they are not used in the rest of the paper.

The abbreviations are the names of the corresponding options in the EMAC setup. To unambiguously specify the emission scheme our study builds on, we would like to keep mentioning the names here.

- Page 6 Line 24: ISORROPIA

The typo has been fixed.

- Figure 1: The color bar should be changed, because the values below -0.2 cannot be distinguished.

The contrast has been increased.

- Figure 8: The authors could replace the letters (A, B, etc.) by the name of the regions in the blue line at the top of the figure.

We have introduced more descriptive abbreviations.

- Figure 9: AERONET data is represented with dots in the figure, while it is a line in the caption.

This has been fixed.

- References: The format needs to be homogenized (notably the use of first names for the first author).

The bibliography has been revised.