Reply to Interactive Comment on "New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC" by M. Astitha et al.

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Review of "New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC by Astitha et al. for publication in Atmospheric Chemistry and Physics.

The paper presents a parameterization of dust aerosol emissions based on soil texture data for inclusion in a global climate model. The model is run for the year 2000 with two instances of the dust source function, one in which soil properties are assumed globally uniform and the other invoking the new parameterization in which soil properties are from a gridded database. For each source function, two instance of the model are run, one where the model is run as a climate simulation and another in which the model is run with "nudged" meteorology in order to more realistically simulate the particular dynamics of the year 2000. Model surface mass concentrations, deposition, and aerosol optical thickness are compared to available observations. Overall it is found that the model reasonably simulates dust transport patterns and amounts. There is some difference between the two different versions of the dust source formulation, but larger differences between the climate and "nudged" simulations than between the source simulations.

The paper is well written, well organized, and relatively complete. I mostly have minor comments on the text. I do have a criticism in that the figures are really quite small and therefore hard to read, especially given the sort of detail one is expected to extract (the color of the dots on Figure 6, 7, 9, 12, and 13 are almost completely impossible for me to distinguish, as are the differences in the maps in Figure 4).

I'm going to recommend the paper be reconsidered after major revisions. I point out below what I think is flaw in the formulation of the soil dependent dust emissions. Either I don't understand something about what was done, or something was done that I think is flawed. If I prevail in convincing the authors of a flaw there it suggests either further simulations or else a discussion of the implications. Again, see below.

[Author reply]: We would like to thank Dr. P.R. Colarco for his valuable comments and constructive criticism on the manuscript. The most important comment on the formulation of the soil dependent emissions is addressed in detail below (comment No5). We hope that with our detailed explanations we will resolve this misunderstanding and succeed in pursuing the reviewer that there is no flaw in our formulation. It is based on previously published work and allows the investigation of a different way of attributing a size distribution to the emitted dust particles.

Regarding the reviewer's comment on the figures: The figures 4, 6, 7, 9, 12, and 13 are improved in the revised version of the manuscript (larger coloured dots in the scatter plots and larger plots in Fig.4) and we will make sure that in the final version all figures will be easily readable.
The notation on the pages and lines refer to the version of the manuscript that was reviewed by the referee. Changes are made in the revised version of the article submitted with the responses to the reviewers.

Furthermore, the significance of the work needs to be better discussed. I understand the appeal of moving toward more physically based models, but I'm not convinced that this work shows "the need to represent arid region as individually and explicitly in global models." For one thing, a single year was simulated. Significance could be more quantitatively assured by looking at more than one annual cycle. For another thing, the dust schemes are still tuned, and tuned differently from one another. So you have a tuning factor of 1e-4 for DU1 and 1e-3 for DU2. These factors are usually selected to give a desired model value: average emissions, or loading, or optical thickness. So how are these numbers chosen in this case, and would slightly adjusting one or the other improve the agreement between the two models (which isn't too bad as it is)?

[Author reply]: The purpose of this work was to implement a dust emission parameterization scheme in the EMAC model, progressing towards a more detailed physically-based emission scheme compared to the previous one included in the model. The new scheme facilitates the online use of meteorological fields and terrain characteristics (soil texture and size), avoiding the a-priori characterization of dust sources and pre-calculated tables. This was done not by implementing a ready-to-use code but building the emission scheme from scratch following the "MESSy/EMAC" coding standard (Joeckel et al. 2006). The need to investigate the differences between a simple version of the dust scheme and a more complex one evolved from the progress of our work.

The phrase we have used in the abstract "*the need to represent arid regions as individually and explicitly in global models*" originated when the comparison of the 2 formulations of the dust emission scheme showed that even when adding the soil properties explicitly, the simulations were not substantially improved. With this statement we meant that it might be beneficiary to the performance of the model if we could apply different scaling factors or different schemes in each arid region, given the heterogeneity of the soils that sometimes leads to different dust entrainment pathways (i.e. Asia versus Africa). This phrase hides some speculation as we did not give specific suggestions on how to accomplish such improvement and we have removed it from the abstract. We agree with the reviewer that by simulating more than one year, we could quantify the significance of representing the arid regions explicitly in the model. This is why we did not attempt to quantify this significance or speculate on how each arid region should be represented in the context of our modelling system. The work presented here is the implementation of 2 formulations of the dust emission scheme and the evaluation of the results using the largest possible number of observations. We are planning to simulate additional years but are currently restricted by the available CPU time on our large cluster whereas simulations on our small cluster would lead to a strong delay. A sentence has been added to the abstract and conclusions, to make the significance of this work more obvious to the readers. Based on the reviewer's comments we have changed the title of the manuscript to "Parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC: impact of nudging and soil properties" in an attempt to be more precise about the topic.

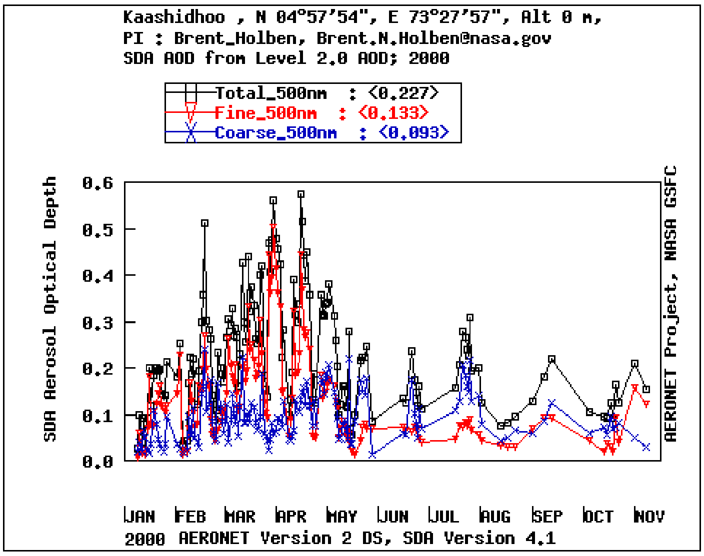
For the reviewer's comment on the scaling factors, we need to state that models using online dust emission modules are faced with the necessity to tune the modeled dust fluxes towards observed values depending on the parameterization scheme they apply. Ginoux et al. (2001) used the scaling constant C (equation 2), Zender et al. (2003) the global tuning factor T (equation 17), Li et al. (2008) used the Ginoux approximation, Perez et al. (2011) the global tuning factor C (equation 11), Ridley et al. 2012, among others. Others have used a tuning factor to the erosion threshold to lower the threshold friction velocity and ensure a correct dust production (Tegen et al. 2006; Prigent et al. 2005; Heinold et al. 2007, 2009; among others). This is especially true for global models where the coarse resolution prohibits the representation of small scale dynamical processes. One could avoid the use of a scaling factor by injecting the dust particles every 1h, 2h or 6h, as has been done in the past (Tegen et al. 2002). We have chosen to assign the emissions in every time-step (10min) as this is more consistent with meteorological conditions. The tuning values in each simulation were chosen on the basis of tuning the modelled dust emissions and concentrations towards published and observed values, thus producing reasonable results. Since the tuning factors are uniformly applied to the entire domain, adjusting the 2 values will not improve the agreement between the 2 formulations as a whole, only in selected regions. The regional differences of the 2 formulations can be "tuned" towards a certain similarity only if different factors are applied to each arid region. Fig.S2 from the supplement shows the regional differences of the 2 formulations per season; for example, in N. Africa, there are positive and negative differences between the 2 formulations that will not be adjusted by the change of the globally uniform tuning factor. Nonetheless, we agree that the use of tuning factors is unsatisfactory and hope to reduce/avoid them in subsequent work.

Minor comments

1. Page 13241: In the model description, please say whether the aerosols are radiatively coupled to the model. I gather not. Additionally, somewhere in the presentation of the dust optical thickness please say something about the dust optical properties. Are other aerosols simulated and included in the computed optical thickness? It seems that other aerosol species are run, but I'm not entirely clear on this point (see page13261, line 1, which suggests poor AOT comparison to the station in the Maldives, which is likely influenced by pollution. Are you simulating this pollution?)

[Author reply]: The aerosols are not radiatively coupled to the model in this work for two reasons: first, the dust production process needed to be evaluated before introducing its feedback to the meteorological conditions and secondly because no full aerosol chemistry is used in the simulations (also in page 13241). A sentence has been added in the text to clarify this issue.

In section 4 (page 13252, lines 15-16) there is a brief description of the modelled AOD: "*The modelled AOD is calculated at 550nm using concentrations of dust and sea salt particles and biomass burning products (black carbon and organic carbon)*". This is done by the submodel AEROPT (Table 1 with references to the work of Lauer et al. (2007) and Pozzer et al. (2012)). We have added additional information in this section according to the reviewer's comments. For the example at the Maldives station (Kaashidhoo), we believe that the discrepancy between observed and modelled AOD is because of anthropogenic pollution that is not simulated by the model (only anthropogenic sulphate aerosols are produced). This is supported by the SDA Fine/Coarse AOD given from AERONET for this station (http://aeronet.gsfc.nasa.gov/new_web/data.html; see plot below with daily averages), that shows fine mode AOD dominating in the months January to April and November and the coarse mode AOD being higher in the summer months. This is a coastal station that is also influenced by sea salt particles, thus the contribution in the coarse mode AOD is from both dust and sea salt particles (such contribution cannot be quantitatively derived from the AOD evaluation). The discrepancies between model and observations (Fig. S3) are explained in the text based on this information.



2. Page 13247, line 21: Reference to equation (10) does not belong here, as it is not unique to DU2 formulation. It appears to be used the same in both formulations.

[Author reply]: Amended.

3. Page 13248, line 22: You don't mean "soil size distribution" here. What you mean, and what Zender et al. are doing, is using the d'Almeida size distribution to represent the aerosol particle size distribution at the source.

[Author reply]: This phrase is replaced by "the particle size distribution at the sources...".

4. Page 13249, line17: Please use "a" rather than alpha in equation (12) to be consistent with equation (10). Likewise in the following text.

[Author reply]: This was a mistake during the file format conversion that we did not see in the proof reading of the article. It has been corrected.

5. Page 13250, line 11: I think there is a flaw in equation 13, but please convince me I'm wrong. This goes back to my previous comment about the d'Almeida size distribution. The horizontal flux (equation 9) should depend on the soil texture and soil particle size distribution, but it's less clear to me that the aerosol particle size distribution (equation 13) has that same dependency. Go back to Marticorena and Bergametti (1995) and Marticorena et al. (1997). You're following up on their formulation, but with global datasets of soil texture. They provide the vertical aerosol mass flux, but not the particle size distribution because there is not this clear relationship between soil particle size distribution and aerosol particle size distribution. It's up to the modeler to impose the particle size distribution of the emitted aerosol. Zender chose the d'Almeida "background dust" PSD, which is what you choose also for the DU1 simulation. Alfaro and Gomes (2001), for example, suggest that the initial particle size distribution is best represented as a function of the surface wind speeds, with different proportions of three size modes that depend on wind speed and not so much on soil characteristics. In the DU2 formulation you are imposing the soil particle size distribution on the emitted aerosol. But this seems wrong, since the mechanism for injection of aerosol is disaggregation of soil particles. So I presume this answers the question of why the tuning factor for DU2 is an order of magnitude greater than for DU1: your vertical mass flux must be apportioned over the four modes of the soil distribution, which may be quite large, depending on the soil type, and so most of the mass is simply not carried in the size bins you care about and need to bump up the overall emissions to get a reasonable load in the 0.2 - 20 um diameter range you care about. So, I presume the information in Figure 4 and Tables 5 - 6 pertains only to the emissions of dust in the 0.2 - 20 um diameter size range. Is that right? The some of the differences you see are not because of differences in where emissions occur (soil dependence) but because of differences in the emitted particle size distribution, reflected in different lifetimes of the DU2 versus DU1 cases, suggesting DU2 carries more mass at larger sizes.

[Author reply]: The most important comment by the reviewer is addressed in the discussion that follows. The structure of Equation 13 may have caused a misunderstanding since it is not written exactly as in Marticorena and Bergametti (1995) or Marticorena et al. (1997). It is similar to equation 17 of Zender et al. (2003) and is provided below (also changed in the manuscript):

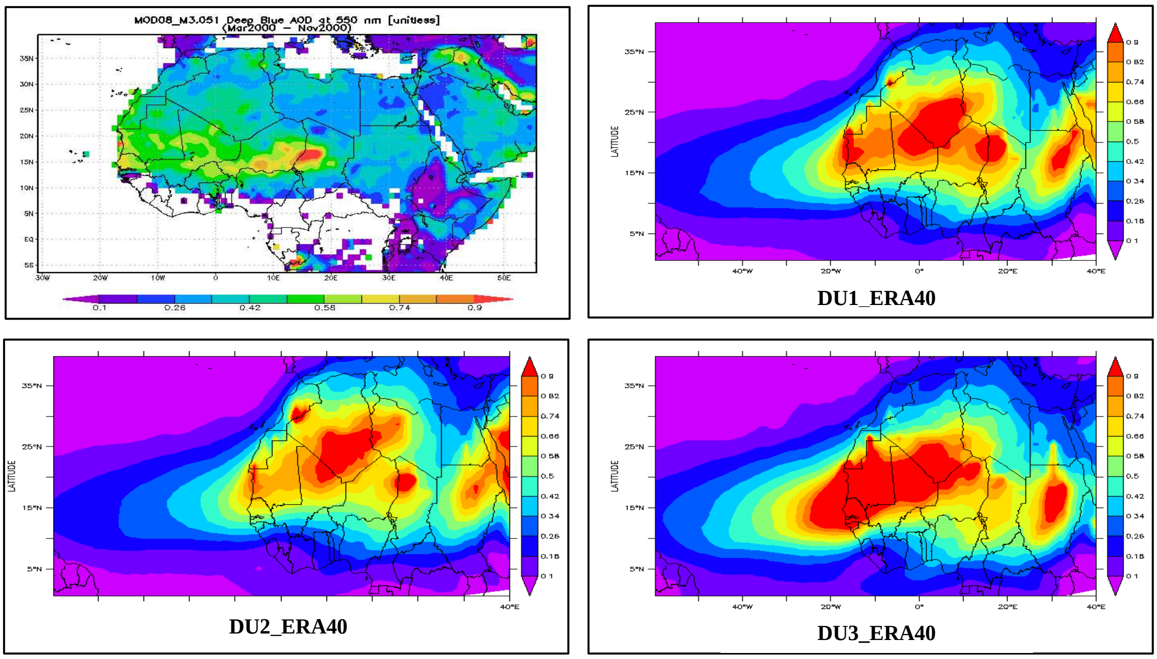
$$V_j = F_2 \; B \; a \; M_j \; \underbrace{\frac{c \; \rho_{air} \; u_*^3}{g} \sum_{D_p} (1 + \frac{u_{*t}(D_p)}{u_*})(1 - \frac{u_{*t}^2}{u_*^2}) S_{rel}(D_p)} \qquad (13)$$

The term indicated by the curly bracket is the total horizontal flux H of the material mobilized by wind and is identical to the original equation of Marticorena and Bergametti (1995), also appearing in recent publications of Laurent et al. (2010) among others. The horizontal flux depends on the diameter of the soil particle D_p and the relative surface covered by the soil particles with diameter D_p (S_{rel}). The calculation of S_{rel} includes the dependency from the soil surface median diameter and geometric standard deviation (soil size population). In other words, the S_{rel} is different for each soil type (as each soil type contains different amounts of soil particles, e.g. the fine soil type includes 33% of dust mass with D_p=15μm and 67% with D_p=2μm). The total H for each grid cell depends on the soil texture and particle sizes assigned to that grid cell and this dependency is transferred to the calculation of the vertical flux V through equation 13. Also V depends on the sandblasting efficiency (α), which depends on the soil properties (clay fraction). In principle there is no error in Equation 13. What is different from other implementations is the calculation of the vertical flux V_j for every particle size bin j. We believe that the choice of using the soil size population instead of a fixed size distribution (as in DU1) is the main objection of the reviewer and we explain our rationale below.

As the reviewer stated, the particle size distribution of the emitted dust particles is not explicitly given in the Marticorena and Bergametti (1995) formulation, and the modeller decides how to make the transition of the particles from the soil to the atmosphere. The saltation process is described by the calculation of the horizontal flux that accounts for the selective mobilization of the soil particles according to their sizes and includes the contribution of each size range (Marticorena et al. 1997). But the sandblasting process which describes the release of dust and larger particles by the saltators as they impact the surface (disaggregation of soil particles) is not represented by the Marticorena and Bergametti scheme as discussed in Grini and Zender (2004) and Grini et al. (2002). In other words, we cannot explicitly account for the sandblasting with the current implemented scheme in the model and we believe this was the main issue raised by the reviewer. To partially compensate for this deficiency of the scheme we tested the use of the soil size distribution for the emitted dust. The horizontal flux is an integrated value that does not include any information on the aerosol size distribution, neither does the vertical flux V. At this point, each modeller assigns a size distribution (d'Almeida, Alfaro and Gomes, among others) uniformly in every grid cell. These assignments are necessary but also arbitrary, since no clear relationship is known for this parameterization and the assignment actually gives a fixed "preferred" value in each grid cell (remember the term $M_j = \sum_{i=1}^{3} m_i M_{i,j}$ in DU1, a 1-dimensional parameter that calculates the fraction of mass from the fixed d'Almeida distribution to each one of the 8 size bins). Some models have simulated the dust size distribution that reflected the original soil size spectrum (Heinold et al. 2007, 2009, based on Tegen et al. (2002)). We have proceeded with the latter, in order to investigate the effect of such implementation in the global dust fields compared to the "safer" option. In this case, the parameter Mj in equation 13 is spatially different, depending on the soil texture and size distribution. Mass from the larger soil sizes is transported to the 8 size bins but to a lesser extent as the reviewer already indicated, compared to the fixed mass fraction used in DU1. This does not make this formulation erroneous, just another approximation of the emitted size distribution. In DU2 we do not assume that 95% of dust emitted mass lies in the size 4.8μm (as in d'Almeida) but we attribute this through the soil properties.

The implementation of the dust scheme shows that the dust sources are correctly located and the evaluation has shown that differences exist between the 2 versions without dramatically improving one against the other simulation. To support our research direction, we provide AOD plots for Africa compared to MODIS Deep Blue from 3 simulations: DU1_ERA40 ($F=10^{-4}$), DU2_ERA40 ($F=10^{-3}$) and a third one (unpublished) that follows Laurent et al. (2010) implementing the dry-sieved soil size distribution, the Alfaro and Gomes (2001) emitted size distribution (at the source) and a tuning factor F=2*10^{-3}. The modelled AOD is averaged over March to November to agree with the available MODIS AOD.

These 3 simulations emerged from different formulations of the same dust emission scheme, and their similarities and differences do not depend on the tuning factor alone. Finally, given the high uncertainties of the soil classifications and the size distribution of the emitted dust particles in the literature, we believe that it is scientifically acceptable and useful to conduct such an investigation.



The dust emissions in Tables 5 and 6 and Fig. 4 (also Fig. S1, S2) are in the diameter size range 0.2-20µm. This is the size range that the dust emitted flux is adjusted to after the emission process to describe the transport of dust particles away from the source. It does not include larger particles since they are supposed to be deposited quickly and locally after being emitted. We agree with the reviewer in his comment on the differences between DU1 and DU2 and we have included this discussion in the manuscript. We must add, though, that the differences occur for both reasons: differences in the locations of emissions (soil dependence) and in the emitted particle size distribution. This is justified by the fact that the vertical flux in DU2 includes the soil dependence of the Zobler classification (Table 3) in the calculation of the horizontal flux (Equation 13, term indicated by the bracket), which is not included in the DU1 scheme and also the difference in the emitted size distribution. The lifetimes of the dust particles are not strongly affected because the dust mass is distributed in the 8 size bins in both DU1 and DU2, before being transported, advected and deposited.

We hope we will convince the reviewer on the scientific credibility of our formulations and the rationale behind investigating different implementations of the same dust emission scheme. In any case, we would be happy to include any new suggestions by the reviewer if he thinks it is appropriate.

6. Page 13255, line 2: I don’t understand this conclusion. The year 2000 simulation with the nudged meteorology looks pretty good compared to the year 2000 observations. But that the free running model for year 2000 has a different amplitude (but same seasonality) as the climatological observations does not impugn the quality of the free running model. Is the free running model way outside the variability in the climatology of the observations? That the year 2000 observations shows this bi-modality and is lower by about 30% than

the climatological peak in July suggests there is maybe significant interannual variability in the observations. The free running model is one possible realization of that variability. Running it for another year and comparing to the climatology you might reach a different conclusion.

[Author reply]: We agree with the reviewer's comment in the sense that we cannot draw a general conclusion on the quality of the free running model by using only one simulated year. We have removed this conclusion from the text, adding the necessity for multi-annual simulations to compare with multi-annual observations. We have to add a comment on the part of the comparison between the 2000 and the multi-annual observations for the Miami station: the multi-annual observations do not include the year 2000; they cover the period 1989-1998, thus we cannot study the variability by comparing the two. We have information on the standard deviation of the multi-annual measurements (as given by J. Prospero) that indicates strong variability for the summer months (see Fig.5 at the end of this document), as pointed out by the reviewer. We have added this information in Fig.5 to show that the free running model simulation is within the variability of the observations, without generalizing this conclusion.

7. Page 13257, line 18 (and Figure 8): You indicate in the text you are talking about the budged simulations. Please reference here and in the caption at DU1_ERA40 and DU2_ERA40.

[Author reply]: Amended.

8. Page 13262, line 9 (and Figure 14): Why are you showing the mass loading from MODIS? This is a by-product of the retrieval algorithm in MODIS, and drops out of the otpical models used to make the retrieval. It is unvalidated, so far as I know. Why not instead just compare to the optical thciness retrieved by MODIS? That at least has some validation behind it. Better yet, compare to the coarse mode optical thickness, which would tend toward removing biomass burning and Asian pollution hotspots.

[Author reply]: We have chosen the mass loading from MODIS for a qualitative discussion on the spatial distribution of the mass in the global scale. Even though this is a by-product (as we also discuss in the text) it gives an overall view of the transported mass loading and we can qualitatively compare the values from the model and the satellite retrievals.

After the reviewer's suggestion we have included the AOD plots for the month of June (also included at the end of this document). A discussion is added in the text in Section 5.2.3. We were not able to find gridded global maps of the coarse mode AOD, neither in the MODIS nor the MISR database; only AERONET offers such information and also MODIS provides the small fraction mode that indicates the location of the fine and coarse particles without providing a quantitative assessment of the model performance. Based on the above, we included the AOD from the MODIS database in line with the reviewer's suggestion. We will follow any suggestion from the reviewer for specific databases of global maps of coarse mode optical thickness that we are not aware of.

We have also plotted the annual AOD from the model and from the MODIS retrievals. The months included are March to November 2000, due to the availability of the MODIS data. We added the aerosol small mode fraction averaged over the same period to indicate the locations of fine and coarse particles. The graphs are included at the end of this document to show the overall performance of the model in an almost annual base. We have added this figure in the supplement of the paper to avoid a bigger number of graphs in the main article. A discussion is also included in the text.

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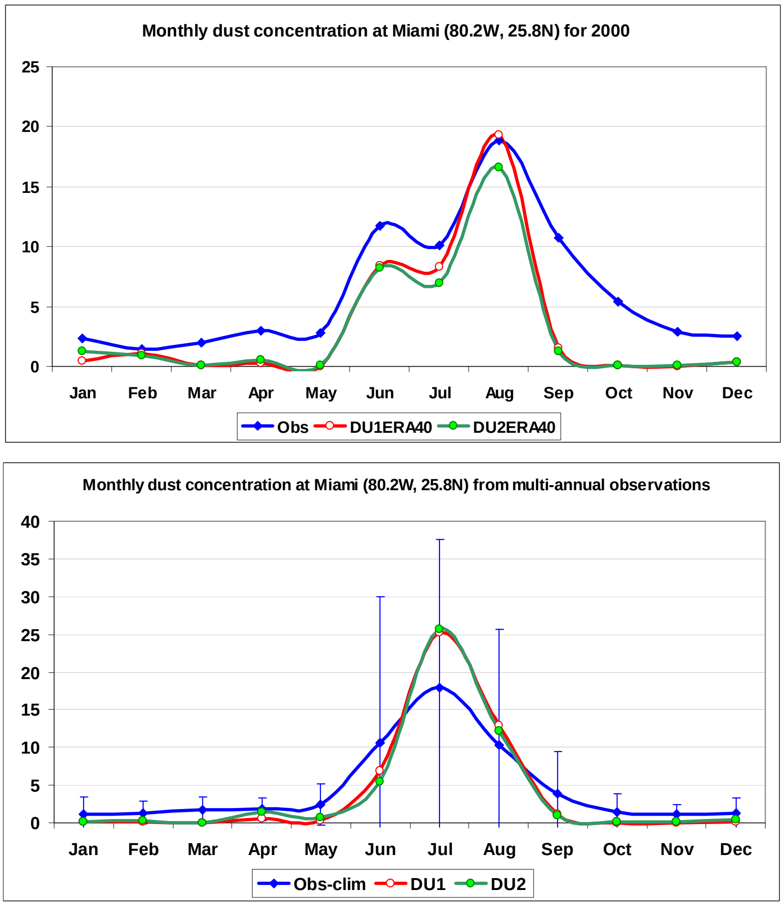

Fig. 5. Comparison of monthly modelled and measured dust concentrations (μg/m^3) at the Miami station. The upper panel shows results from the nudged simulations and the lower panel from the free-running simulation. Measurements for the year 2000 are indicated in blue line in the upper panel; the climatology of the station (multi-annual averages) is shown in blue line in the lower panel including the standard deviation for each monthly average. The model results from DU1 are shown in red and from DU2 in green.

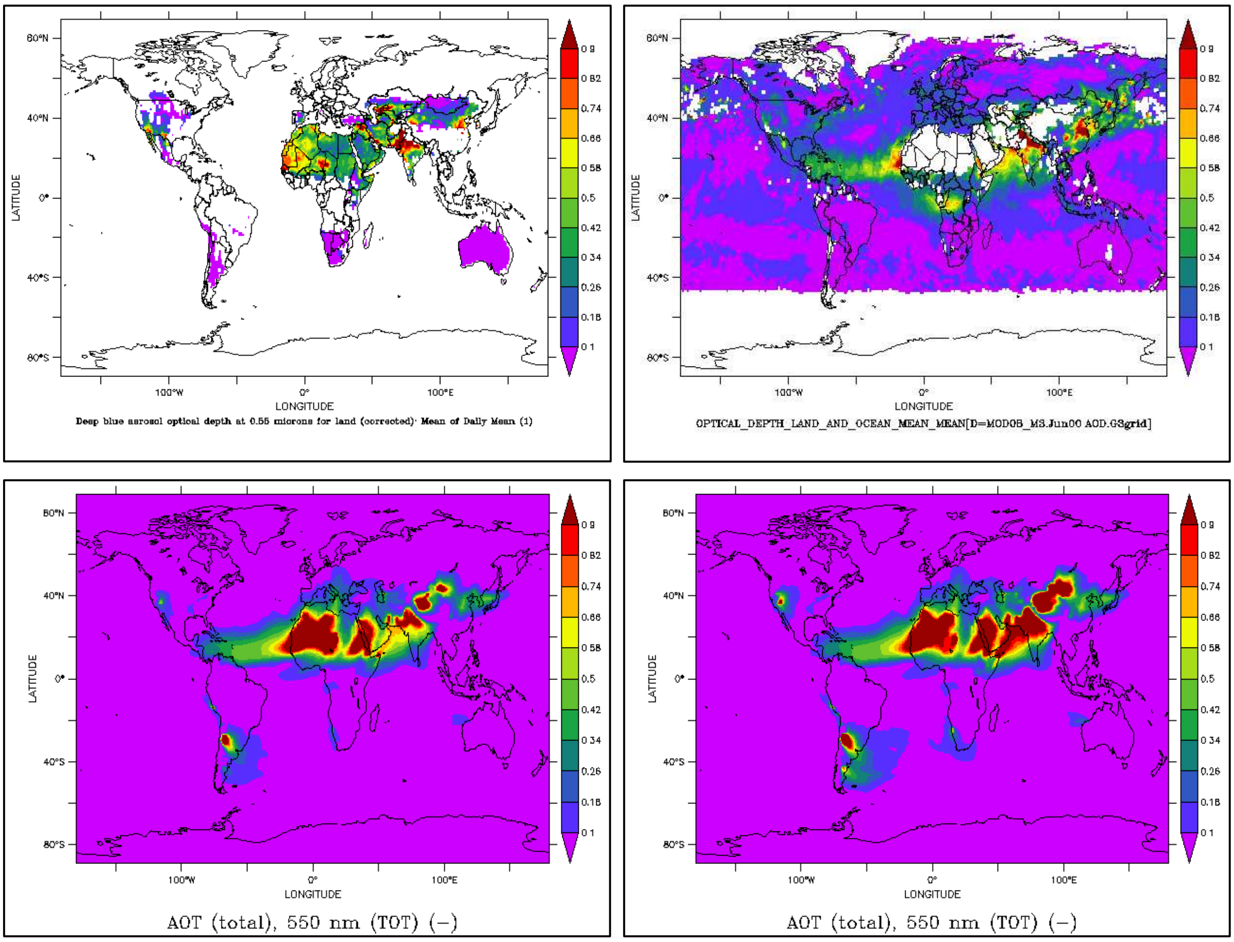

Figure 14 (additional plots): Aerosol Optical Depth at 550nm from the MODIS-Terra (v5.1) satellite (upper panels) and from the EMAC simulation with the DU1_ERA40 formulation (lower left panel) and DU2_ERA40 simulation (lower right panel). The period is June 2000.

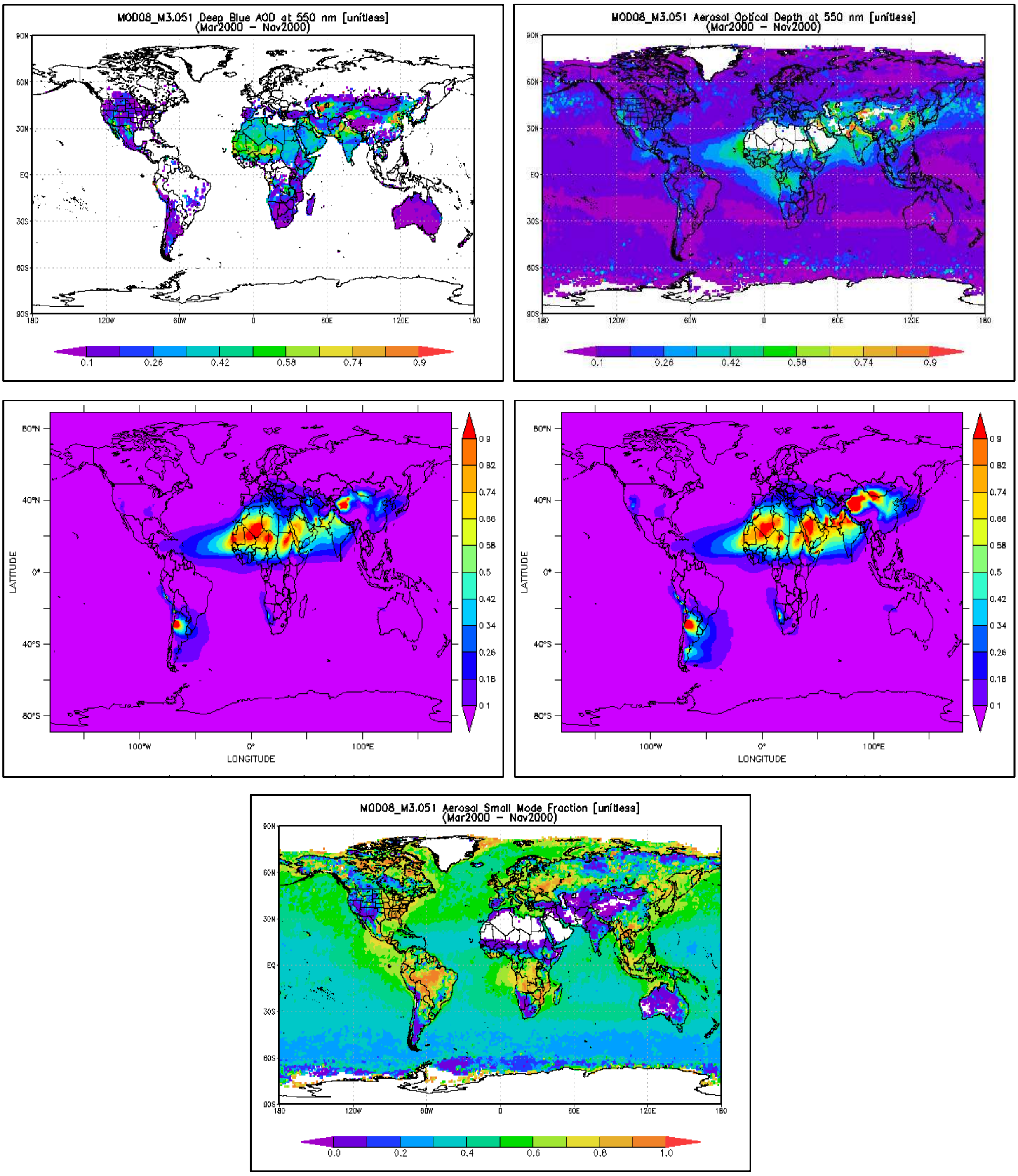

Figure S4: Average Aerosol Optical Depth at 550nm from the MODIS-Terra (v5.1) satellite (upper panels) and from the EMAC simulation with the DU1_ERA40 formulation (middle left panel) and DU2_ERA40 simulation (middle right panel). The lower plot shows the aerosol small mode fraction produced by MODIS-Terra (v5.1) satellite. The period covered is March to November 2000.