

## **Reply to the Interactive comment on “New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC” by M. Astitha et al.**

### **Anonymous Referee #2**

This manuscript describes the dust schemes implemented into the EMAC model and provides evaluations of the model simulated dust concentrations and AOD by comparing with surface measurements and remote sensing data. Significant amount of effort has been put into the work. The manuscript is well organized.

However I have several major issues with the paper that have to be addressed before the manuscript being considered for publication:

[Author reply]: We would like to thank the reviewer for the extensive assessment of our work. The detailed comments gave us the opportunity to address several inconsistencies that needed clarification. We address the comments/questions in detail below. 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.

1. “New parameterization” and “two schemes”: While I appreciate the detailed description of the dust uplifting processes and parameterization, it is difficult to tell which part of the parameterization is “new” that is unique and has not been published in the literature. If there is something new, it should be made more explicit. Also, I really don’t see “two” schemes; DU1 and DU2 are the same with the only difference in particle size distributions (Table 4).

[Author reply]: We acknowledge the reviewer’s objections and we have made the following changes: The title of the manuscript is changed to “Parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC: impact of nudging and soil properties”. The word “New” reflected the new implementation in the EMAC model compared to the previous one and not a new developed parameterization scheme and we removed it to avoid additional misunderstandings. The phrase “two schemes” has been replaced with “two versions of the scheme” which is more appropriate for our work.

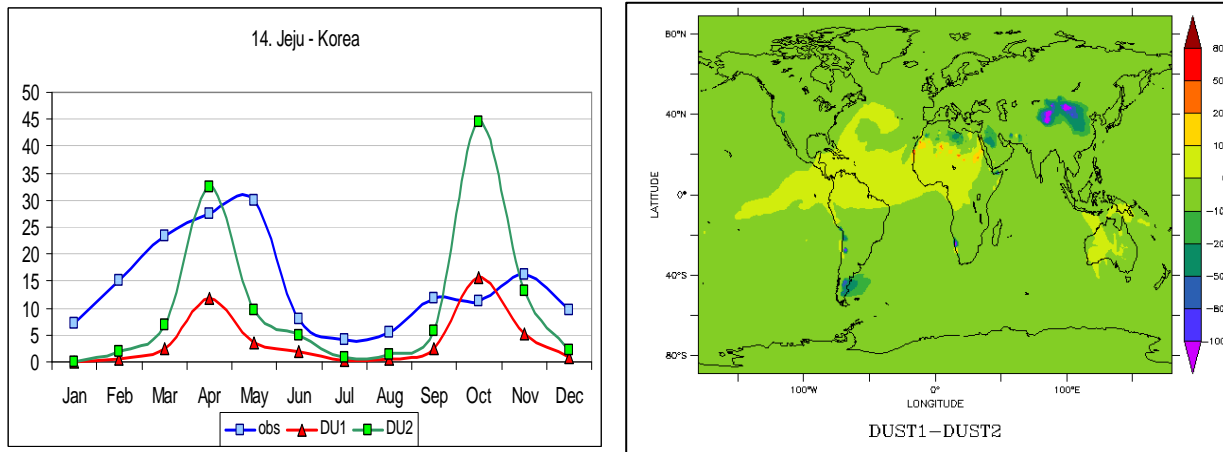
2. “The need to represent arid regions individually and explicitly. . .”: In contrast, after reading this manuscript, I don’t see any need to have Dp that is unique to individual regions. DU1 and DU2 perform very similarly and DU1 is overall better than DU2.

[Author reply]: 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. From that conclusion we presume that either the included soil properties are obsolete or the emission scheme cannot be uniformly applied to all arid areas. With this statement we implied that it might be beneficiary 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, after both reviewers find it contradictory.

3. The emission and atmospheric loading are significantly different between DU1 and DU2 (Table 5 and 6), yet, the concentration, deposition fluxes, and AOD are very similar between the two? How do you explain and reconcile?

[Author reply]: The total budgets presented in Table 5 are calculated for the entire domain on an annual basis whereas the budgets in Table 6 are per region on an annual basis. First of all, the emissions are not significantly different if we look at the regional budgets in Table 6. The emissions from N. Africa for both schemes are similar (e.g. DU1ERA40=528Tg/y, DU2ERA40=460Tg/y) as well as the emissions from N. America and Australia. The differences in the total budget (Table 5) originate from differences in the emissions in the Middle East, Asia and S. America. Also, the calculated fields are not similar for the entire domain. To be more precise, there are stations in the AOD evaluation (Fig.S3) that show significant differences between DU1 and DU2. For example, the AOD between the two versions of the emission scheme in Anmyon, Arica, Bahrain and Solar Village vary significantly for some months, with the biggest difference (almost double) occurring during December in the Arica station (AOD(DU1)=0.52, AOD(DU2)=0.97). These 3 stations are located in S. America (Arica), Asia (Anmyon) and the Middle East (Bahrain, Solar Village), where the emissions between DU1 and DU2 vary significantly (Table 6). In stations located in N. Africa the differences are not that distinctive given the similarity of the emissions. When plotting the monthly AOD against the observations the differences are obvious, but not big enough to significantly shift the regression line in the scatter plot of Fig.12. In the annual AOD (Fig. 13) the differences are more obvious per station.

The same applies to the dust concentration fields. The differences in the monthly dust concentration are important for some locations (as shown for station Jeju below which is close to the Asian deserts), but for others not (as in Miami or Barbados). Furthermore, the plot of the averaged differences in the surface dust concentration for the month of April, clearly shows that the DU1 and DU2 versions of the scheme (units: ug/m<sup>3</sup>) do not give similar concentrations in the entire domain. The differences can reach up to 1000ug/m<sup>3</sup> for the Asian desert for example.



The same pattern is followed for the deposition fluxes (see differences in dust concentration above). The deposition fluxes are not similar in the entire domain but for most of the stations used in the analysis. These stations are located away from the main dust sources areas (Figs 9-10) and the dust plume arrives in those locations with similar intensity, thus the deposition fluxes are found to be similar. Nevertheless, there are few stations (from the 84 in Table S2) that have significantly different deposition fluxes: Asia (DU1=25.9g/m<sup>2</sup>, DU2=64.3g/m<sup>2</sup>), East Pacific (DU1=2.75g/m<sup>2</sup>, DU2=6.47g/m<sup>2</sup>), Indian Ocean (DU1=14.8g/m<sup>2</sup>, DU2=20.6g/m<sup>2</sup>).

We should note here that the deviations between DU1 and DU2 (AOD, deposition, concentration) are caused by the differences in the dust mass because all other species are similarly treated in the two versions of the scheme and the aerosols that contribute to the AOD calculation do not interact with each other.

4. My biggest problem with this manuscript is the model evaluation. There are way too many subjective, descriptive words and phrases assessing the model performance, such as “very well”, “good agreement”, “satisfactory”, etc. What are the standards to warrant such satisfactions? It seems that if the differences between model-simulated and observed values are within a factor of 10 (Figure 6, 7, 9, 12), the model is considered to have “good agreement” with observations. However the differences at some locations are more than 100 times! The evaluation should be as objective as possible and the self-praising, subjective words should be avoided.

[Author reply]: The evaluation procedure followed in this work is based on the methodology followed in the AeroCom project on the modelling of dust (Huneeus et al. 2011; Kinne et al. 2006; Perez et al. 2011). We have used extensive datasets to accomplish the difficult task of evaluating the global distribution of desert dust, in line with the current state-of-the-art model evaluations. The characterizations that the reviewer found inappropriate are rephrased in the revised version of the manuscript. Nevertheless, we have also characterized the evaluation as having a “poor agreement” in several occasions in the manuscript. The differences that the reviewer is invoking are fully explained in the text and are mostly associated with the comparison of the year 2000 simulation with multi-annual observations, where the 1:1 relationship is not expected at all. Detailed answers are given in the ‘Specific comments’ section, where each part of the manuscript is thoroughly assessed by the reviewer.

5. The simulation includes dust, biomass burning, and sea salt, but omits anthropogenic and natural aerosols from volcanoes and terrestrial biosphere. This is not appropriate for comparisons with AERONET or satellite AOD, even at the dust dominated sites unless you screen out the non-dust components. Using  $AE < 1.2$  is very ineffective to exclude small particles.

[Author reply]: The simulations include sulphur dioxide from anthropogenic, biogenic and volcanic sources, which results in aerosol sulphate through the simple sulphur chemistry scheme (Page 13241, line 28). We also include black carbon and organic carbon (from wildfires, bio-fuel, fossil fuel, and secondary species), dimethyl sulphide from terrestrial sources, nitrogen oxides, sea salt and dust (Page 13242-lines 1-6). These emissions provide a substantial amount of aerosols that can accommodate the use of AERONET and satellite AODs (Kinne et al. 2006). We cannot agree that the anthropogenic aerosols can cause important deficiencies to the comparison of modelled versus measured AOD, since we have carefully selected the AERONET stations that are mostly influenced by dust. Also the satellite instruments provide products that can identify the areas where fine or coarse particles dominate (e.g. aerosol small mode fraction from MODIS or AERONET). Furthermore, there is a big number of publications in the literature of models simulating desert dust that do not include any other species. It is a common practice in the community to use the AOD as the most appropriate tool for evaluating the dust simulation results, given the lack of direct dust measurements in a global scale (Nickovic et al. 2001; Tegen et al. 2002; Zender et al. 2003; Heinhold et al. 2007, 2009; Laurent et al. 2006, 2010; Huneeus et al. 2011; Perez et al. 2011; Ridley et al. 2012, among others). Quoting a phrase from Prof. Y. Shao’s review “The combination of satellite and ground based networks, such as GALION and AERONET, provides an extremely powerful tool for monitoring the global dust cycle” (Shao et al. 2011).

For the use of the Angstrom exponent below 1.2 we have followed the work of Kinne et al. (2003): “*For the spectral region of the Sun photometer, the Angstrom parameter is sensitive to size of submicrometer aerosol. Values between 1.5 and 2.0 indicate particles sizes of the ‘accumulation mode’ with a few tenth of a micrometer in size. These aerosol sizes are characteristic for biomass-burning dominated aerosol (July to November at Mongu) and urban influenced aerosol (GSFC) [Eck et al., 1999]. Sites dominated by larger ‘coarse mode’ particles (e.g., dust at Cape Verde) display smaller Angstrom parameters. Values below 0.4*

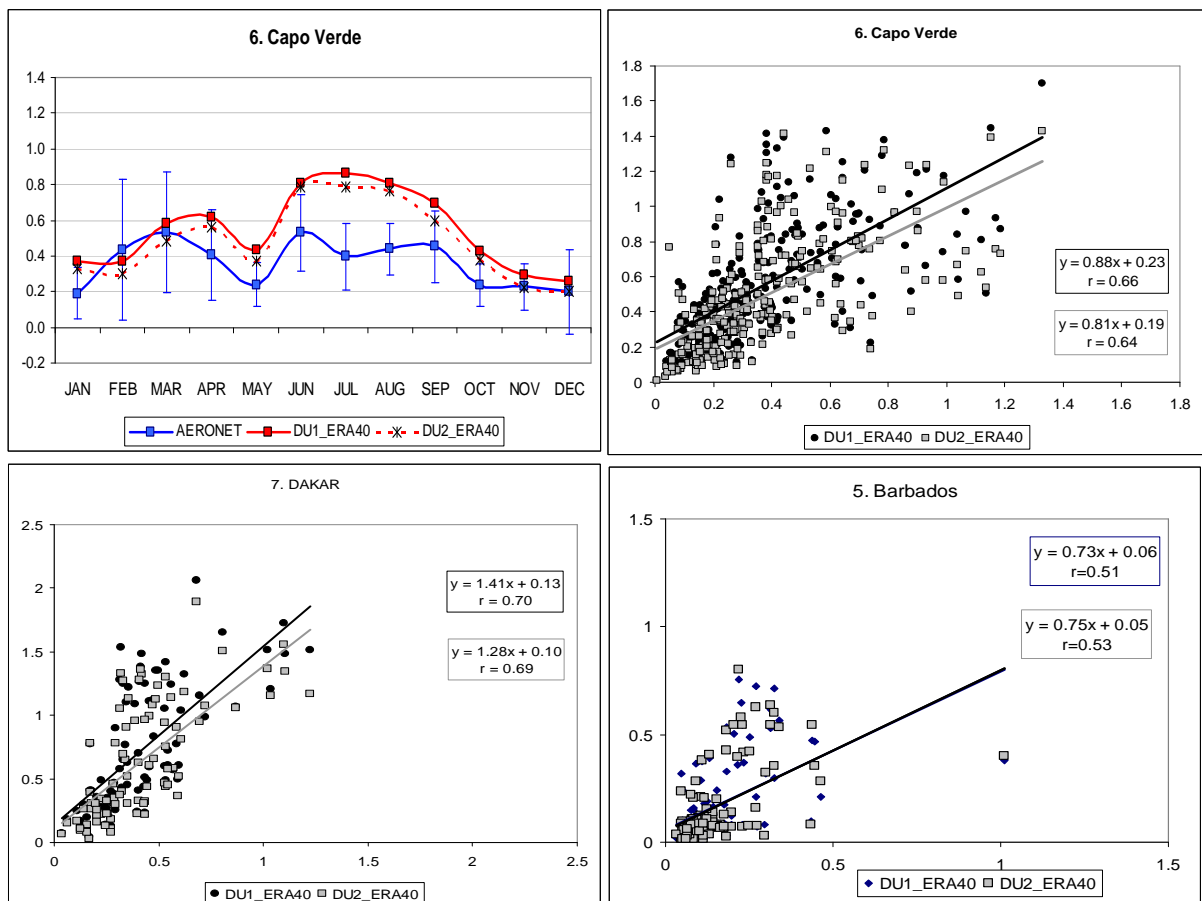
almost resemble the spectrally neutral behavior of clouds (thus are often a cause of mistaken identity in Angstrom based cloud-screens of aerosol retrievals)” Also, Dubovic et al. (2000) analyzed 8 yrs of worldwide distributed data from the AERONET network of ground-based radiometers, categorizing the aerosol absorption and other optical properties in several key locations. For the dust aerosol we quote “...in contrast to biomass burning and urban-industrial aerosol, a (Angstrom parameter) is low (ranges from ~1.2 down to -0.1) and the phase function asymmetry is relatively high at all wavelengths considered”. Based on the above, we believe that  $AE < 1.2$  is appropriate to locate the stations dominated by coarser particles. Furthermore, the  $AE < 1.2$  is applied together with  $AOD > 0.2$  in the attempt to exclude stations that have  $AE$  close to 1 and  $AOD < 0.2$  that would suggest the dominance of fine particles in the measurements. The stations selected are those that at least 20% of the total measured daily AOD comply with the mentioned criteria. We did not imply that this criteria completely excludes the fine particles, but it is a reliable filter for choosing stations that are mostly associated with coarser particles. Nevertheless, if the reviewer would like to suggest a different methodology for the station selection, we will be happy to include it.

Specific comments:

P13238, Abstract, line 13-14: “The dust outflow from Africa over the Atlantic Ocean is accurately simulated. . .” what is the standard of “accurately simulated”? Clearly from the figures the differences can be as large as more than 100 times.

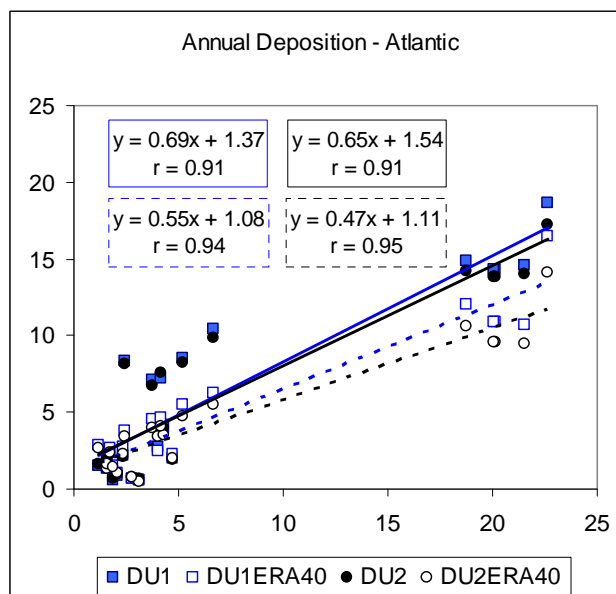
[Author reply]: We are not sure to what differences and figures the reviewer refers to. The dust outflow over the Atlantic Ocean is well described by both schemes and this is supported by the following comparison with observations/measurements in the Atlantic Ocean that is specifically described in the text:

From the comparison with AERONET: Monthly and daily AOD at Cape Verde, daily AOD at Dakar and Barbados (Page 13260, line 3):



From the comparison with measurements from the year 2000: At the Miami and Barbados (Page 13257) the dust modelled concentration (Fig.8) shows that the model captures the seasonality and the magnitude of the measurements, with an overestimation at Barbados during the summer months and an underestimation at Miami during September to November. These differences are not even close to 100 times.

From the comparison with multiannual observations of dust deposition (stations in Fig.3b and discussion in Page 13258): the stations located in the Atlantic Ocean (denoted with the green colour) have shown a correlation close to a 1:1 linear regression (Fig. 8 and also separately below):



From the comparison with multiannual observations of dust concentration (Fig.3a): the stations denoted again with green colour correlate almost with a 1:1 relationship with the modelled values in the annual averages (Fig.6, green squares). The biggest differences are found in the monthly dust concentrations of this dataset (Fig. 7, green squares). Surely, one cannot judge the dust outflow only looking at this comparison and dismiss the previous ones where simulation year and observations coincide.

Finally, in Fig.14, the aerosol mass concentration from the MODIS-Terra satellite and the model results indicate a reasonable approximation of the dust outflow over the Atlantic Ocean, even with this qualitative comparison. We have added AOD plots in this Figure to obtain a more quantitative comparison; it is also evident from these figures that the measured AOD over the Atlantic Ocean is reasonably reproduced by the model.

P13238, Abstract, line 20-22: As I mentioned earlier, your results do not corroborate with this statement.

[Author reply]: We have removed this sentence from the abstract as explained in comment 2 at the beginning of this document.

P13239, line 1: global models are “less dependent on boundary condition” – global models do not dependent on boundary conditions at all.

[Author reply]: This phrase combined boundary and initial conditions to make a distinction between global and regional models on the context of these conditions. Nevertheless, we have rephrased this sentence. General circulation models (as the EMAC model used here) do

however rely on boundary conditions: sea surface temperature, sea ice coverage and greenhouse gases concentrations are examples of such boundary conditions.

P13239, line 3-5: interaction between pollutants and dust is not unique for global models.

[Author reply]: The sentence that the reviewer refers to is: “*Furthermore, anthropogenic influences, including interactions between pollutant gases and aerosols with dust particles can be analysed, and their role in atmospheric chemistry and climate change simulated*”. There is no statement in the text that the interaction between pollutants and dust is unique for global models. We merely state that the development in the EMAC model concerning the dust production/distribution will be useful and appropriate to study these interactions in a global scale. Moreover, the current state-of-the-art in atmospheric chemistry modelling moves towards fully integrated systems (regional and global) that include all natural and anthropogenic pollutants so that the complex interactions among pollutants and the atmospheric conditions can be investigated in detail.

P13239, line 10-12: “In many cases models are tuned. . .” Can you give several examples among the many cases?

[Author reply]: 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). Examples have been included in the text.

P13240, line 5-6: Can you give examples on how regional models are more sophisticated in representing dust emissions? On what regards?

[Author reply]: The height of the saltation layer is of the order of 1 m (Marticorena and Bergametti, 1995), which underscores the small spatial scale of the dust emission process. Moreover, the entrainment of dust particles in the lower atmospheric layers is a highly dynamic procedure that depends on the structure of the lower boundary layer and is affected by the meteorological conditions and the terrain characteristics. Regional or limited-area models are capable of applying high spatial and temporal grid resolutions to resolve these small scale dynamic processes. The high spatial and temporal resolution can account for the fluctuations in these scales, whereas with a global model that uses coarse grid resolution (usually the fine resolution is of the order of  $1^{\circ} \times 1^{\circ}$ ) the fields are smoothed and the details of the underlying terrain (which can be prescribed in 30'' resolution) are lost. Furthermore, the necessary input fields (soil texture and size distribution, terrain, vegetation) can be implemented in these models in a fine horizontal resolution, thus affecting the lower boundary structure and physics and subsequently the emission of dust particles from the lowest atmospheric layer. A brief discussion is included in the text.

P13240, line 10-11: I don't see how this work can overcome the difficulties listed in the previous paragraphs, one is that the regional model is more sophisticated, and another is the difficulty of directly measuring dust emission fluxes in the source areas. Your work does not deal with these at all.

[Author reply]: We have replaced this sentence with a more appropriate one: “and investigate the dependence of the dust distribution on the soil properties and the emitted particle size distribution”.

P13241, line 12-13: If the model covers a complete diurnal cycle in 5 days, how do you get “daily average” from the model and compare with observations?

[Author reply]: As it is stated in the manuscript “the model output is recorded every 5h providing an entire daily cycle (1h resolution) after 5 days”. This means that the fields are provided every 5h (i.e. 0,5,10,15,20,25,.. UTC) and for this work they are recorded as averages of the previous 5hrs. The daily average is calculated from the 5h recorded fields of each specific day (e.g. we have fields saved for 5,10,15,20,25 UTC; the average of these fields gives the daily average for that specific day). We have removed the information on the diurnal cycle from the text since it is misleading.

P13247, line 21: I don't see the use of equation (10) in calculating horizontal fluxes.

[Author reply]: This was a mistake that has been corrected in the text.

P13249, line 6-8: What is the physical justification for the size adjustment?

[Author reply]: The size distribution assigned to the transported dust particles follows the formulation of Perez et al. (2006) also used in Perez et al. (2011) for the NCEP Non-hydrostatic Multiscale Model. The sub-micron particles correspond to the clay-originated aerosol (bins 1–4 with diameters 0.2-2um) and the remaining particles to the silt (bins 5–8 with diameters 2-20um). Particles of this size are considered capable of being transported away from the dust source areas, allowing the model to represent the dust transport patterns in an efficient way. More details are included in the mentioned publications.

P13252, line 4: So the data are from various time periods. How appropriate is this to compare with model with only one-year (2000) simulation?

[Author reply]: The datasets used in the model evaluation are not only multi-annual observations. The datasets (as explained in Pages 13251-13252) consist of dust concentration and AOD (daily, monthly and annual) for the year 2000, dust deposition and concentration from multi-annual observations. The multiannual observations present a first order approximation of the station's climatology (though not in a strict sense) as it is the average of measurements taken for several years. The evaluation of a global simulation is hindered by the lack of available measurements and all available data should be included in such procedure. We followed the methodology of the dust model evaluation presented in Huneus et al. (2011) for the AeroCom project. References to recently published work using the same datasets is provided in Huneus et al. (2011) (also Prospero et al. (2010), Perez et al. (2011) among others). The evaluation of the model with these datasets is discussed with caution in the text, as the comparison reveals if the simulated year can approximate the climatology of the stations.

P13252, line 15-18: I disagree that the anthropogenic fraction of aerosol is not relevant. Not having a full chemistry should not be the reason for not including anthropogenic aerosols. Anthropogenic BC and OC are mostly primary aerosols, and not having a full chemistry scheme is no excuse for not including anthropogenic aerosols.

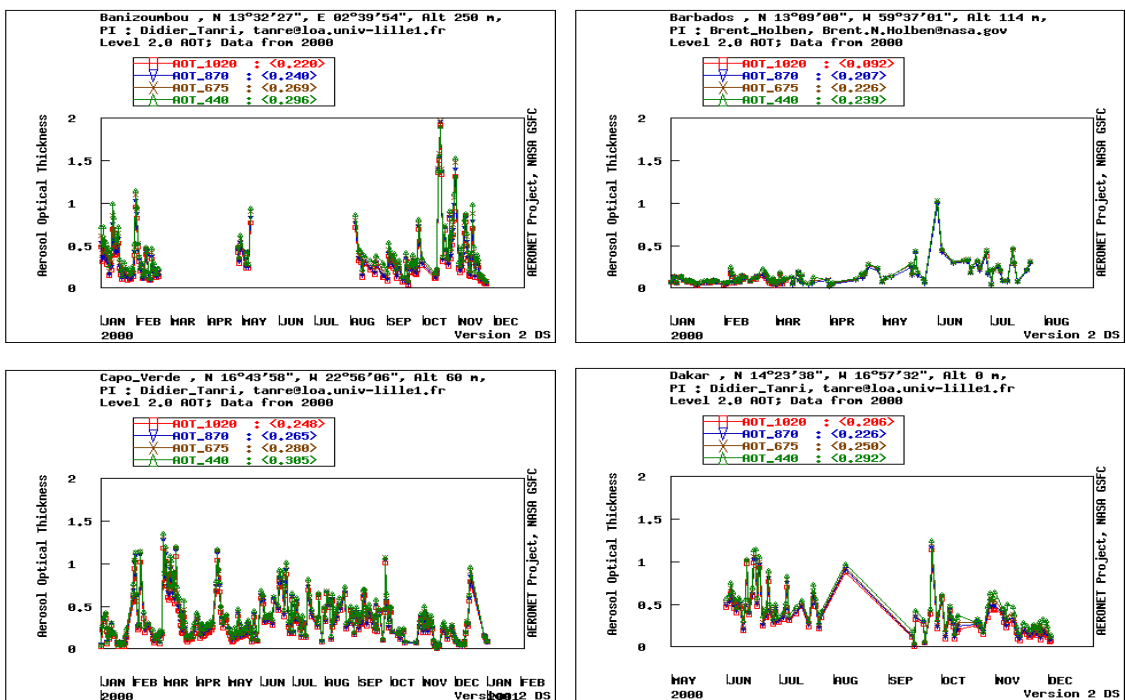
[Author reply]: There must be a misunderstanding concerning the phrase we wrote on the anthropogenic fraction of the modelled AOD. We have added additional information for the methodology of the AOD calculation. The anthropogenic aerosols missing from the

simulation are the secondary species. Black carbon and organic carbon emissions from specific sources are included (biofuels, fossil fuels, wildfires and biomass burning; Pages 13241-13242). These are considered more important in the areas susceptible to desert dust transport, which is the main subject of this work. A sentence has been added to justify this phrase.

P13252, line 19: In fact, 500 nm is a standard wavelength of AOD sun photometer measurements in AERONET. AOD at 500 nm is available at all AERONET sites. “extracted” – you mean “interpolated”.

[Author reply]: This is not true. There are stations that do not include the AOD at 500nm wavelength (for the Level 2 data): Banizoumbou (N. Africa), Barbados, Capo Verde, Dakar, among others (plots from the AERONET website are shown below).

The phrase is changed to: “the measured AOD at 550nm is obtained by an interpolation method...”.



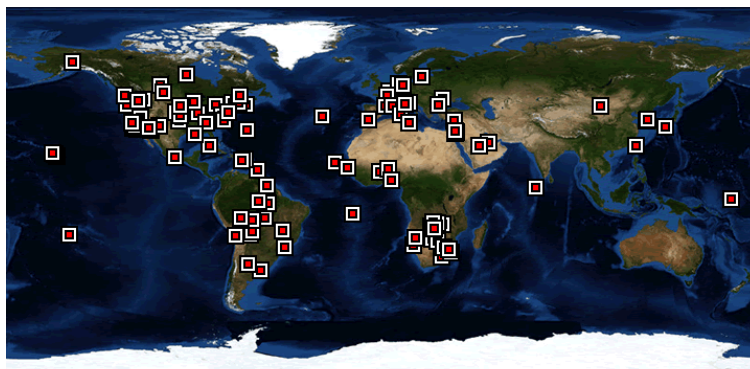
P13252, line 28: AE 500-870 nm – how do you get AOD at 500 nm if it is “rarely given” (line 19)?

[Author reply]: The Angstrom exponent at 500-870nm is directly given by the AERONET database. We do not calculate this parameter.

P13252, last line: As I said earlier, AE<1.2 does not exclude most small particles. It is a poor filter for selecting dust.

[Author reply]: We have replied to this issue in the previous comment by the reviewer (comment 5 at the beginning of the document). We have based our selection method on previously published work and the results of the double criteria imposed to the AERONET data provided 19 stations that are known to be influenced by dust throughout the year (Fig.3d). Note that all the available AERONET stations for the year 2000 are shown below and do not include some sites located in the desert areas.





P13253, line 11-12: “it is not recommended” by whom? What kind of conclusion is a “strong” conclusion?

[Author reply]: It is not recommended to draw strong conclusions based on Level 3 monthly data products, because the sampling of actual retrievals is highly non-uniform in space and time, even at the resolution of these products (MODIS  $1^\circ \times 1^\circ$  and MISR  $0.5^\circ \times 0.5^\circ$ ) (Kahn et al., 2009; Leptoukh G., 2011; Dr A. de Meij personal communication with Dr. R. Kahn and Dr. G. Leptoukh, 2010). L3 data does not include information about the sampling of the satellites and the spatial resolution is too coarse to accurately represent the locations of for instance the AERONET stations. A discussion on the quality issues of the Level 3 products is found from Dr G. Leptoukh’s talk at EGU (<http://ntrs.nasa.gov/search.jsp?R=20110015255>). With this recommendation we wanted to note that a comparison between the satellite Level 3 products and AERONET data should be done in caution, given the significant differences between the two sampling methods. This is why the MODIS and MISR AOD retrievals in comparison with the model results are placed in the Supplement, to be complementary to the AERONET data mostly on a qualitative way. Additionally, the work of Levy et al. (2009) on the quality of the derivation of monthly AOD from satellite data shows the need to use these products with caution since they are dependent on the choices made for data aggregation and weighting, that can lead to variations up to 30%. The sentence has been rephrased accordingly.

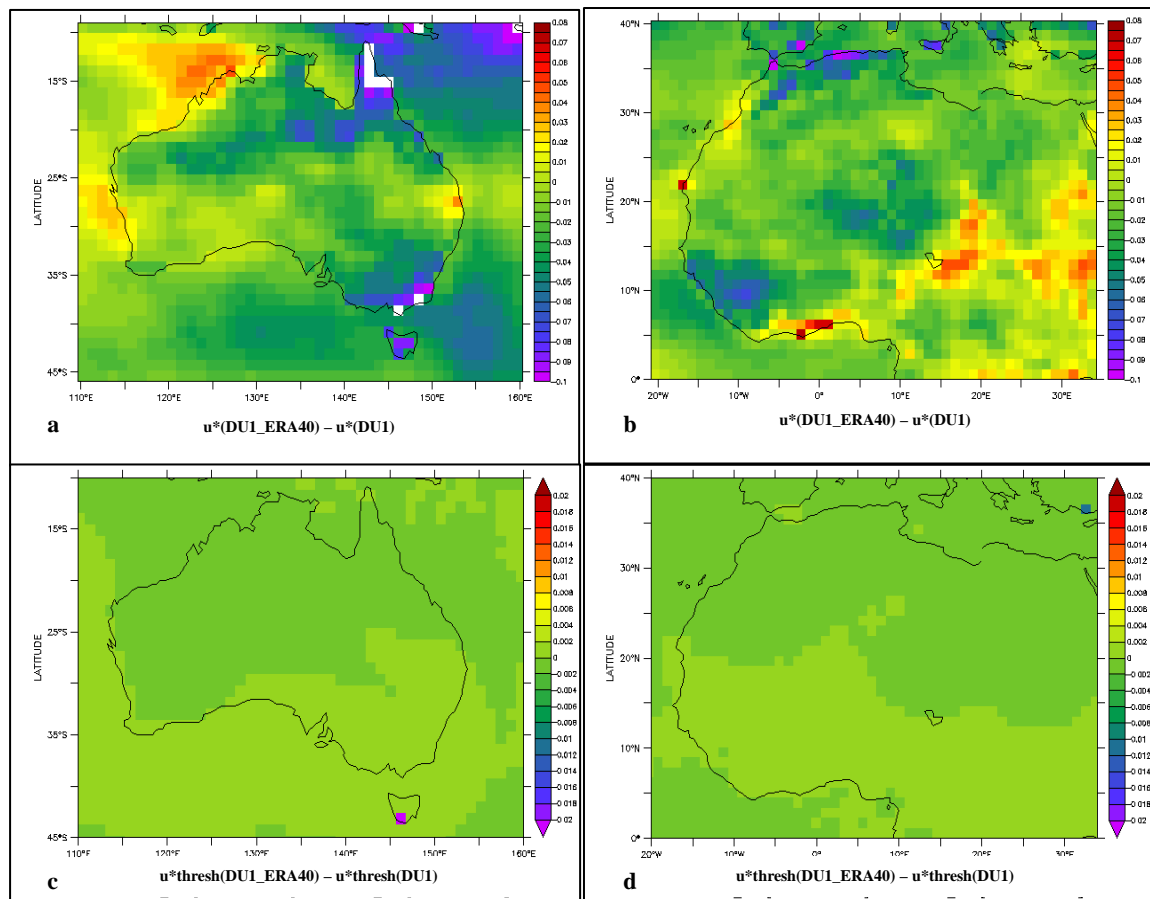
P13253, The last paragraph: If you plot a diff map (absolute or relative difference) the emission difference will become apparent at different geographic regions.

[Author reply]: We already include such plots in the Supplement (Figs S1 and S2) and also mentioned in the text (Page 13254, lines 18-19 and Page 13265 lines 10-11).

P13254, line 6-7: Why can’t you be sure about the changes of wind speed? Can you simply directly compare the wind speed from free running GCM with the ERA40? How different is the soil moisture between GCM and ERA40?

[Author reply]: The nudging reduces the wind speed in several locations and increases it in others. Overall there is a reduction of the wind speed in the dust source areas as we have seen by comparing the nudged with the free-running simulations. We rephrased the word “indicating” as this has caused the reviewer’s comment.

The differences between nudged and free running simulations concerning the friction velocity (which is the variable used in the dust emission scheme) is shown in the upper left plot (a) below for Australia and in the upper right plot (b) for N. Africa (annual average difference:  $DU1\_ERA40-DU1$ ). The soil moisture influence on the dust emissions is not straightforward, because it participates as a correction to the threshold friction velocity (Eq.7, p.13247). The changes in the threshold friction velocity are very small as can be seen in the lower panels below (c and d).

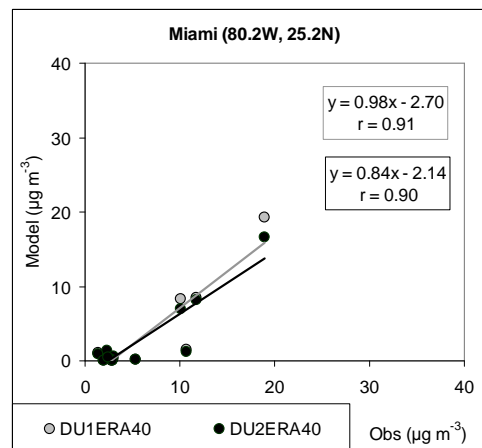
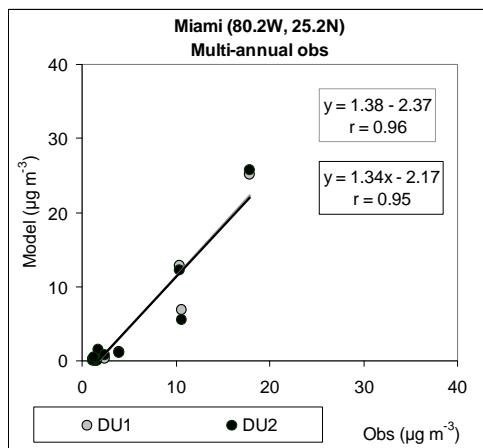


P13254, line 17: “probably because the wind speeds. . .” – again, please check the wind speed to be sure, not just guess the differences.

[Author reply]: We have checked the winds before making that conclusion and it is our mistake that the word “probably” indicated that we were guessing. This has been corrected accordingly.

P13254, line 25: How well is “well”? The model just captures the peaks but severely underestimate dust concentrations from Sept to May. The concentration from the model is basically zero.

[Author reply]: The phrase the reviewer refers to is: “The lower panel shows the multi-year mean simulation, indicating that the seasonal cycle is captured well by the free running model...”. The lower panel in Fig.5 shows a very good correlation between the model and the multiannual measurements also supported by the correlation coefficient and the regression lines, which are both close to 1 (left plot shown below). We have also added the standard deviation of the measurements to show that the modelled values are within the variability of the observations. We believe that the “well” in line 25 is fully justified by these values. The underestimation that the reviewer describes exists in the comparison for the year 2000 (Fig.5 upper plot) where the seasonality is clearly captured by the model and months underestimated are September-November. During these months the transatlantic dust transport from N. Africa is weak and these concentrations are mostly associated with local sources. The correlation is very high between model and observations (right plot below) even with these underestimated values. The performance of the model in reproducing the seasonality of the observations is considered successful, keeping in mind that this is a global model using a resolution of  $1^{\circ} \times 1^{\circ}$  and a perfect match with the in-situ observations could not be anticipated.



P13254, line 26 and P13255, line 1: What statistics?

[Author reply]: We have rephrased these sentences giving quantitative information on the comparison.

P13255, line 10 and 11: “good agreement”, “quite close” – avoid using these judgmental, subjective phrases. Be quantitative.

[Author reply]: Amended.

P13255, line 16: “Cheju” has been changed to “Jeju” since more than 10 years ago.

[Author reply]: We apologize for this inconsistency, but there was no way of knowing the change of the station’s name. The station’s data was provided by Prof. J. Prospero and Dr N. Huneus and it appears with this name also in recent publications (Huneus et al. 2011). We have corrected this in the text.

P13255, line 18: I cannot tell which one is no. 12 in Fig. 3a.

[Author reply]: We have replaced Fig.3a with a new one that corrects these problems.

P13255, line 23-24: So it is “good” within a factor of 10??

[Author reply]: After the reviewer’s suggestion, we have replaced these phrases with more appropriate characterizations. Nevertheless, these measurements cover different periods (mostly during the 80s and 90s) where the simulation year is not included and the uncertainty of the particle size distribution (that plays important role in the transport and deposition processes) is not known. Therefore, we believe that the results from this comparison (Fig. 6 and 7) should be considered reasonable and acceptable. The stations severely underestimated by the model are located in specific areas (i.e. Pacific Ocean) and the reasons for such underestimation are discussed in the text (Page 13256, lines 1-7).

P13256, line 4-5: Now you are saying that your model is just the same as “many global models”. However in several places you seem to distinguish this model from many global models. “have difficulties” – in terms of what? Models are too high? Too low?

[Author reply]: We state in the manuscript that the model does not seem to capture the annual average concentrations below  $1\mu\text{g}/\text{m}^3$  in these regions (Pacific Ocean stations) and this result coincides with the behavior of other global models as discussed in detail in Huneus et al. (2011). This concerns the comparison with the specific dataset. We have not attempted to compare our model with other global models anywhere in the text, as this would require a

totally different approach and information. A detailed discussion on the difficulties of each of the 15 global models to capture these low concentrations can be found in Huneus et al. (2011).

P13256, line 10-11: How does the bias calculated? Are they absolute bias? You show high bias (positive) of the model simulations, but from the Figure it is clear the model has an overall low (negative) bias.

[Author reply]: The statistical analysis is based on the mean bias:  $MB = \frac{\sum(C_m - C_o)}{N}$ . The

3-4 overestimated concentrations give the positive bias, even though most of the points show underestimation that would result in a negative bias. We can provide the statistical analysis, if the reviewer wishes to.

P13256, line 18-19: “correlate rather well” – how well? Even if they are “well” correlated, the slope is far away from 1:1, and a lot of these green points are outside of 1:10 line with difference up to 100x!

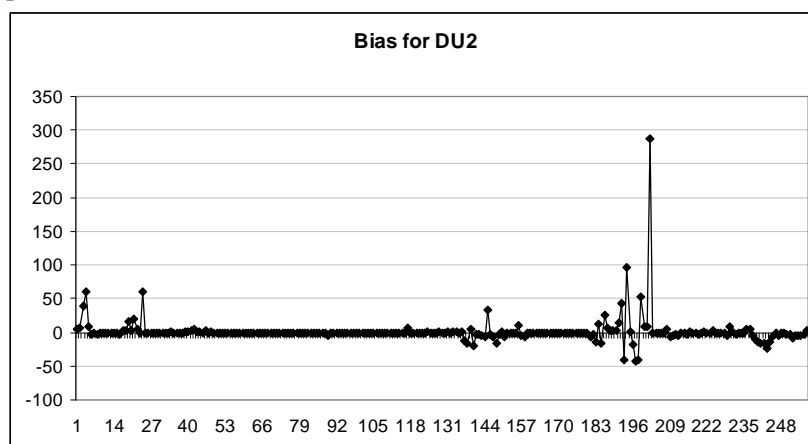
[Author reply]: The continuation of this phrase, commented by the reviewer, includes the detailed discussion of the model underestimations per station and per month. We do not rely on the characterization “well” but we try to explain different aspects of the model performance. Again we should note that this part of the evaluation involves multiannual observations against model results for 2000. Nevertheless, we have rephrased every characterization “well”, “good” etc throughout the manuscript with more quantified information, as requested by the reviewer.

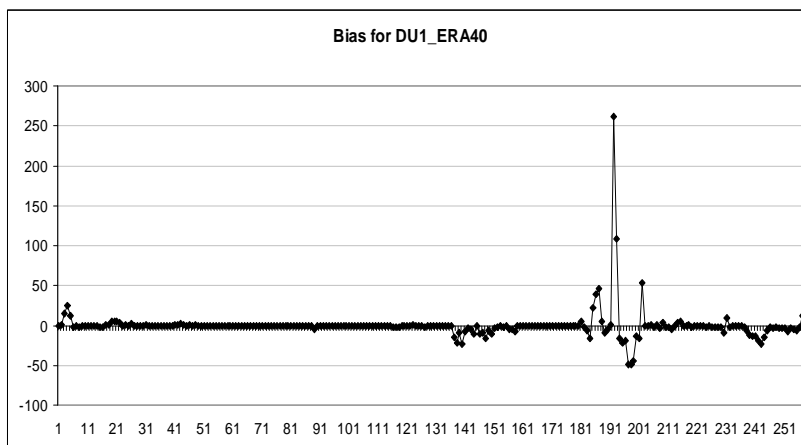
P13256, line 23-24: Similarly, “agree well” should not be used. I am puzzled about this level of satisfaction given the fact that many of the model points are a factor of 10 too low, and some are as low as a factor 300!!

[Author reply]: Again, we need to say that this comparison is between data of different time periods and a deviation of the modelled values is expected, especially since the year 2000 is not included in the time periods covered by the observations. Nevertheless, these characterizations are removed from the text.

P13256, line 28: I don’t understand how come they all have positive bias (except DU1 ERA40) - the model is definitely much lower than the observations.

[Author reply]: As explained earlier, in every statistical analysis the calculated bias is the mean bias. To give an example for the DU2 (the mean bias is 1.42ug/m3) and the DU1\_ERA40 (the mean bias is -0.18ug/m3), we have plotted the bias of each model-observation pair:

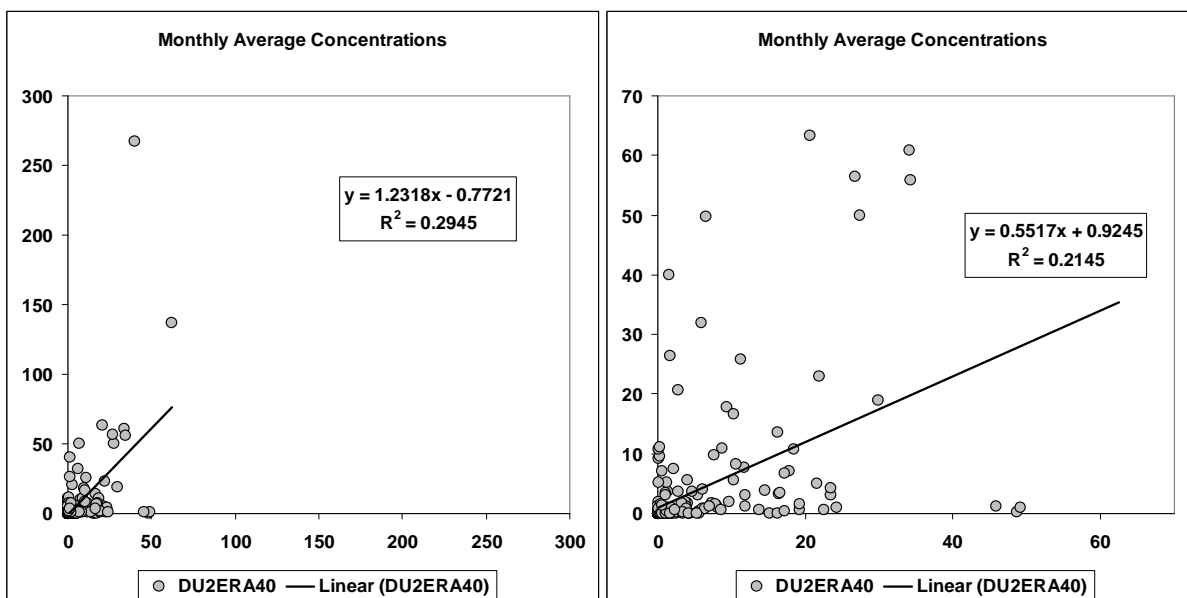




The underestimation mostly occurs in very small values, whereas the overestimation is in higher concentrations (the spike in the upper plot is for obs=6.62 versus model=292). This is why the final mean bias is a positive value in most of the cases. In DU1\_ERA40 there are less positive spikes that give an overall small negative mean bias.

P13257, line 1, and Figure 7+Table 8: I just don't see how it is possible to get "a liner regression close to one". It is so obvious from Figure 7 that there are large fraction of points deviated far away from 1:1, the slope should be much higher than 1, and the intercept should be negative in all simulations.

[Author reply]: The phrase "linear regression close to 1" is removed from the text. The linear scatter plot of the mentioned statistics is shown below on the left. If we discard the two outliers (model overestimations) then the plot looks like the one on the right. The intercept is negative in the nudged and positive in the free-running simulations due to the changes in the concentrations which are not discernible in Fig.7. The scatter of the points is evident when looking at the high standard deviation for each simulation in Table 8, which is more than twice the standard deviation of the measurements.



P13257, line 3-5: "This may indicate. . ." This sentence sounds like a guess. Does the comparison of dust deposition support this statement? Regarding the solubility, can you do sensitivity experiments to reduce the solubility to see the effects?

[Author reply]: The deposition measurements (84 stations, Fig.3b) are annual averages at different locations compared to the measured concentrations and for different time periods. This means that we can not conclude on the monthly average concentration by looking at the annual average deposition. There are a lot of sensitivity experiments we can do with the model, one of which can be to test the solubility of the particles. The problem lies on what we can do with the results of such experiment. Since no measured data exists, we would have to speculate on the quality of the results, which is beyond the scope of the present work.

P13257, line 6-10, Prospero datasets and AERONET data: They all cover multiple years, not just for year 2000. In fact, Prospero's data cover 15-20 years and AERONET more than 10 years.

[Author reply]: The datasets for Miami and Barbados for 2000 were given to us by Prof. Prospero and they are different from the multiannual measurements at the same stations (they cover mostly the 80s and 90s). Since AERONET could be used for the year 2000 together with the Prospero data and measurements from Haifa (Prof. B. Herut) for 2000, we decided to simulate this particular year.

P13257, line 12: "satisfactory though not ideal": What does it take to make you satisfied? This is very subjective statement.

[Author reply]: Fig.8 shows clearly the performance of the model in capturing basic patterns of the dust transport in remote areas. As with all modelling exercises, some features are captured and some are lost, resulting in the "not ideal" comment.

P13257, line 19-22: ". . .may be related to the meteorological conditions" – can you pull out the met data to be sure, instead of guessing?

[Author reply]: The high dust concentration in Tel Shikmona for the two days of April 2000 is a result of the model underestimation of the precipitation rate in this area. TRMM data shows daily precipitation rate 3.4mm/day for April 11 and 0.02mm/day for April 12, whereas the model precipitation is almost zero. This is the main reason of the overestimated dust concentrations as the wind speed produced by the model is similar to the observed wind speed at the WMO station in Haifa. All the details of this comparison are included in the manuscript.

P13258, line 7: "very well" – again, how well is very well?

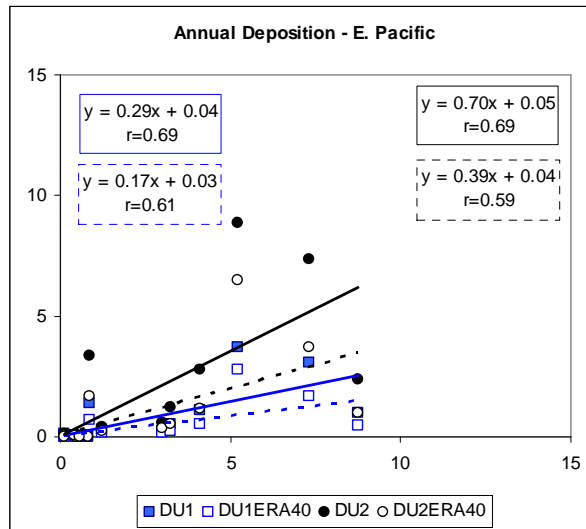
[Author reply]: These characterizations have been replaced in the text.

P13258, line 9: Is a factor of 10x difference considered as "accurate"?

[Author reply]: The answer to this comment is the same with the one in the beginning of the "Specific Comments" section. We provided a plot of the deposition from the stations in the Atlantic Ocean where there is no difference 10x as the reviewer suggests. Of the 22 points, only 2 are largely underestimated by the model, being approximately 5 times smaller compared to the observations. The reviewer must agree that in a global simulation such result can be considered as accurate.

P13258, line 11-12: Can you quantify "substantially improves the simulation"? It is difficult to tell from the Figure how DU2 has substantially improved the simulation.

[Author reply]: The improvement is substantial compared to the other simulations and it is evident in the scatter plot shown below. A comment has been added in the text to make it also obvious to the readers.



P13258, line 12-13: It is not just “overestimated at some locations”. It is overestimated by a factor of 100 at those locations.

[Author reply]: This is true for some points. We added this information in the text.

P13258, line 15: “Reasonably well” means within a factor of 10!

[Author reply]: We have replaced this characterization in the text.

P13258, line 15-17, sentence starts with “Only for one location. . .”: It is Hard to see from the Figure. There are many stations that the model systematically either over or underestimates.

[Author reply]: This has been replaced by the following: “In two locations, the Taklimakan desert in central Asia (purple) and Lake Kinneret in Israel (blue), the dust deposition is systematically underestimated by the model, with no significant changes among the simulations. The underestimation of some E. Pacific stations (red) is also persistent in all simulations, with small deviations among them”.

P13259, line 8: “satisfactory” - Another subjective words. You have to tell us your standard of being satisfied. Within a factor of 100?

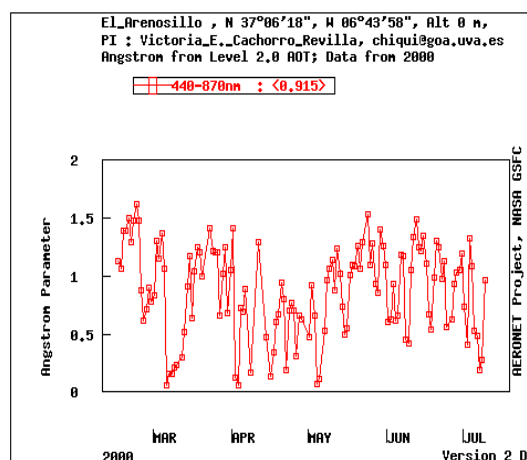
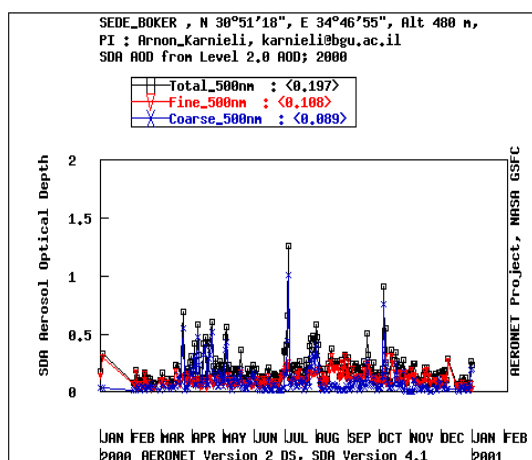
[Author reply]: We have replaced this characterization in the text: “Since we are comparing different model and measurement periods, we cannot expect quantitative agreement, and we conclude that the model performs in an acceptable way for these locations”.

P13259, line 11: Many of the sites are not dominated by dust. Sites in the Mediterranean Sea have significant amount of pollution, so has Jeju.

[Author reply]: The 6 Mediterranean sites (Oristano, El Arenonsillo, Lampedusa, Erdemli, Nes Ziona and Sede Boker) are influenced by both dust and anthropogenic pollution. They are considered susceptible to severe desert dust intrusions and as dust transport is of episodic nature, we did not want to include only stations at the heart of the desert areas. This allows us to investigate the ability of the EMAC model to represent the correct timing of the dust intrusions over this area. Furthermore, the Angstrom exponent by AERONET shows that coarse particles exist for a number of days at stations like El Arenonsillo. The Fine/Coarse AOD at Sede Boker also shows the existence of coarse particles for several days. These

AERONET stations were selected because at least 20% of the daily AOD values satisfied the  $AOD > 0.2$  and  $AE < 1.2$ .

Jeju is not part of the AERONET stations used for the AOD comparison.



P13259, line 14: How do you get the daily average from the model if it takes 5 days to complete a diurnal cycle?

[Author reply]: This has been explained in detail in a previous comment.

P13259, line 20: “all days for the month” – how about the days when no data are available?

[Author reply]: The L3 monthly data from MODIS and MISR are obtained from the Giovanni online data system (<http://disc.sci.gsfc.nasa.gov/giovanni/overview/index.html>) where no information is given on the specific days included in the monthly average. We compared monthly AOD from the satellites with monthly AOD from the model including all days of the month. This has also been explained in the reply to the question [P13253, line 11-12].

P13259, line 23: “reasonably good agreement” – avoid using such subjective phrase.

[Author reply]: Amended.

P13260, line 7-9: Does it mean that the EMAC physics is not quite right to represent the Bodele dust source? Why is there a risk using a physical representation if you have confidence in EMAC physics?

[Author reply]: The physics of the model is not related to the decision not to use a-priori definition of dust sources in areas like the Bodele region. The Bodele depression is a classic example of a paleolake preferential dust source which is an area of little or no surface relief consisting of fine-grained lacustrine sediments deposited by paleolake Chad in the early to mid-Holocene (Tegen et al. 2002). The vegetation (ecosystem biomes) and soil texture maps do not include these paleolake beds, thus an implicit inclusion of them in a global model is not possible. Researchers have used explicit ways of introducing these areas in the models by a-priori defining their location.

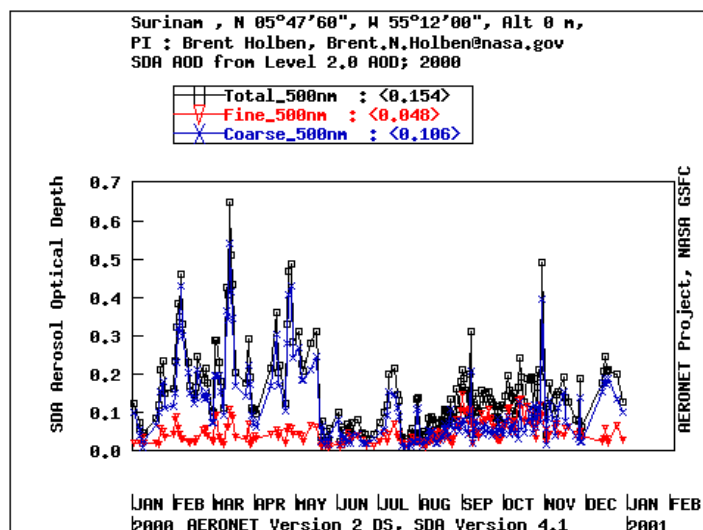
P13260, line 12: “well”; line 18 – “well”; line 21, “mediocre”, and line 23, “good”: too many subjective words!

[Author reply]: Amended.



P13260, line 26-17: Why do you think that the modeled dust is right but other aerosols are wrong? You should show some evidence, e.g., by comparing the AE and absorbing AOD.

[Author reply]: We agree with the reviewer and we have added a discussion about that in the text. The fine/coarse AOD from AERONET showed that during January to May and July the coarse AOD is dominating the total AOD. During these months the model values are close to the observed ones (Fig. S3, station 19). The discrepancies are more enhanced during June, and September to December where the fine and coarse AOD almost equally contribute to the total AOD.



P13260, paragraph from line 111 to 21: This paragraph is too descriptive, not quantitative. No one can tell which station is which from the figures.

[Author reply]: We have added more quantitative information on the evaluation in this paragraph and we have also enlarged the numbers in Fig.3d to make the stations more visible to the readers. The discussion on the daily AOD evaluation is also related to the monthly plots per station in the Supplement (Fig. S3). We have added this information to the text. The numbers in the parenthesis correspond also to Table 10 with the statistical analysis for each station.

P13260, last paragraph that ends in next page: There are no figures or numbers to look at about these evaluations. Can you supply figures to let the readers know what you are talking about?

[Author reply]: The discussion in this paragraph is related to Table 10 (statistics for each station's daily AOD), Fig.11 and additionally Fig.S3. We have added this information in the text.

P13261, line 26: another "well"!

[Author reply]: Rephrased.

P13262, line 2: another "good agreement"!

[Author reply]: Rephrased.

P13262, paragraph starts from line 10: MODIS aerosol mass burden? This is not a MODIS product. Where do you get this product?

[Author reply]: This product was taken from the Giovanni online platform (<http://disc.sci.gsfc.nasa.gov/giovanni/overview/index.html>), at the MODIS-Terra satellite section denoted as “Mass Concentration (QA-weighted)”. Quoting the information from the website “MODIS provides columnar aerosol mass concentration over land, and over ocean”. A reference to the website is also provided in the text.

P13262, same paragraph as above, and Figure 14: Are you showing dust mass burden from the model, but MODIS “mass burden” of all aerosols?

[Author reply]: The inclusion of these plots is meant for a qualitative discussion on the main aerosol patterns shown, focused on the main desert sources. New plots with total AOD from the model and from MODIS have been added (after the suggestion from reviewer 1) that accommodate the quantitative discussion on the transport patterns.

P13262, line 20: Why is there no data over N Africa if MODIS Deep Blue is used?

[Author reply]: This paragraph refers to the mass concentration from the MODIS-Terra satellite and the platform does not provide mass concentration from the Deep Blue algorithm.

P13262, line22: another “well represented”!

[Author reply]: Amended.

P13263, line 2: Would you please provide evidences and examples of “many models apply regionally tuned emission fluxes”?

[Author reply]: The answer to this question is included in a previous comment by the reviewer (Comment: P13239, line 10-12). In addition, we refer to the work of Cakmur et al. (2006) and Miller et al. (2006) where they used separate tuning factors for clay and silt emissions to minimize the error between model and observations. These references have been added to the text. We have also changed the word “many models” with “some models”.

P13263, line 24-25: I don’t understand this claim. AOD of any values, not just in the range of 0.4 to 1.2, can be a mixture of coarse and fine particles.

[Author reply]: This is an error in the text. Instead of AOD it should have been AE (Angstrom exponent). This is discussed in Huneus et al. (2011) with references therein.

P13263, last line and P13264, first two lines: Have you evaluated the modeled sea spray aerosols, or it is just a guess?

[Author reply]: The sea salt distribution has been evaluated in previous publications (Kerweg 2005, Kerweg et al. 2006, Pozzer et al. 2012). The exact contribution from dust and sea salt to the total AOD in the AERONET stations can not be directly evaluated since both are found mostly in the coarse mode. However, we have seen that in some places the dust AOD from the model contributes more to the total AOD than in other stations. We can qualitatively presume if the dust or the sea salt is responsible for over or underprediction of the total AOD in locations and months that the coarse particles are dominant.

P13265, line 2-6: Does it mean that you should not concentrate on comparing DU1 and DU2 over N Africa, Middle East, and their downwind regions but focus more on the regions where DU1 and DU2 emissions have large differences? However 15 out of the 19 AERONET sites are located in N Africa, Middle East, and downwinds.

[Author reply]: We do not make such statement in the manuscript. We only present the results found from the evaluation of the different simulations that show smaller discrepancies in N. Africa and Middle East and larger in Asia and S. America. The AERONET stations selected were the ones available globally for 2000 that satisfied the imposed criteria.

Lastly, I don't see MISR is used anywhere in the manuscript. It is only in the Supplement material. You should either include MISR in the main manuscript, or move MISR related material to supplement.

[Author reply]: We have added that the MODIS, MISR and Deep Blue comparison associated with the AERONET stations is included in the Supplement as it is complementary to the model evaluation using the AERONET data.

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