

Interactive comment on “Global dust model intercomparison in AeroCom phase I” by N. Huneeus et al.

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We greatly appreciate the referee’s comments and the extent to which the referee made suggestions to improve the manuscript. They improved the quality of the paper and helped to render it more accessible to the reader. Furthermore, we greatly appreciate the comments pointing out errors in the data processing. The main modifications in the paper are: 1.- Normalized statistics were introduced. The differences model-observations and model-model were quantified based on these normalized statistics. 2.- The hovmollers of the individual models illustrating the seasonal cycle for the different variables were moved to the supplement. Instead, the hovmoller of the observation together with the hovmoller of the bias, the centred pattern root mean square errors (the root mean square error with the bias extracted) and the standard deviation are

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shown. The goal of this approach is to identify whether the general differences with respect to the observations are due to the bias or the centred pattern root mean square error. The standard deviation is included to illustrate the spread of the models. 3.- The compilation of fraction of wet deposition (Mahowald et al. 2009) was included in the study to expand the analysis on the model performance to simulate wet deposition. 4.- The surface concentration measurements from Midway Island, important in the analysis of long-range transport of Asian dust, were included and the text was adapted accordingly. 5.- Errors on data processing were corrected. These errors were the computation of the deposition fluxes of model ECMWF, the preparation of figure 1 and the pre-processing of model data of the year 2000. The missing sedimentation of CAM was included and the mass balance corrected. In what follows the answer to each comment can be found. 6.- The same numbering is used for AERONET stations for the year 2000 and climatology throughout the text. 7.- A paragraph in the General Discussion (section 4.4) was dedicated to the AeroCom median model and its general performance compared to the other models.

General comments:

The findings of the paper are summarized in the abstract. I will go through each of the finding including my comments and questions about the paper in general.

R: 1. "In general, models perform better in simulating climatology of vertically averaged integrated parameters (AOD and AE) in dusty sites than they do with total deposition and surface concentration." I understand the arguments of the authors in the paper to reach this conclusion but I feel that this point should be at least reformulated. AOD and AE values in this comparison span 1 order of magnitude while measurements of surface concentrations and deposition can span up to 3-4 orders of magnitude. Indeed AOD is used for stations with high influence of mineral dust, while deposition and surface concentration includes all types of stations. The statement above and the related discussion in the paper should be reformulated. I also strongly recommend including additional statistics in the paper when comparing to deposition and surface concen-

tration. RMS and Bias definitely overweight the regions with higher concentrations. Please include normalized statistics as well. A: Normalized bias and RMS have been included in the scatter plots as suggested. Statement above has been changed in the abstract to “In general, models produce smaller normalized mean bias and root mean square errors for the climatology of vertically averaged integrated parameters (AOD and AE) in dusty sites than they do for total deposition and surface concentration.”. Corresponding changes were done in the discussions and conclusion of the article. The total deposition and surface concentration measurements were conducted in stations dedicated and conceived to study the dust cycle (e.g. AEROCE and SEAREX).

R: 2. "Almost all models overestimate deposition fluxes over Europe, the Indian Ocean, the Atlantic Ocean and ice core data." In the paper there is a full discussion on the observed data quality issues. I believe that the limitations of the data should be somehow outlined also in the abstract since, 1) some measurement periods vary according to the site, 2) a 3.5% iron content is assumed to infer dust deposition fluxes from iron deposition, 3) some stations from Tegen et al (2002) may only include 50 days of data (1/7 of the total days in a year). In this sense, can you provide in the paper some estimates of the uncertainty of the different data used? A: The statement highlighted by the referee was removed from the abstract and a mention to the impact of the data uncertainty was introduced. To our knowledge error estimates for most of the data used here are not available except for Aeronet AOD. We have added the Aeronet error estimate. The referee is right in suggesting that the limitations of data and their quality should be more investigated.

R: One additional comment here: it is difficult for me reconcile the deposition estimates from table 2 and the comparison in Figure 1. For example ECMWF model simulates almost 6000 Tg/yr of dust deposition, about a factor of five more than the AEROCOM median, while in Figure 1 ECMWF has the lowest positive bias. Is there any error or the implication is that the current network of dust deposition is not representative at all of the global dust deposition? The latter is highly improbable since most of the stations

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lie within the main dust transport pathways. Please clarify this point and address the implications accordingly. A: Indeed, an error existed in both, the way data for Figure 1 were prepared and in the computation of the deposition of ECMWF. Figure 1 and Table 3 were corrected and the text was changed accordingly. We greatly appreciate the referee for pointing this out.

R: 3. "Differences among the models arise when simulating deposition at remote sites with low fluxes over the Pacific and the Southern Atlantic Ocean." A: We did not understand the referee with respect to this remark.

R: 4. "This study also highlights important differences in models ability to reproduce the deposition flux over Antarctica. The cause of this discrepancy could not be identified but different dust regimes at each site and issues with data quality should be considered." I don't understand the last statement. The cause of discrepancy among models is independent from the observed data quality. Please consider revising the sentence. A: The statement was removed from the abstract.

R: 5. "Models generally simulate better surface concentration at stations downwind of the main sources than at remote ones." Ok. I have just one comment concerning the surface observations. Is there any additional information provided by the comparison with cruise data (Figure 4)? I don't think it is a helpful approach to consider the cruise data as a yearly average. A: This conclusion is mainly based on the long-term measurement from the SEAREX and AEROCE programs. The inclusion of the short-term surface concentration helps to provide data on remote regions such as the oceans and especially the southern ocean with scarce data. An additional purpose to include this data in the study is to illustrate that models in remote regions in the southern hemisphere show the same performance in simulating this data than they do with long-term data and that therefore this data should be exploited for model validation in this region.

R: 6. "Likewise, they simulate better surface concentration at stations affected by Saharan dust than at stations affected by Asian dust." Dust modelers highly tune the

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emission scheme, and most of the community is biased towards tuning the model with Saharan dust observations. I strongly encourage that you include in the paper a discussion about this aspect and comment how the emission scheme was tuned in the different models (for example in table 1). A: In the general discussion (section 4.4) a reference to the tuning of the emission was made. We compare in this paper the different models to multiple datasets and also compare them to each other. It is not within the scope of the paper to enter into all model detail to explain the observed differences. This conclusion has changed in the current version. The new hovmoller figures used in the present version suggest that in general the models perform better in simulating the surface concentration at stations affected by Asian dust than at those affected by Saharan dust, contrary to what was stated in the submitted version.

R: 7. "Most models simulate the gradient in AOD and AE between the different dusty regions, however the seasonality and magnitude of both variables is better simulated at African stations than Middle East ones." Don't you think this can partly be explained by the influence of anthropogenic aerosols in the Middle East? Even if only months dominated by coarse dust aerosols or the mixture of coarse and fine aerosols are analyzed, shouldn't the anthropogenic contribution affect the AE? How can you then assure (page 23804 - line 16-18) that higher AE indicates smaller dust particles in the Middle East than in Africa? The fact that Ilorin and Djougou in Africa (highly affected by biomass burning but nevertheless are not filtered by your selection criteria) show higher AE contradicts your argument. A: Indeed the stations of Iloring and Djougou in Africa present an averaged AE equivalent to the one observed in the Middle East and were therefore excluded from the analysis pointed out above (pg 23804-line 16). Yet the averaged AE in the remaining selected dusty stations using the climatology and with the year 2000 (where Ilorin and Djougou have been filtered out) show AE smaller than the Middle East stations. Since a higher AE corresponds to a dominance of smaller particles in the atmospheric column we conclude that smaller particles are present. As correctly pointed out by the referee this larger AE could also be explained by the influence of anthropogenic aerosols. The text has been modified to include this.

R: 8. "The models also reproduce the dust transport across the Atlantic in terms of both AOD and AE; they simulate the offshore transport of West Africa throughout the year and limit the transport across the Atlantic to the summer months, yet overestimating the AOD and transporting too fine particles. However, most of the models do not reproduce the southward displacement of the dust cloud during the winter responsible of the transport of dust into South America." Ok. Is there any clue if this is due to emissions or transport/deposition? Can't this be checked with the results from the different models? I understand that for this exercise, the models were nudged towards (or run with) an Atmospheric Reanalysis. Since the southward displacement in winter is not reproduced by most of the models, can this be due to problems in the reanalysis? Have you checked whether if the models that reproduce the displacement use different atmospheric reanalysis?

A: As stated above, it is not within the scope of the paper to enter into detail of the model configuration to explain the observed differences. However, Table 1 describing the configuration of each model was expanded to include the winds chosen to constrain the model. As can be seen most models use reanalysis winds of the year 2000. We don't think that any of the models is reproducing the southward displacement to the extent visible in the observations.

R: 9. "Based on the dependency of AOD on aerosol burden and size distribution we use model data bias with respect to AOD and AE and infer on the over/under estimation of the dust emissions. According to this we suggest the emissions in the Sahara be between 792 and 2271 Tg/yr and the one in the Middle East between 212 and 329 Tg/yr." This is a very original and interesting aspect of the paper. However, taking into account the different characteristics and representation of the size distribution in the models I would be cautious on the emitted mass estimates or at least this should be clearly highlighted in the paper and the abstract. A: The bias was recomputed for Africa and the Middle East and the section discussing the emissions was changed according to the new results. In addition, this section was also changed in order to

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include the influence not only of the size distribution but also of other factors such as mass extinction efficiency. A more careful statement was phrased with respect to the plausible emission range that can be derived from joint analysis of AOD and AE.

Other comments:

R: Page 23784 L23-24. There is some speculation about the role of dust on meningitis epidemics in the Sahel. However, meningitis (bacterial) is not a vector-borne disease, it is spread person to person. Please reformulate. A: Text has been changed and now reads "Links between the occurrence of meningitis epidemics in Africa and dust have been suggested".

R: I suggest including an additional figure that shows the global distribution of wet and dry deposition, surface concentration and AOD for AEROCOM median. I would include the same maps for each model in the supplement. I think this could be useful for the reader and would complement table 3. A: We treat all models equally, including figures of the AEROCOM median model and not from the other ones would highlight it with respect to the other models. It could lead to conclude that the AEROCOM median model is used as a reference, which is not the intention of this study. Furthermore, we believe that the inclusion of these additional figures does not compensate for the large increase in figures. These figures among others can be found in the AEROCOM website and this is noted in the text.

R: Table 1: I understand that most of the information about the models is available in Textor et al 2006. However I would recommend including the specific information on the dust module in this table apart from the references. I am referring to whether the emissions are interactive or not, and how the model was tuned. Additionally it would be good for the reader to know which of the models are CTM's and GCM. . . . How were they run (offline, nudged??) and what data (climatology,analysis, reanalysis, NCEP, ECMWF?) A: We agree that additional model information would allow the reader to have a clearer idea of each individual model however we believe that the information

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given is enough for the comprehension of the paper and references are given if more information is required. However, we included in Table 1 the winds chosen to constrain each the model as suggested by the reviewer.

R: Table 3: Please revise. There are some inconsistencies. For example ECMWF model 514 Tg/year of emission and 5999 Tg/year of deposition. A: The inconsistencies of model ECMWF have been corrected.

R: Table 4: Please note the different size distribution characteristics of the models.. A: Table 4 has been reorganized in order to group models with equivalent size distribution.

R: Figures 1 and 5: Please include additional normalized scores as commented above. A: Changed as suggested.

R: Figure 4: As commented above, is this figure providing additional information? Can the cruise data be taken as a yearly average? A: The cruise data give valuable information in regions of the world where scarce data or no data at all are available (e.g. southern ocean, south Atlantic) and where the dust deposition might have an impact on the biogeochemical cycle. One can notice from Fig. 4 that models have the same performance on reproducing cruise observations from the Southern Ocean and South Atlantic than on long-term measurements sites. It is in regions affected by large dust sources where models present a larger disagreement with cruise observations. We consider that these data can be exploited for the evaluation of model performance to reproduce dust deposition in remote regions and we therefore decide to keep the data in the study. The paragraph considering these data has been modified to strengthen this point.

R: Figure 7: Please remake the left plot so one can see the upper part. A: Model UIO_CTM largely overestimates the surface concentration in Barbados ($\sim 460 \mu\text{g}/\text{m}^3$); including the full range of values in the figure would prevent to see the model diversity around the observations. We include in the text a sentence stating the magnitude of the overestimation. We include the figure as suggested by the referee to illustrate the

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advantage in limiting the range of the Y-axis (Fig. 1).

R: Page 23798- L4: Here it should be noted that many models do not include the Kalahari desert as a source. I believe that the different modeler co-authors could comment on that. A: All models indicate to have emissions in the South Africa (Table 5), region corresponding to the Kalahari Desert according to the definition we give to this region (Fig. 2).

R: Page 23800-L14: Hawaii station is #14 A: The text has been modified according to the new hovmollers in Fig. 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23781, 2010.

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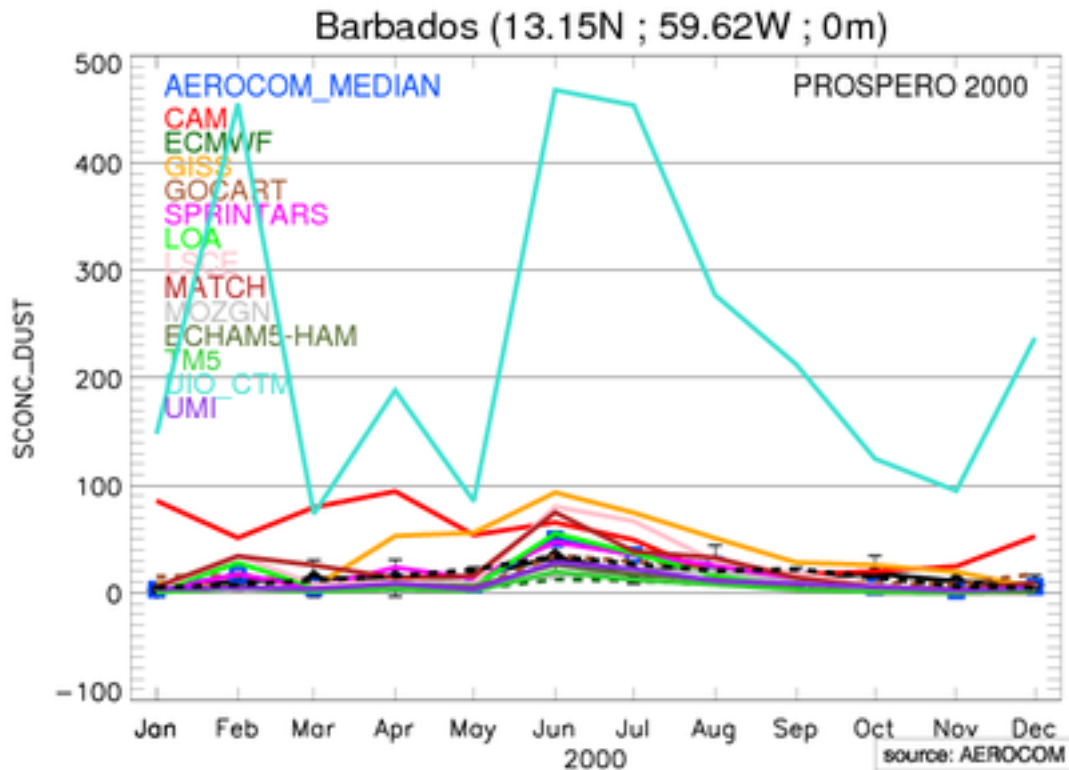


Fig. 1. Surface concentration for the year 2000 at Barbados

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