Atmos. Chem. Phys. Discuss., 10, C15089–C15108, 2011
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Atmospheric Chemistry and Physics Discussions

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

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Received and published: 27 April 2011

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. 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 shown. The goal of this approach is to identify whether the general differences with respect to the obser-

vations 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.

Main Comments

Reviewer: In Figures 3 and 7, the data as presented is clear and the maps in Figure 1a and Figures 2, 8 and 9 are very useful to understand the location of the individual sites. However, Figures 1, 4, 5, 6, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19 contain too much information and I fear that the reader will not be able to take away the important aspects. In each of these Figures there are plots for the AEROCOM median model followed by a succession of individual plots for each of the 10-15 models that submitted data for that diagnostic. In the Hovmoller plots there are additionally an extra plot for the observations. While this approach is admirable, and very complete – I think the manuscript would benefit from, moving the individual model plots to the Supplementary material and making each of these plots just present the results from the AEROCOM median model. Also, in the text for the results (sections 3.1, 3.2. 3.3, 3.4), the text should begin with an assessment of the skill of the AEROCOM median model and then, the general patterns for the individual model results that are highlighted in the current text are set

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into an overall context. With this approach, the reader has a chance of following the Figures whilst reading the text – at the moment I found it very difficult to keep track of the discussion in the text. Having only the AEROCOM median model in the Figures but having the individual plots available in the Supplementary material would allow detailed inspection of the performance of the individual models without detracting from the overall presentation of the results of the average behaviour of the models. Moving the individual plots to the Supplementary Material would force the reader to concentrate on the performance of the AEROCOM-median-model against the observations. The authors have reached several general conclusions about specific deficiencies of some sub-sets of the models – and this is useful so should be kept in the text. But I believe it will greatly improve the manuscript if, in each case, the main analysis begins with the skill of the AEROCOM median model. This way, particular deficiencies that are common to all (or most) of the models can be drawn out first from the inter-comparison. I recommend that, in the Hovmoller plots (Figures 6, 11, 13, 14, 15, 17, 19) in the main part of the paper, the authors, in addition to the observations and the AEROCOM median model, show the seasonal cycle for an AEROCOM multi-model normalised geometric standard deviation plot that illustrates the normalised variance among the models – this would summarise the variance of the models about the AEROCOM median and indicate where the model deficiencies in the median tended to be a symptom of all the models – or whether a substantial number of models did better/worse than the median.

Answer: We greatly appreciate this comment and the referee's effort to make the paper more accessible to the reader. Starting the model intercomparison by describing the performance of the AEROCOM MEDIAN to reproduce the results would change the scope of the article. It would no longer be an intercomparison paper but one where the AEROCOM median model is validated against observations and used as reference for the remaining models. The main focus of the paper is to compare all models to observations and that's why each section with results discusses first the observations and then the general model behaviour to reproduce the observations. Therefore we

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decided to leave the scatter plots as they are but consider the recommendation for the hovmoller figures which represent half of the figures mentioned (Fig. 6, 10, 12, 13, 15 and 17). For the latter we do as suggested by the referee and move the individual model figures to the supplement material. We decide to present in text the hovmoller corresponding to the observations and one for the bias, one for the centred pattern root mean square error (CPRMS) and one for the standard deviation. These figures allow to explore whether the general differences with respect to the observations is due to the general bias or the CPRMS. In addition the model spread to reproduce the observation is illustrated through the standard deviation. A description on how these figures are constructed has been added in the text. We believe this gives in a concise and comprehensible way a general evaluation of the model performance.

R: Another particular issue that needs addressing in the manuscript before publication in ACP is related to the statistical measures that have been written out on each of the scatter-plots are RMS error, mean bias, correlation coefficient and standard deviation. The RMS error and mean bias are absolute measures of bias and are not very meaningful where the quantity being assessed varies over several orders of magnitude as for the dust deposition and mass concentration. For instance a poor performance at low and moderate burden sites could be masked by general good performance at high burden sites. To address this, I recommend that the authors add the normalized mean bias to the list of and also some measure of normalized mean error in addition to the RMS error. By doing so, one could then compare, for example, how well the AEROCOM median model performed at simulating the different metrics.

A: We include normalized statistics as suggested by the referee. However, we use the mean normalized bias instead of the normalized mean bias as suggested (see definition in the section 2.4). The former (MNB) reduces the impact of the extremes by normalizing before averaging whereas in the latter (NMB) the mean bias is normalized by the average of the observations. We believe that this approach is in the spirit of the referee's comment. The same criteria is applied to the root mean square error and we

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normalize by the observation before averaging.

R: The discussion in section 4 reads much better than section 3 and the discussion of the role of different processes in the models is important. There should however, also be some discussion with reference to the role of model tuning here in increasing emissions to account for missing (possibly associated with sub-grid-scale processes) – could tuning the models to the Saharan dust plume be consistent with the general biases seen in the various AEROCOM models? Is there a role here for combining global models with regional models to better parameterize processes leading to emissions at the sub-grid-scale?

A: A reference to the tuning of the emission was included in the general discussion (section 4.4). The new hovmoller figures used in the present version suggest that 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 and contrary to the general belief that "... dust emissions in global models are generally tuned to fit observations in a given part of the world and often this tuning is done with observations from North Africa." And they should therefore have a better performance with the Saharan dust than the Asian one.

There are four main conclusions from the paper:

R: 1) The models perform better at simulating the integrated properties AOD and AE than they do with total deposition and surface concentration. The 1st conclusion is an interesting one and is founded on the finding that AOD and AE are generally within a factor of 2 of the observations whereas the surface concentration and total deposition are often biased by more than a factor 10. The authors should be careful here because the total deposition and surface concentration vary over several orders of magnitude whereas the AOD and AE varying only over 1 order of magnitude. So this finding is open to question here I feel. Perhaps the authors might consider changing this conclusion slightly so that it refers to the AEROCOM median model – it is clear from

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the Figures that the median model simulates quite well the surface mass concentration at U. Miami sites (Figure 5) and AOD (Figures 10 and 12) but systematically over-estimates the total dust deposition (Figure 1).

A: We have reformulated the conclusions according to the suggestion and based it on the new normalized metrics. Note that the systematic over-estimate of deposition is less obvious after correction of the figures as suggested by reviewer 2.

R: 2) Models generally simulate better surface concentration at stations downwind of the main sources than they do at remote ones. And do better at sites mainly affected by Saharan dust than those mainly affected by Asian dust. I consider this result to be very interesting and I wonder whether it could also be related to the 2nd conclusion. The global models run at coarse resolution and hence are missing emissions from processes at scales finer than the grid-scale. Also, we know that the dust emission parameters in the models are frequently tuned until they get the AOD in the main outflow from the Sahara approximately correct. Such tuning of the emissions upward will make the models compare better to the AOD and surface mass concentrations observations downwind of the key source regions but might be leading to the over-estimation of the dust deposition simulated by the models If the authors agree, then perhaps the revised manuscript could comment on this in the discussion and the conclusions be modified accordingly (and also the Abstract).

A: This conclusion does not hold anymore. The new hovmoller figures used in the present version suggest that the models actually perform better in simulating surface concentration at stations affected by the Asian dust than at those affected by the Saharan dust, contrary to what was stated in the submitted version. However, we agree with the referee that the tuning of the emissions is an important aspect in simulating the dust cycle, but we believe that this is not enough to explain the model performance. Processes such as transport and deposition are also important when explaining the dominant features mentioned above. While the models simulate better the dust downwind of the key source regions, they underestimate the observations (surface concentration)

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in remote regions of the southern oceans. For the same reason (impact of transport and deposition on dust cycle) we have limited the analysis of the over/under estimation of the emissions based on the bias in AOD and AE to stations close to source regions

R: 3) Using the model-observation bias with respect to AOD and AE, the authors infer an emissions range that is narrower than shown by the models – i.e. an inference is drawn that some models are under- or over-estimating emissions which is biasing their simulations. The 3rd conclusion is a novel outcome from identifying models with a bias in AOD and AE and inferring that those models must have a problem with emissions – and use this to provide a narrower range of emission simulated from the regions. This is an nice way of using the observations to better constrain the emissions fluxes – but I think it may in this case be compromised by the fact that the emissions fluxes quoted are actually over a different size range (some go up to 20 microns, others only to 8 microns – this is likely introducing a bias into the inter-comparison of the models – so I would be careful about making this kind of statement – the authors should at least provide a caveat about the different size-range for the emissions fluxes from the different models.

A: The entire section discussing the emissions has been changed in order to include the influence not only of the size distribution but also of other factors such as mass extinction efficiency. We agree that the missing description of the very coarse aerosol fraction in most models makes the upper end of the plausible range of emissions uncertain. A more careful statement was phrased with respect to the plausible emission range that can be derived from joint analysis of AOD and AE.

4) From the current comparison one may conclude, with very large uncertainty, that the impact of dust on the ocean biogeochemical cycle is overestimated in most models. The 4th conclusion is very important – and yet it is not included in the Abstract – suggest that it should be added as a last point to the Abstract.

A: After revision of the deposition with the corrected fluxes the conclusion does not

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hold anymore. A reduced number of data exist in the Southern oceans and these data are both over- and under-estimated by the models preventing from concluding on a predominant model performance.

Specific Comments

Abstract:

R: i) Suggest to remove 1st sentence “Desert dust plays an important role. . ..” from abstract as this is more Introduction and not needed in the Abstract which is rather long already at present.

A: Changed as suggested

R: ii) Suggest to move 2nd sentence “A large degree of diversity exists. . .” to before the sentence beginning “In general, models perform better. . .”

A: Changed as suggested

R: iii) Suggest to make the 1st sentence be the “We present the results of. . .” but change slightly at start to that it goes something like “This study presents the results of. . ..”

A: First sentence is now “This study presents the results of a broad intercomparison of a total of 15 global aerosol models within the AeroCom project.”

R: iv) Need to re-word part of the sentence that says “. . .focusing on variables responsible for the uncertainties in estimating the direct radiative effect and the dust impact on the biogeochemical cycle”. When you say that these variables are “responsible” for the uncertainties it sounds like you mean that they cause the uncertainty – rather I think you mean something else here – perhaps that they are variables which need to be well simulated for the two specific effects you refer to – need better wording here.

A: The sentence was changed to “Each model is compared to observations related to desert dust aerosols, their direct radiative effect, and their impact on the biogeochemi-

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cal cycle, i.e., aerosol optical depth (AOD) and dust deposition".

R: v) Suggest to extend the sentence ending "assessment of model performance" to also say something like ". . . and to identify common biases present in most of the models"

A: Changed as suggested.

R: vi) Reword sentence that currently reads "These datasets form a benchmark dataset which is proposed for model inspection and future dust model developments" to something like "We use these observational datasets as a benchmark for the evaluation of the AEROCOM model simulated global dust distribution."

A: As stated above we intend to evaluate all models in the AEROCOM project with respect to the observations considered in the study and not the AEROCOM model only. The observational dataset gathered in the study goes beyond this study and could be used to evaluate the performance to simulate the global dust cycle of any global model, either being created or developed.

R: vii) Suggest to reword sentence "Differences among models arise. . ." – here arise is the wrong word – perhaps you mean that all models are biased in this respect. Again I think the emphasis should be in identifiying common model deficiencies and that the skill of the AEROCOM median model could be used to identify systematic biases in the model – see my comments about the 1st and 2nd conclusions above in the Main Comments section.

A: Sentence was removed from the abstract

R: viii) Suggest to remove or at least the sentences "This study also highlights important differences in models ability to . . ... deposition flux over Antarctica" and sentence after "The cause of this discrepancy could not be identified. . ." Again – I think you are pointing to a systematic bias among the models which is that they tend to over-estimate the deposition at remote regions.

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A: Sentences were removed from the abstract.

R: ix) Add sentence something like “From the current comparison one may conclude, with very large uncertainty, that the impact of dust on the ocean biogeochemical cycle is overestimated in most models.”

A: The correction made in the data processing, in particular in the deposition data changed the result considerably. The conclusion, as stated in the original draft, does not hold anymore. At present the scarce deposition data do not allow to conclude whether the global models analysed in this work predominantly over or underestimate the data. The corresponding sentences in the abstract are now “Large uncertainties still exist with respect to the deposition fluxes in the southern oceans. Further measurements and model studies are necessary to assess the general model performance to reproduce dust deposition in ocean regions sensible to iron contributions.

R: 2.1 Dust deposition – pg23787, line 13 – the Ginoux et al (2001) paper presents dust deposition observations from 16 sites but here only 10 are used (Table S1) – the observations at Shemya, Nauru, Samoa, Rarotonga, New Caledonia and Norfolk Island seem to have been omitted. Obsevations from several of these sites are included in the dust mass concentration observations (Table S2) but are not included in the total dust deposition. Was there a reason why these were left out?

A: The dust deposition compilation in Ginoux et al. (2001) is actually a combination of measurements and model outputs. We use only those data corresponding to actual measurements. In the text we mention we state that the dataset is “based partly upon measurements”. The sentence “Only those data corresponding to actual measurements were considered.” was added in the text to clarify this and an additional sentence has been added in the caption of Table S1 in the supplement material .

R: 2.4 Aerocom dust results – pg23795, lines 8-9 – the range of emissions given is 500-4400Tg/yr but the simulated size-range of dust particles over which this mass is distributed is different among the different models For instance from Table 1 some mod-

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els simulate particles up to 25 microns whereas other consider only up to 8 microns. Consequently, the comparison of the emissions fluxes between the models is not a fair one because the models with a larger upper size limit will necessarily have a higher emission flux. This also affects the calculated lifetime and global total deposition fluxes. For instance, assuming a flat size-distribution of the emissions flux across the 5-25 micron size range, the dust mass emission for particles in the range 5-15 microns would be about a factor lower than that in the 15-25 micron size range. The authors should add a caveat noting that some of the inter-model differences could be explained by different size range of the emitted dust. Note however that the deposition fluxes calculated at the site location should be consistent because the largest particles will fall out close to the source.

A: The statement concerning the range of the dust emissions is made in section 2.4 and is only made with the purpose of giving a general presentation of the AEROCOM models. A note on different size distributions used was added in section 4.3. To highlight the different size distributions in each model we have restructured Table 3 and 4 grouping the models with similar size distribution as suggested by the referee elsewhere.

R: 3.1 Dust deposition results – pg 23795, lines 25-26 and pg 23796 lines 1-2 – this is an example of where the presentation of the model results could be improved – the text says “All the models in this study mostly overestimate largely. . ..” But it would be better here to illustrate this point by referring to the performance of the AEROCOM median model and how from Figure 1 it is clearly biased high -perhaps on average by a factor of 10 – this also illustrates how the statistics printed in the Figures are not very useful in interpreting the performance of the individual models in relation to the performance of the AEROCOM median model – see my specific point above about adding the normalized mean bias and normalized mean error (which would then give values that could be referred to when the skill of the AEROCOM median model in simulating the different quantities is assessed). I suggest also to delete the sentence

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"The level of overestimation varies largely from model to model" and instead give some solid numbers for range of the normalized mean bias in the models.

A: As suggested by the referee we have changed the above mentioned sentences and quantified them using the mean normalized bias, the sentence is now "All the models in this study present a positive mean normalized bias (MNB) in the deposition fluxes ranging from 0.1 to 140.3. However, if the model CAM is not considered the maximum MNB decreases to 13.4". The sentence "The level of overestimation varies largely from model to model" has been removed. We want to point out that we have chosen to keep the individual model results and not focus only in the AEROCOM median for the same reasons already given before.

R: 3.1 Dust deposition results – pg23796, lines 1-14 – there are many instances where statements like "Almost all models underestimate the, ". . .the degree of under-/overestimation varies largely from model to model". These findings need to be made more quantitative by adding reference to the normalized mean bias (nmb) values from the AEROCOM median model and the typical range of nmb values from the individual model plots.

A: The section has been modified and the normalized statistics have been included in the analysis. However, the reference is done with respect to the observations rather than the AeroCom median as suggested by the reviewer.

R: 3.2 Surface mass concentration results – pg23797, line 6 âĂŤ I don't think it is appropriate to compare annual-average model values against cruise observations taken over a few days – suggest that Figure 4 only include the longer-term observations from the ground station data from Mahowald et al (2009) and omit the cruise observations. In fact one can see from the Figure that the long-term observations (diamonds) are not as biased against the model as the cruise observations (filled-in circles). I suspect some of the scatter may be due to the fact that the cruise observations are short-term observations and that dust deposition is so episodic. And remove the sentences that

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try to address this difference between the short-term measurements and the annual average of the model – I don't think an error bar fully addresses the issue.

A: We choose to leave the cruise observations in the study since they give valuable information in regions of the world were 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 figure 4, that models have the same performance on reproducing cruise observations from the Southern Ocean and South Atlantic than on long-term measurements. 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: 3.2 Surface mass concentration results – pg23797, lines 8-11 – suggest to shorten this text as it is a bit convoluted – for instance suggest re-writing these 2 sentences as 1 as "The underestimation is mainly within a factor of 10 of the observations, except in remote regions of the Southern Atlantic Ocean," There are likely other parts of the Results part of the text that could be re-worded to be more succinct in a similar way. For instance also on lines 21-22 suggest to add a comma after (Figure 5) and delete the text "The cases where the underestimations exceed this limit correspond mostly to stations in Antarctica." and instead say "except for Antarctica (stations 1, 8 and 9)."

A: The lines 8-11 were eliminated (see answer above) while the second sentence (lines 21-22 were changed as suggested

R: 3.2 Surface mass concentration results – pg23798, lines 11-15 – suggest to shorten this text by deleting the text "The choice of colours is the same as the one applied in Figs. 2 and 5. In Fig 6 the groups have been labeled Low, Medium and High according to their regime of surface concentration." Instead insert "As is Figures 2 and 5," before

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"We continue to group" and replace "as done above" with "as Low, Medium and High according to their surface concentration regime".

A: Changed as suggested

R: 3.2 Surface mass concentration results – pg23798, lines 19-20 – Again there is the qualitative statement "A large number of models (57%) mainly underestimate. . .." – suggest to make this more quantitative with reference to the normalized mean bias in the AEROCOM median model – and some general information about the range of nmb in the models.

A: The sentence was removed

R: 3.2 Surface mass concentration results – pg23798, line 26 – the text "not enough dust is transported to remote sites" – I don't think this can be seen from Figure 6 which is currently being described – although it is probably clear from Figure 5 and S1 – be specific about where this is clear from the Figures.

A: The sentence was removed and the analysis on the model performance to reproduce the seasonality of the surface concentration was adapted to the new Figures illustrating the bias, the centred pattern root mean square error and the spread in the models through the standard deviation.

R: 3.2 Surface mass concentration results – pg23798, lines 22-26 – justify why you choose to discuss particularly the sites near Australia (rather than downwind of any of the other dust sources). Perhaps something like, "It is interesting to compare the seasonal cycle of dust between the two sites to the East of Australia which are geographically close but in quite different dust regimes."

A: Changed as suggested.

R: 3.2 Surface mass concentration results – pg23799, lines 3-5 – Again the sentence "Most of the models fail to reproduce, to different degrees, the surface concentration in stations belonging to the 2nd group" needs to made more quantitative. Suggest to

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refer to the skill of the AEROCOM median model first (perhaps separate normalized mean bias scores could be calculated for the median model in the 3 groups?). Then, there should be a more quantitative statement about the skill of the models.

A: The paragraph was adapted to fit the new Figure 6.

R: 3.2 Surface mass concentration results – pg23800, lines 1-4 – Suggest to add a sentence at the start of this para something like "To test the simulated seasonal cycle in dust transport across the Atlantic, we compare the models to measurement. . .." (as you have it). Then suggest that the next 2 sentences can be deleted – i.e. those starting "These two stations . . .." And "Large model diversity . . ." Then sentences after that need to made more quantitative with reference to the normalized mean bias of the AEROCOM median model and how the other models perform in relation to that.

A: The first sentence was changed to "To test the simulated seasonal cycle in dust transport across the Atlantic and its northern latitudinal extend, we compare the monthly mean model results to means of daily measurements in Barbados (18) and Miami (19) for the year 2000 (Figure 7).". The sentence starting with "These two stations . . . " was removed while the sentence starting by "Large model diversity. . ." was changed to "At Barbados the model results differ greatly from the measurements over much of the year". As previously stated we decide not use the AeroCom median as reference and have therefore kept the structure of the remaining sentences. Minor changes were introduced in an attempt to make the paragraph more understandable to the reader.

R: 3.2 Surface mass concentration results – pg23800, lines 12-13 – re-word the sentence "The climatology in Fig. 7 illustrate that it is close to the seasonal cycle of the year 2000" It needs to be clearer what you mean here – I presume you mean that the seasonal cycle for the year 2000 is not unusual and follows the average from the 1996-2006 climatology. Since this is the case does one need to include Figures 6 and 7 at all? Do you they add anything new?

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A: The sentence was replaced by “The seasonal cycle for the year 2000 is not unusual and follows the average from the 1996-2006 climatology (Fig. 7)” as suggested by the referee. We decide not to eliminate Figure 7 since it is the only data set with surface concentration measurement coinciding with the simulated period. A description of the model diversity with respect to the data of the year 2000 is given.

R: 3.3.Aerosol Optical Depth results – pg 23801, lines 3-6 – again this sentence needs to be more quantitative – again, perhaps the general performance of the AEROCOM median model should be presented first (it seems to do much better at simulating AOD than either the dust-dep or suface-mass) – can this be seen from the normalized mean bias (nmb) statistics? Then there should be more quantitative statements made about the individual models and the range of normalized mean-bias – maybe it is clear also that the model range of nmb against the AOD observations is narrower or closer to zero than for the dust-dep or surface mass? The authors should comment on this. Also re-word subsequent sentences particularly lines 9-13. Also re-word lines 20-22 to be more succinct – e.g. the observations show that for southern-African, northern-African and Middle-Eastern sites, the dustiest periods are December-April, July-September and May to September respectively. Or something like that. The rest of this section should also be re-worded with more quantitative statements about the mean bias of the AEROCOM-median model and more quantitative statements (involving nmb or normalized-mean-error or something like this) about deficiencies common to several models.

A: Section 3.3 was adapted to the new figures and made more quantitative. However the focus of the text kept and the AeroCom median model was treated as all other models.

R: 3.4 Coarse mode optical depth results – pg23803, lines 28-29 – is there also an alternative explanation for this – perhaps a different fine-mode aerosol type (e.g. biomass aerosol) might be contributing more AOD in the model than the observations? It doesn’t necessarily follow that it is the size distribution of the dust that is at fault here.

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A: In lines 28-29 we did not suggest that the fine mode AOD would be only dust, but that might indeed not have been clear enough. We remove the sentence for clarity.

R: 3.4 Coarse mode optical depth results – pg23804, lines 1-6 – suggest to remove this Figure 15 (or move it to the Supplementary material) as it does not seem to offer any additional information here (the text could just state the result with "not shown".

A: We have removed Figure 15 and the corresponding figures from the Supplement material.

R: 3.5 Angstrom exponent results – pg23804, lines 12-15 – again the general statements should be made with reference to some statistical measure of the bias here (preferably nmb). Suggest again also to begin the description of the results with the AEROCOM median model. Parts of the rest of section 3.5 should be re-worded with reference to the nmb or normalized error where possible.

A: Section 3.5 was adapted to the new set of figures and made more quantitative through the use of the normalized statistics.

R: 3.5 Angstrom exponent results – pg23806 – suggest to remove Figures 18 and 19 as the main reference to them seems to be that they behave similarly to the climatology from 1996-2006.

A: Indeed, the AE from the year 2000 behaves similar to the climatology, but model and observations should agree more for the year 2000 comparison since the models have used the year 2000 meteorology to simulate dust plumes. A discussion is added on the model performance against two benchmark datasets (climatology and year 2000). The station numbers in the figure are now equal for the year 2000 and climatology comparison. We prefer to keep the figures to allow the reader to appreciate the different comparisons.

R: Figure 4 page 23839 – suggest to remove the cruise observations here as they cannot be described as annual-averaged observations. The short-term nature of these

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observations means that they are not comparable with the year-averaged model data. Or else change the caption to explain that they are not annual-averaged.

A: Figure caption was changed to clarify that filled circle correspond to short term measurements taken during cruise.

R: Figure S1 – legend has units ug/m3 but the caption says this is the relative difference – is the legend wrong or have you shown the wrong plot?

A: The units from the legend were removed.

Typos & minor points

R: 2.2 Surface concentration – pg23789, line 14 – replace “mean” with “means”, A: Changed as suggested

R: 3.2 Surface mass concentration results – pg23797, line 25 – replace “mayor” with “major”. A: Changed as suggested

R: 3.2 Surface mass concentration results – pg23798, line 20 – state what is meant by “first group”. A: This sentences was removed and adapted to the new Figures.

R: 3.2 Surface mass concentration results – pg23798, lines 22,24,25,26 – add “New” before “Caledonia” and “Island” after “Norfolk” A: Changed as suggested

R: 3.2 Surface mass concentration results – pg23798, line 26 – add “from Australia” after “pathway”. A: Changed as suggested

R: 3.2 Surface mass concentration results – pg23799, line 17 – “as captured” rather than “with captured”? A: The sentence has been reformulated and is now “The measurements in Barbados (18) and Miami (19) capture the transatlantic transport of Saharan dust.”

R: 3.2 Surface mass concentration results – pg23799, line 25 – what is meant by “year around phenomena” here? Do you just mean that there is some dust all year round?

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As you explain there is a pronounced high-dust season in Asia in the spring. Please re-word sentence to clarify what you mean here. A: The sentence was reformulated and is now “The measurements in Hedo (20) and Cheju (22) present an annual cycle with maximums in spring and minimums in summer, which corresponds to the maximum in dust storm activity in China (Prospero et al., 1989; Prospero, 1996). An additional peak in surface concentration exists at these stations in winter or late fall. The observations suggest that there is substantial dust transport to these costal regions throughout the year; however, some of this dust may be derived from relatively localized sources.”

R: 3.4 Coarse mode optical depth results – pg23803, line 18 – insert comma between “measurements” and “the coarse mode”. A: Changed as suggested

R: 3.4 Coarse mode optical depth results – pg23803, line 21 – delete “with maxima in coarse AOD coninciding with the ones in total AOD” – that is obvious as you have already said it has the same seasonal cycle. A: Changed as suggested

R: 4.3 Emissions – pg23809, line 26 – replace “To illustrate this lets suppose” with “To illustrate this, let’s suppose”. A: Changed as suggested

R: 4.3 Emissions – pg23809, line 28 – insert comma between “underestimation” and “a larger fraction of fine”. A: Changed as suggested

R: 4.3 Emissions – pg23810, line 5 – insert comma between “In the same way” and “the opposite case”. A: The sentence has been changed and is now “Likewise, the simultaneous underestimation of the AOD and overestimation of the AE, points to an underestimation of the coarse dust emissions.”

R: 6 Conclusions – pg23815, lines 20-22 – insert comma between “conclude” and “with very large uncertainty”. A: The conclusions have changed due to the corrections introduced in the processing of the simulated deposition flux. The corresponding sentence is now “The limited number of deposition data in HNLC regions and the dependence of the models performance in simulating these data to the location of the data prevent

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us from concluding on the atmospheric iron contributions in HNLC regions from global dust models."

R: 6 Conclusions – pg23816, line 12 – replace "regional model performance" with "regional performance of the models" so as to be clear you are not referring to regional models. A: Changed as suggested

R: Table 1 – pg 23831 – no mmr values are given for the 2 modes in the TM5 scheme. A: Missing values have been given

R: Table 3 – pg 23833 – is there a typo here for the ECMWF emissions flux – it says 514 Tg/yr but the deposition fluxes suggest it should be about 6000 Tg/yr. Similarly the budgets don't add up for the CAM model – is this also a typo (or does the NaN sedimentation value account for this? A: Indeed there was an error on the budget of the ECMWF model and the deposition fluxes were largely overestimate. Likewise, for the CAM budget a removal flux was missing. The errors have been corrected and we thank the referee for pointing this out.

R: Table 4 – pg 23834 – the different models do not have equivalent dust size ranges – could the models be grouped into models with approximately the same size range so that one can at least compare the fluxes and lifetimes for the models that are similar. A: Tables 3 and 4 have been rearranged as suggested by the referee and the caption has been adapted.

R: Table S2 – caption – replace "Marin" with "Marine". A: Changed as suggested.

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

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Interactive Comment