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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Development of a global model of mineral dust aerosol microphysics" *by* Y. H. Lee et al.

Y. H. Lee et al.

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Summary

This paper presents a description and evaluation of a recently developed bin-resolved dust aerosol module for the GISS-TOMAS global aerosol microphysics model. The paper describes the new emissions module and outlines existing modeling approaches to removal processes. The paper then presents the simulated dust burden and evaluates modelled surface mass concentrations and deposition fluxes against observations from the University of Miami network of surface sites and from Ginoux et al (2001). Comparison against a single size distribution from the NAMMA campaign (presumably representing an average over the campaign) near the African source region is then used to assess the simulated size-resolved dust in the model. Finally, the impact of dust on global CCN concentrations is assessed and a small decrease is found down-wind from Africa when dust is included. This is attributed to reduced growth due to coagulation scavenging of particles and condensation of sulphuric acid onto the dust particles. The impact of dust on global CCN is of interest to the community, but the analysis of the impact is somewhat rudimentary, and given the size-resolved nature of the model and its treatment of each of the processes, the lack of information on the contributing processes is rather disappointing (see specific comments). Similarly, the use of a size-resolved model requires a wider range of observations to constrain model performance than is used here. Or at the very least some evaluation against aerosol optical depth data. I consider the use of only a single size distribution to be insufficient. Throughout the model evaluation, no representation of variability about the mean is considered. Although only a single climatological year is simulated in the model, the variability in the University of Miami observations should at least be included in Figures 4 and 5. The spatial variability in the simulated size distribution should be included in Figure 9 and also the variability in the observations if available. Where discrepancies are found between model and observation, attempts to evaluate the cause are rather cursory. Sensitivity simulations to processes considered potentially deficient should be carried out. A sensitivity to a different threshold velocity is attempted, but there are several other potential causes of the discrepancies (see for instance the box model sensitivity simulations in Grini Zender, 2004). The paper is fairly well written but several references are missing and I consider that this paper requires considerable major additional work to extend the model evaluation and assess the process contributions to CCN reductions before it be published in ACP.

Response: A greater analysis of the CCN impact is now included in the manuscript as are a greater range of observations. See responses to previous reviewer related to these points.

Specific Comments

1) Section 2.1: Several references here are missing from the reference list: Adams and Seinfeld, 2002; Pierce et al 2007; Benkovitz et al, 1996; Clarke et al, 2006; Bond et al,

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2004; Koch et al, 1999; Seinfeld and Pandis, 1998; Tegen et al, 2002.

Response: The missing references have been added.

2) Section 2.2: The empirical formulation of Ginoux et al (2004) is used to parameterize the dust emissions flux with predicted mass emitted in a bimodal size distribution representing clay and silt sized particles. A soil fraction is used to calculate the mass emitted in each bin. However, it is unclear from the manuscript how this fraction is derived. It is stated that an assumption is made that 75% of the mass emissions flux is in the 2 to 10 micron size range and that 10% is in the clay size range. Are these assumed proportions globally constant? However, as alluded to by the name of the size ranges, some soil have higher clay contents than others. Is this included in the model? If not, what is the potential impact of neglecting the potentially enhanced emissions of smaller particles from clayey soils – could this help to explain the discrepancies in the observed and simulated size distributions?

Response: Our aerosol microphysics model (TOMAS) use dry aerosol mass to define size section. Using two log-normal distributions derived from dAlmeida and Schutz (1983), to obtain the soil mass at each size bin, the distributions is integrated in between size sections. We add the following explanation in section 2.2.1 in the revised paper. "The soil fraction for each TOMAS size bin is obtained by integration of these log-normal distributions between the corresponding size bin boundaries."
Size-resolved soil mass fraction is assumed to be globally constant in the model. To be clear, our soil fraction is based on uplifted emission flux, and, therefore, it is not explicitly comparable to soil (aggregate) size distribution. For the soil aggregate size distribution, although some soils certainly have higher clay contents than others, to our knowledge there is no global data set available with the relevant data on soil size distributions as a function of region/location. To our knowledge, no global dust model has attempted to deal with this difficult issue. The assumption of globally constant soil mass fraction introduces errors in the model predictions, but at present it appears impossible to quantify them without having a well constrained dataset. Section 3.4, we add the

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following discussion. ”The discrepancy could result from the global constant soil mass fraction in the model. According to Grini and Zender (2004), physically based models tend to predict better dust source size distribution compared to observations. Using soil mass fraction, which neglects the dependence of vertical flux on friction velocity, in the dust emission scheme might result in the discrepancy in size distributions between our model and observation.”

3) Section 2.2: As mentioned above, the model simulates uplift using the empirical approach of Ginoux et al (2004). The paper should include some discussion of alternative approaches; for instance wind tunnel experiments have illustrated that smaller particles are only emitted during stronger saltation (e.g. Alfaro Gomez, 2001). What impact could resolving this size-resolved uplift have on the simulated size distributions? The authors should at least state that this is neglected in the model and give reasons for this.

Response: The following has been added to the introduction of the revised manuscript. ”A more physically based model has been proposed by Alfaro and Gomez (2001) that accounts for the sand-blasting process with a parameterization of the size-dependent vertical flux as a function of the horizontal flux (saltation), the soil aggregates size distribution, and wind friction velocity. A difficulty in this approach is that the dust emission vertical flux is a function of the soil aggregates size distribution. Because there is no global database of soil aggregates size distribution, it is generally assumed to be uniform everywhere.
In this work, soil mass fraction is also assumed to be globally constant and is not a function of wind friction velocity.”

4) Section 2.2: The GCM mean gridcell wind speed is used to determine the dust uplift and 2 potential issues are mentioned by the authors here: sub-grid-scale variability and bias of the modelled wind speed. The latter is addressed by using an emission ratio derived from comparison between the model and NCEP analysed wind fields. However, the former issue is not mentioned again after referring to it as a potential problem.

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The authors should state more clearly the potential impact of neglecting subgrid- scale variability here. Additionally, it is not clear at what frequency this emission factor is being applied to the model. If it is being applied at each time step, is the emissions factor really determined from the NCEP value at that timestep, or is it rather an interpolation between two analysis fields to the timestep? Are 6-hourly or daily NCEP fields used?

Response: We did not investigate the impact of neglecting sub-grid scale of wind speed. Instead, we left a reference of Cakmur et al. (2004) that shows increase of dust emission with sub-grid variability. This is a straightforward result of the (approximately) 3 power dependence of emissions on wind speed. In principle, neglecting sub-grid variability should lead to an underestimate of emissions. However, in practice, other factors in the dust emissions calculations also represent grid-cell averages and may or may not compensate for the underestimate. Therefore, it is difficult to say a priori whether a parameterization will overpredict or underpredict systematically. In the absence of observations of grid-cell average emissions fluxes, there is no way to evaluate the dust emissions parameterization per se. Therefore, we prefer to continue to evaluate the overall model against dust mass concentrations and deposition fluxes. The emission ratio is calculated using Eq. (3) based on 6-hourly NCEP fields. It is used to understand the bias in our predicted dust emissions due to biases in our model wind speed. The emission ratio is not used as a correction factor to rescale the dust emissions in the model simulation. As an example, we cite Figure 1 that shows a low bias in the model predicted dust from Africa during the NH summer and quote from the associated text. "Because dust emissions are proportional to wind speed cubed (neglecting any threshold speed), this definition of the emission ratio gives the factor by which emissions are biased high or low due to biases in the GISS wind fields, assuming the NCEP winds are correct. This definition gives a time-averaged wind speed that appropriately weights the high wind speed events that largely determine total emissions; our experience has shown that comparisons based on the usual time-average wind speed (without any exponent) are often misleading in this regard. The functional form of Eq. (3) is approximate because it neglects the threshold velocity, which depends on the

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time history of soil moisture. Figure 1 shows the dust emission ratios thus calculated. The most important features are an underestimation of dust emission in the Sahara during summer and in Taklimakan throughout the year. Cakmur et al. (2004) and Miller et al. (2006), dust models that run in the GISS GCM ModelE, report significant underestimations of dust in Sahara especially during summer and in Taklimakan throughout the year as well. Although they used a different version of the GISS GCM, similar wind speed biases in their version may cause biased dust emissions over these locations."

5) Section 3: The GCM is free-running (I assume) and presumably the results are from a multi-annual run? If so, model inter-annual variability should be included in each of the model evaluations. Also the variability in the multi-annual University of Miami observations should be included.

Response: Based on the dataset we obtained from Univ. of Miami, we were not able to estimate the inter-annual variability of the observations. We can only get a standard error of each month measurement, which indicates the variability of one time measurement in the month. Our model result is based on one year simulation. We add the following sentence in Section 3. "The model results presented here are based on a one-year simulation with an initial three months discarded for model spinup purposes."

6) Section 3.4: When the simulated size distribution is compared to the observed size distribution a monthly-mean simulated size distribution is compared to a campaign specific average of strong wind cases. To address this, the model mass size distribution is scaled so that the mass concentration matches that in the observations. The authors should comment on the impact this scaling has on the comparison of the size distributions. Since the threshold friction velocities are size-dependent in the model, sampling stronger wind events only is likely to result in a different size distribution than a mean over a month. The authors should include some representation of variability (ideally spatial and temporal) in the comparison (e.g. using 15th/85th percentiles from the observed and simulated size distributions) if possible. This would give valuable additional information to assess the performance of the model.

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Response: First, we do not have data to get spatial or temporal variability of the size distribution comparison. The campaign data is not necessarily strong wind cases. The reason for scaling the concentration is that the observation is taken at or near the dust plume, while the model grid size is much bigger and includes regions with lower dust concentrations. Therefore, the following sentence is corrected in the revised manuscript. ”The campaign targeted the strong dust events" to "The campaign targeted the SAL (Saharan dust layer)”.
The reviewer 's point that a size-dependent threshold velocity formulation will yield a size distribution that depends on wind speed is correct and well taken. To see the impact of wind speed on size-resolved emissions flux in our emission parameterization, we calculate these at four wind speeds offline. These results (unfortunately, we could not include figures because the ACP system does not allow figures to be posted in a comment) indicate that the predicted size-resolved emissions flux follows a similar size distribution over a wide range of wind speeds. This occurs because the wind speeds are generally much higher than the threshold wind velocities. Generally the size distribution is more dependent on the soil mass fraction. However as the wind speed goes down, the size distribution starts to be affected by size-resolved threshold friction velocity. Since these results are not very interesting, we choose not to add them to the revised manuscript.

7) Section 3.4: The GCM used is free-running and uses climatological meteorology. The authors should comment on the impact of differences between this average meteorology and the observations from the single field campaign. Also the time of year of the NAMMA observations is not stated and which monthly-mean is used from the model?

Response: As we stated in the paper, we tried to avoid single-year observations due to GCM climatological-meteorology. However there are few multi-year measurements of dust mass size distribution. Therefore it is necessary to compare the GCM derived dust to single field campaign measurement. We included the NAMMA campaign period as

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well as the period of model dust mass concentration in the revised manuscript as shown in below. ”The observed distribution in the NAMMA campaign during the summer in 2006 is generated from data collected between 20 and 30 degrees W longitude and 10 and 20 degrees N latitude” and ”Model dust mass concentration is sampled during JJA for the NAMMA”

8) Section 3.6: This section is the most interesting to me in the paper but again the discussion of the results and the attribution of the reduction in CCN concentrations as a result of interaction with dust is not explored. Examination of the model simulated coagulation scavenging and condensation fluxes would help to explain what is occurring here. Or alternatively sensitivity simulations where these processes are switched off should be carried out to determine which of the suggested 2 processes dominates here. Also, does the enhanced H2SO4 condensation onto dust affect new particle formation?

Response: We agree that our CCN part had the lack of information we provided. In the revised paper, we included more analysis on aerosol budgets that explains why the CCN concentration is changed by dust aerosol. And we also included the impact of dust on CCN concentration at various supersaturation ranges (see Figure 11). Section 3.6 is substantially enhanced from what was originally submitted.

About the following comment: Sensitivity simulations to processes considered potentially deficient should be carried out. A sensitivity to a different threshold velocity is attempted, but there are several other potential causes of the discrepancies (see for instance the box model sensitivity simulations in Grini Zender, 2004).

Response: We agree that there are several other potential causes of the discrepancies. However the dry threshold velocity is the easiest parameter we can change. It allows our model to emit more fine mode and to test our hypothesis that a short dust lifetime biases our model in regions with low dust. A systematic evaluation of all potential discrepancies would be lengthy and well beyond the scope of this work (witness

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the numerous dust papers that have been published and still it is a difficult aerosol to simulate). Our focus in this paper was to document the development of the GISS-TOMAS dust module, evaluate it against dust–specific observations, and investigate the impacts of dust on the CCN cycle.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18765, 2008.