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Interactive
Comment

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

Anonymous Referee #1

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Referee Comment on "Development of a global model of mineral dust aerosol microphysics" by Y. H. Lee, K. Chen and P. J. Adams.

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 (pre-

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sumably 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 downwind 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 it's 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.

Specific Comments ————————

1) Section 2.1: Several references here are missing from the reference list: Adams & Seinfeld, 2002; Pierce et al 2007; Benkovitz et al, 1996; Clarke et al, 2006; Bond et al, 2004; Koch et al, 1999; Seinfeld and Pandis, 1998; Tegen et al, 2002.

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?

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.

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. The authors should state more clearly the potential impact of neglecting sub-grid-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?

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.

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 spe-cific 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 distribu-tions. 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 ob-served and simulated size distributions) if possible. This would give valuable additional information to assess the performance of the model.

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 mete-orology 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?

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?

References ———-

Alfaro & Gomez, "Modelling mineral aerosol production by wind erosion: Emission intensities and aerosol size distributions in source areas." J. Geophys. Res., 2001

Grini & Zender, "Roles of saltation, sandblasting and wind speed variability onmineral dust aerosol size distribution during PRIDE", J. Geophys. Res. 2004.

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