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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Implementation of dust emission and chemistry into the Community Multiscale Air Quality modeling system and initial application to an Asian dust storm episode" *by* K. Wang et al.

Anonymous Referee #3

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The primary objective of this study is to include a detailed treatment of natural dust emissions in the CMAQ modeling system. The authors have attempted to enhance the representation of three specific processes as they pertain to airborne dust in the CMAQ modeling system: (1) inclusion of algorithms to estimate the emissions of wind-blown dust emissions (two approaches are discussed), (2) inclusion of representation of heterogeneous reactions on dust particle surface, and (3) updating the treatment of aerosol equilibrium thermodynamics in the model through incorporation of an updated version of the ISORROPIA module. Overall the study is of interest to the wide

modeling community and represents enhancements in an aspect of PM source and composition traditionally missing in CMAQ model. In its current form, there are aspects of the manuscript which could benefit from additional work, especially in terms of (a) the description of the implementation details of the dust emission module and (b) streamlining the presentation of results so that the salient features in terms of impacts on model predictions and process characterization are readily apparent to the reader. The following suggestions are offered:

1. While two dust emission flux schemes (based on Westphal et al and Zender et al.) are included in the model, it is not readily apparent what the impacts are (if any) on inferences drawn from model predictions. Does one scheme systematically over/under estimate relative to the other? What are the relative impacts on model predictions of fine and coarse PM and how do they differ with the two schemes? If a user was to choose, which scheme should they use?

2. The description of the data sets used in estimating the emissions refer to the BELD3 and the STATSGO data sets. What is the geographic coverage of these data sets – is it just North America? If so how was the relevant information derived for other parts of the modeled domain?

3. What land use information is needed by the two the two dust emission flux schemes and how are these derived? Are they based on the land use scheme used in the WRF simulations – if so which one?

4. The fraction of erodible land is determined to be an important factor in regulating the source strength of the estimated dust emissions. While the authors term it an adjustable parameter, it is not apparent how one should go about estimating its value (between 0 and 1)? Does it vary by application? If it represents the fraction of erodible land capable of emitting dust, what is the dependency on the horizontal grid resolution employed by the model? Some additional clarification of these aspects is needed.

5. The authors present comparison of a number of modeled variables with measure-

ments from a variety of model runs and have clearly put in a lot of effort. However, the presentation of the results is somewhat disjointed and in some instances difficult to follow:

a. The inclusion of the MM5 simulation results in Table 4 appears to be more of an afterthought than something central to this study – which CMAQ simulation was driven by these data?

b. What is the point of including results from the old CMAQv4.4 simulations? A number of aspects of the model appear to have changed relative to v4.7 in which the dust emissions are examined. The MM5 and CMAQv4.4 results are distracting and do not add anything to the primary focus of this study – their discussion only adds unnecessary text and length. I suggest deleting these aspects of the analysis.

c. What impact did the inclusion of K, Ca, and Mg (via ISORROPIA II) have on the partitioning of oxidized and reduced nitrogen between the gas and the aerosol phase? The CRUST_ONLY simulation appears to have been designed to examine this issue – however the discussion of results does not clearly demonstrate the effects.

d. Section 4.2.2 presents a lot of information on model performance statistics but little interpretation from a model process standpoint – there is useful information in this section as it pertains to the dust model and its impacts on model predictions but is obscured in the current presentation of the results. The authors should consider restructuring this section by presenting only the statistical comparisons for the relevant species and simulations. Given the model process enhancements included in the simulations, did the authors realistically expect to see differences in predicted CO and TOR between the various simulations? Would global statistics such as NMB capture any such possible changes?

e. Page 13480, line 15: what does gas phase NO3- represent? Do the authors imply nitrate radical?

f. Page 13481, lines 10-15: it is difficult to follow this discussion without seeing the figure (not included) that the authors are referring to. Line 18: “and replaces NO3- as ions. . .” is awkward – the discussion needs to be reworded.

g. Page 13484: lines 5-10: Are the reported increases in O3 and CO of 3.6% and 2.1%, respectively in “background” levels or total simulated levels – the authors need to be carful in the terminology.

h. Page 13484: lines 11-15: what results in the “negative contribution” of Asian NOx emissions to NOx levels in the US? From a process stand point how can this be attributed to a possible negligible import? Is the suggestion that this is a numerical effect?

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